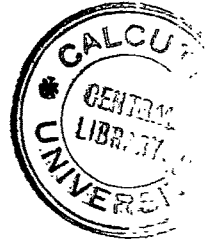


WK-H 738-48-P35411

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Volume LXIII

BIMONTHLY

JULY 1957—MAY 1958



UNIVERSITY OF CHICAGO PRESS
CHICAGO, ILLINOIS

CAMBRIDGE UNIVERSITY PRESS, LONDON

P 35,411

COPYRIGHT 1958 BY THE UNIVERSITY OF CHICAGO
PUBLISHED JULY, SEPTEMBER, NOVEMBER, 1957
JANUARY, MARCH, MAY, 1958

COMPOSED AND PRINTED BY THE UNIVERSITY OF CHICAGO
PRESS, CHICAGO, ILLINOIS, U.S.A.

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THE AMERICAN JOURNAL OF SOCIOLOGY

Volume LXIII

JULY 1957

Number 1

HISTORY, SOCIOLOGY, AND CONTEMPORARY AREA STUDIES¹

BEN HALPERN

ABSTRACT

Contemporary life requires both historic and sociological analysis, but neither discipline is fully adapted for the task. History legitimately demands historic perspective, and its latent generalizations are selected by uncontrolled intuition. Sociology is rigorous in generalizing, but its assumption of the integral group as the limit and norm of systematic change restricts and misleads historic analysis. In order to retain methodological rigor and still apply to historic process, insofar as it lends itself to systematic study, sociology should make the interaction system of culture rather than the social system the framework of analysis.

It seems only yesterday that sociology made the fruitful decision to "climb down from the big words of social science, at least as far as common-sense observation";² to beat a strategic retreat from "high-order abstractions" to "first-order abstractions," from "upper range" generalizations to "middle range" generalizations, and from history to the study of the integral group and more particularly the small group. The fruits of that decision have been theoretical and empirical contributions of major significance, and the promise of the microsociological approach is far from exhausted. Yet voices are already heard questioning the necessity and wisdom of abandoning the attack on the larger problems of historic anal-

ysis to which classical sociology devoted so much attention.

In a recent article Barrington Moore says:

It may be argued that a major function of sociologists, though by no means the sole one, is the analysis of the grosser structural features of the world in which they live. This would include assessments of possible and probable future developments in particular national states and the likely consequences of various policy alternatives. In turn, concern with these problems involves the analysis of movements and circumstances that will be in many respects unique.³

Moore concludes that problems of contemporary politics call for an attempt to grasp both the unique and the general—that is, for both a historical and a sociological approach; that we cannot hope for, nor should we demand, strictly deterministic prediction of events, but we should aim at defining possible alternatives of policy and determining their costs; and, finally, that sociology, in

¹ I owe thanks to several colleagues who have helped me with their comments on an earlier draft of this article, in particular to Professor Talcott Parsons. Needless to say, he shares no responsibility for what is largely a critique of the implications of some of his views, but for that very reason I have found his remarks invaluable in rephrasing certain statements in a way which I hope may avoid misunderstanding.

² George C. Homans, *The Human Group* (New York: Harcourt, Brace & Co., 1950), p. 13.

³ "Sociological Theory and Contemporary Politics," *American Journal of Sociology*, LXI, No. 2 (September, 1955), 110.

order to fit itself for these new tasks, should give up its equilibrium theory and revert to the old-fashioned type of process theory.

The present paper shares many of the assumptions and, in a general way, the conclusions of Moore. In pursuing the subject along a rather different track, we should like to begin by considering a point made by implication only by Moore: that not only sociologists but also historians fail to give the guidance needed in contemporary studies; they, too, avoid the analysis of the grosser structural features of their own world.

The fact is that both history and sociology, each in its own way, rebel against being used in this kind of undertaking. Moreover, to employ history and sociology in combination invites another difficulty, for these two disciplines seem in some ways opposed in their aims and assumptions.

Roughly speaking, the sociologist is prepared to explain anything that may happen among men—preferably something relatively trivial—in terms of the social system, while the first task of the historian is to weed out such social phenomena as are not worth his while explaining. For this reason the sociologist is willing or even prefers to deal with contemporary events, although he wishes to confine himself to directly observable actions. The historian, however, generally assumes he must wait for the erosion of time to select among all events that occur those that have the particular importance that makes them historic. When confronted with social happenings in the raw, so to speak—that is, with the immediate presence of everything that occurs within a contemporary field of experience—the historian generally pleads professional incapacity, for he lacks the historic perspective without which he cannot begin to work.

In what ways does time produce historic perspective? We may consider first a rather trifling but nonetheless frequently crucial effect of time upon historiography. Time selects the data which remain available to the historian. He need not deal with all that happened at some time in the past but with

only that part of it which left a permanent impression in official archives, in the literature of lasting value preserved in libraries, or in archeological remains. The historian may, and often seems to, hope that this selection of data by time has taken place according to the very same criteria of significance which make events themselves historic. But, on the other hand, it may be nothing more than chance that determines what is left behind in archives or literature, let alone in archeological sites. For a reasonable explanation of what happened the historian may have to reconstruct facts and imagine the purposes of acts by inference from these fortuitously available data.

Thus, it remains rather dubious whether time alone, merely by destroying part of the data, really gives us historic perspective. It does, however, render the problem of historic perspective less acute, for, where data are scarce, any trifle is likely to give illumination. In the face of the superabundance of data, the "welter of the actual" that constitutes the historian's own contemporary world, the situation is quite different.⁴ No study of contemporary affairs could be made by anyone with a historic sense without facing, at once and squarely, the need for distinguishing between those data that are historic—that will probably be remembered and exert an influence over a span of generations and that will create new situations, rendering obsolete certain elements of the old situation and replacing them—and those data that are non-historic, or merely sociological. We conclude then that time, at least in its selection of data, does not really give historic perspective to the historian—it only obscures the need for it—while the first effect of confronting the overwhelming mass of contemporary data is precisely to under-

⁴ In view of the masses of materials on modern and contemporary history being piled up in libraries and archives, at a time when to read a single issue of the *New York Times* is no minor undertaking, this very situation may confront the future historian of our times. This could make the problem of selecting significant materials, in the light of a clear distinction between historic and non-historic events, an important problem of practical historiography.

score the need for a valid theory of the difference between historic and non-historic events.

Another reason for the historian's assumption that time alone gives historic perspective is the element of bias. One of the crucial problems of historical explanation has always been to infer the real reasons from the known and stated reasons of historical actions. What contemporary observers and tradition may have recorded concerning historic events may be inaccurate because of the deliberate or subconscious intent of the authors of our records to present the past as they would have liked it to have been rather than as it actually was. History's task is, then, to correct the records by inference of bias gained from internal evidence or from a check of one set of evidence against another.

Time presumably facilitates this task by reducing the bias of the historian himself in the face of the data and by giving him the historic perspective of a position far removed from the events. But everyone knows that historians have by no means always been free of prejudice; it may even be true that they are capable of greater personal involvement in things dead and gone than are ordinary men. Nor is the elimination of bias part of the practice of history alone. All social scientists, and not least among them those concerned with contemporary affairs, are aware of the need to eliminate bias as one of the fundamental methodological imperatives of their trade.

Furthermore, here, too, we note a more general and systematically significant question hidden in the principle of eliminating bias by historical perspective. Most modern historians make some use of theories, most elaborately presented in the classical sociologists and social theorists, according to which deliberate distortion is only a superficial reason for the bias with which historical records are affected. These theories contend that all conscious purposes of action are no more than illusory explanations of events and that behind the superficial play of stated purposes history is moved by mas-

sive forces, geographic, ideational, or social-economic, which really determine both the events and their conscious interpretation by contemporaries. It is the task of the historian, then, on the basis of his theory of the underlying nature of history, to interpret both events and the rationalizations of the actors in them in the light of the deeper dynamics of history.

Much of the uncertainty and changeableness of historiography results from the conjectural nature of its tasks, as we have just defined them. To try to restore that part of the true historical past which has been left out of the record by chance, to correct the distortions introduced into the record by deliberate bias, to decipher the underlying significance of epiphenomenal events and rationalizations—all this is an imaginative effort which can prove conclusive only if there is conclusive evidence of the assumptions upon which it is based: that there is a real process whereby time distinguishes historic from non-historic events, that there is an objective approach to events such that it can record or reconstruct this reality without bias, and that it is possible to distinguish conclusively between processes which are the substance of history and others which are no more than the effects of the underlying reality.

But there is a third way in which history is able to go behind its evidence and check the contemporaneous explanation of events against its own better qualified knowledge. It is in this third way that we find a clear definition and a firm basis for the historical perspective that time, and time alone, gives the historian.

The historian has the great advantage over the contemporary observer of knowing what actually resulted from efforts to shape history. Accordingly, he is in a position to judge the relevance and historic significance of motives which contemporaries assigned for the events of their own times. If the upshot of their acts was significantly different from their stated purposes, the historian, from his superior perspective in time, can construct a more adequate chain of purposes

and consequences to explain what he knows actually happened. On the other hand, if the chain of unforeseen and unintended consequences has not had full play, the historian does not yet know what truly historic change has occurred. He has not yet ascertained the historic fact and does not know to what ultimate event he must attach a reasonable chain of purposes. It is for this reason, above all, that the historian rebels against the task of interpreting contemporary events.

To be sure, the historic perspective gained by knowing the ultimate event is not an altogether unequivocal advantage, nor does it really provide a stable and secure foundation for a truly objective discernment of what happens in history. For the real ultimate events which anchor the chain of historical explanation are, at any rate, for any historian of whole societies,⁵ the events of his own time. While he disqualifies himself from determining what in his contemporary reality will remain historic, he is bound to determine what is historic in the concluded past (however far back he may wish to conclude it) by a chain of reasonable connections ending only in the observed character of his own times. Because of this, history is a notoriously labile discipline, for the significance of the past must necessarily alter subtly or sharply as new times bring new events to which whatever is past must sustain some reasonable connection. Thus it is that, in the philosophy of scientific method, the term "historism" connotes a relativistic approach to reality.

The professional intuition which enables the historian—and, above all, the modern historian—to write comprehensive rather than narrowly special studies, by guiding

him through the welter of redundant and trivial materials, implies the making of two sorts of judgments. There is first the judgment that all historians of whole societies are likely to make, no matter what the century of their interest: the more or less unconscious determination of the character of their own time and the nature of the more fundamental problems it faces and consequently of the alternative directions between which it must choose—problems and alternatives, moreover, which are not always considered unique but quite as often thought to be perennial and, in other forms, to be basic also to the history of other times.

There is also, however, the judgment about general forms and relationships which recur in all particular historical series of events and which enable the historian to determine in advance what kinds of data will be most significant.

Sometimes these judgments are expressed in fully formed sociological or philosophical or theoretical conceptions like those of the Marxian historians or of a Spengler or Toynbee. But even where this is not the case anyone who essays a comprehensive history is bound to have a predilection for certain types of general connections among events which, in his judgment, are likely to prove most illuminating. On the basis of this predilection he will determine what materials should be sought and consulted. Such a suppressed theoretical judgment is also his guide in discriminating the "true" underlying chain of events and purposes from the "illusory" or superficial.

Theories of precisely this sort were the characteristic achievement of classical sociology, for it was one of the primary goals of the sociological movement in social philosophy to establish a systematic, generalizing, scientific history that should remove from it, once and for all, all speculation and all difference of opinion. But, while this was the purpose of classical sociology, it had quite the opposite effect. The sociologists themselves multiplied contending schools of opinion, each asserting a different causal relationship as the true, substantial process of

⁵ One could, perhaps, argue that some kinds of special histories, and in particular histories of literature, art, etc., where the ultimate event is an unequivocal material fact—a manuscript or art object—are exceptions to this statement, but it is well known how these special histories, too, are subject to changing interpretations when scholars try to relate the material of their special field to its total historic and particularly social context.

history—the climatic, geographical, economic, and other materialistic schools of historiography, as against the many schools that regarded various dialectical or cyclic conversions of the spirit as the underlying historic reality.

This result having been reached, historians and sociologists took different courses arising from the same disillusionment. The former tended to regard the whole matter as one for intuitive rather than objectively demonstrable decision and allowed unexamined predilections to determine what generalized connections between events each historian chose as a tool in isolating the truly historic from “the welter of the actual.” The sociologist, on the other hand, holding true to his professional aim of achieving a universally valid, objective science of society, decided that, for the time being, at least, history was not the appropriate field in which to construct a general theory but retreated to a more favorable ground, the study of social interaction.

The complaints of modern against classical sociology were twofold: first, that it set up untenable, because mutually contradictory, theories of unilateral cause and effect in history and, second, that this led to an eclectic proliferation of unilateral causal analyses that was capable of being multiplied indefinitely without ever guaranteeing logically adequate and comprehensive explanation—the so-called hodgepodge method of heaping up “factors” in the interpretation of society and history. In view of these defects, some sociologists concluded that, to reform sociology and rebuild it on a sounder basis, it was necessary to abstract from historic process and construct a general theory based on a cross-sectional analysis of behavior in groups. The systematic core of such theories has been the analysis of the mechanisms of group integration, the concrete data most easily available and readily verifiable for which have come from the small group.

These objections to classical sociology are reflected in the methods characteristic of modern sociology. In opposition to the sev-

eral mutually contradictory, unilateral relationships of cause and effect, sociology has adopted the concepts of function and system. A “functional” as opposed to a “causal” relationship between variables—for example, the relation between economic and religious “factors”—can be construed in either direction. Fruitless controversies such as bedeviled nineteenth-century historiography are thus to be avoided methodologically. Nor is this a mere verbal avoidance which would allow us to postulate any factor at will to explain social phenomena so long as it was a factor about which we could collect statistical data for correlations. Many current sociological and anthropological theories not only speak of “functional” relationships in a mathematical sense but, applying a biological analogy, refer to “functions within a system.” The idea of a “system” within which functional relationships obtain and which these relationships tend to perpetuate sets a limit upon the number and kind of factors considered significant in analyzing social phenomena. In this way, social scientists try to overcome the hodgepodge analysis of social process in terms of factors chosen *ad libitum*.

But, in setting up a structure of functions within a system, the aim is not only to establish criteria of relevancy for the factors applied in particular social analyses. Contemporary theory often proposes to provide a limited, generally applicable set of relationships within which everything that happens in society *must* fall. In order to be sure that everything that can possibly happen in society will indeed fall within them, a theory must give further assurance that its system of functions is based on a logically exhaustive classification of all possible factors. Thus, a highly regarded model of contemporary sociological theory, Parsons’ general theory of action, begins with what is presented as a logically exhaustive classification of the determining factors in human action and throughout its further development strives to demonstrate a rigid, deductive relationship to the original classes.

If history is to free itself from the ar-

bitrariness of the intuitive approach—as far as it inherently can—it should certainly take advantage of these achievements of sociological methodology. There is nothing in the ideas of functional rather than unilateral causal relationship, of logically exhaustive classification of functions based on a theory of human action rather than their unlimited, hodgepodge multiplication, and, finally, of the defined systematic interconnection of the classes of factors rather than their arbitrary election, which is not as fully applicable to history as to sociology. It is only when we come to a third characteristic of some contemporary sociologies, most radically exemplified in the retreat to the analysis of the small group but also inherent in the ruling assumption of the integral group, that we find that sociology has impaired its usefulness for history. This characteristic is the assumption that the fundamental correlations in action (or “interaction”) form social systems that tend to maintain themselves within boundaries coterminous with groups.⁶

There are two reasons why the sociology of the integral group seems in principle precluded from yielding an adequate theory for systematic historiography. In the first place, it does not deal with the questions prerequisite for any systematic historiography. On the other hand, its implied promise to master the problem of history at some future stage of development, through an extension of its theories of the integral group or the small group, seems to contemplate determining certain crucial aspects of history which cannot, in principle, be determined.

It is obvious that the phenomena isolated for study by the concepts of sociology ordinarily include more than what is historic. Sociology, as we have already remarked, attempts to explain anything that may occur in a social context, no matter how trivial, in terms of a social interaction system. But if

we roughly define the “historic” as whatever proves memorable beyond the span of the single generation or exerts an influence over a span of generations, and add perhaps that “historic” also implies any social phenomenon which, arising in a given generation, renders obsolete for future generations other phenomena which it tends to replace—then we have to deal with a range of events which is far more narrowly and specifically qualified than the sort of thing for which the bare idea of a social interaction system undertakes to provide a tenable theory.

What is more, the range of phenomena isolated for study by some theories, which seems in one way to exceed the specifically historic, in another way appears to exclude the specifically historic. The history of whole societies implies as the characteristic subject of its inquiries such purposes as can be predicated of whole groups: for example, imperial rule, social welfare, or national liberation. The variance and dynamic interplay of such purposes chiefly constitutes the substance of history. We have seen that a grave defect of historiography, from the point of view of social science, is that it is not based on a systematic interrelationship of all possible functions it may employ in explanation. Precisely this defect has been overcome by the theories of the integral and the small group; but this was accomplished only by abstraction from the concrete, historic purposes groups devote themselves to. Instead, the sociology of the integral or the small group focuses upon a universal “purpose” of the group qua group: its own “integration.”⁷

⁷ (a) “Integration”: This highly ambiguous term, which cannot be further analyzed here, is used, among other uses, to cover a range of phenomena varying from the achievement of a relationship of sharing from which strain is totally absent to the mere continuance of a relationship of sharing. For the latter use cf. R. F. Bales, *Interaction Process Analysis* (Cambridge: Addison, Wesley, 1951), pp. 10–11, 53, 60 ff. When we speak of “integration” as a constant factor in social system equations, it is this latter sense we have in mind. (b) “Purpose”: We refer above to integration as a “purpose” (in quotes) because even though—as the following paragraph makes clear—it is not a

⁶ Such ideas are pervasive in contemporary sociology. I particularly refer here to two relatively specific formulations: those of Talcott Parsons and George Homans.

This, it need hardly be said, is not a purpose in any empirical sense, for the sociologists do not assume that "integration" is ordinarily an aim consciously held by the group or that group efforts are deliberately directed toward achieving it. Rather is it a postulate formulated by the theory regarding any group that exists as a "boundary-maintaining system." This means, of course, that integration—which should really be regarded as a constant factor in any functional equation of a social system, if it is to have any systematic significance whatever—is not conceived as a motive of historic action.

Indeed, the purposes of human action, to the extent that they are internally comprehended at all in this kind of system, are understood in a largely unhistoric sense as tending to an equilibrium. This is true in spite of the fact that the theory of integral groups claims to analyze social change systematically, and the expositions of the doctrine contain a good deal of straightforward description of the historic purposes of various groups and of their consequences, exactly as unsystematic historiography does. One should note that in most of these descriptions the source of social change is some factor "external" to the social system: population growth, technological change, the rise of new cultural values, and so on. The distinctive contribution of the analysis is not to establish a systematic relationship of such historic trends and historic aims to each other but to follow out their effects within the system of the integral group. Most historic purposes of groups, then, are external and, in relation to the system, are chance factors when considered over the range of their free play. Within the system such purposes are analyzed in terms of the strains to which they subject the existing integration of the society and the limits to which they

are confined or the alterations they must undergo if the group's equilibrium is to be maintained.⁸

By restricting its systematic analysis to the equilibrium of purposes within an integral group, sociology largely excludes historic purpose from its consideration. The motives which determine history are essentially indifferent to the integration of social units—except, of course, in the limiting case when dire necessity makes the preservation of the group a conscious, and hence a historic, purpose. Apart from this and other special cases,⁹ the purposes of historic action—particularly the specific purposes to which whole groups devote themselves—are not usefully described as functions in the system of an integral group. Such purposes may vary widely without discernible effect on the integration of the group. This is a point which has been fully documented in Ruth Benedict's classic study, *Patterns of Culture*.

The fact that the divergence or rapid change of purposes may produce "strain" does not mean that such strain is necessarily a sign of disintegrative tendencies in the group. There is a systematic relationship of historic purposes themselves,¹⁰ and the strain of the clash and shift of historic purpose may be best understood as a strain

⁸ Talcott Parsons, *The Social System* (Glencoe, Ill.: Free Press, 1951), pp. 480-535. The "institutionalization" of a tendency to cumulative change, as in the case of the profession of science, does not make the concrete development of science any the less an external factor, as far as the social system is concerned. It means merely that the social system has adjusted its equilibrium system to allow for the free play, within limits, of this chance factor. In the same way, the history of agriculture is not directly advanced by knowing (what is undoubtedly a relevant fact) that the human organism can adapt itself to a variable diet of cultivated foods.

⁹ Notably the case of the "institutionalization" of a charismatic idea, studied, for example, by Weber and Troeltsch in the field of religious social history.

¹⁰ Using the terminology of the "general theory of action," these relationships occur within the system of culture rather than the social system; and it should be noted that culture, unlike the social group, is inherently historic, involving an inter-generational span.

purpose in the ordinary sense of deliberate, conscious intent, it can be comprehended under a wider definition of "purpose" as an inherent "goal" governing the direction (functional or dysfunctional) of processes within a teleological system. Cf. R. G. Collingwood, *The Idea of History* (Oxford: Clarendon Press, 1946), pp. 309-15.

within that system. For example, the Japanese adopted the whole of Western technology and scientific culture in the nineteenth century while maintaining their social relationships relatively unimpaired—but not without cultural strain. On the other hand, the rise of a new historic purpose may impose an intolerable strain upon a social system to the point of its complete disintegration.¹¹ But the continued dominance and development of that purpose within the system of culture need not be affected, for it will be carried forward by a new group or by a new organization of the members of the old. Examples of such a process are too numerous to need citing.

Let us now consider a second way in which the theories under discussion seem to disqualify themselves as a method of systematic historiography. Not only do these theories exclude the specifically historic from the conceptions they have hitherto developed. They also appear to promise that in a later stage of development they will arrive at new conceptions by which to determine those things in history that cannot in principle be determined.

The basis upon which this impossible undertaking is to be achieved is through the development of the sociological conception of deviance. Under the sociological assumption that social integration is the norm which establishes an intelligible direction for social processes and that, if change is to be interpreted systematically, it can only be in terms of strains that drive toward integration, the question arises: How do the overall forms and patterns of the integral societies themselves change, as we observe them to do? It is in this kind of question that such theory tends to formulate the problem of history;¹² and the answer generally given provides a clue to the sort of conceptions by which it hopes eventually to achieve a systematic analysis of history. Changes of integral societies themselves, as distinguished from changes within a social framework, will

occur, it is said, when a deviant trend is so powerful that it cannot be accommodated within a given form of the integration of the group. Such deviance then destroys the integration of the group on the old basis and establishes new ultimates, charismatic centers of attraction for the whole society, around which a new form of social integration grows up under the familiar laws of the sociology of the integral group.

When sociologists tell us they hope to gain such full knowledge of the processes within the framework of the integral group that they will be able to progress to the analysis of total changes of the framework, they imply that they will be able to make certain determinations: not only to understand why a particular kind of deviance puts intolerable strain upon social integration but also to predetermine what forms such deviant trends will assume in order to be capable of forming a new nucleus for the organization of the historically succeeding integral society. These two determinations are only superficially separable; for, short of physical extermination or dispersal, there is no such thing, in reality, as the total disintegration of societies.¹³ So long as a group has physical existence, it is always tending by definition to one form of integration or another. Thus sociologists have even written studies describing the social system of German concentration camps and extermination centers. Consequently, there is only one way to

¹² Some of the fallacies involved in this description of the historic process have been shown above. Here, possibly, is the reason that Talcott Parsons shows notable caution in defining the place of history among the social sciences, if, indeed, it is not more properly placed among the humanities—which he is more than half inclined to believe (*op. cit.*, p. 555).

¹³ This point is clearly grasped by Homans (*op. cit.*, pp. 269–70). But he concludes that it is essential, for this very reason, to confine sociology at the present stage to the study of small groups, where the processes of integration and disintegration can be concretely observed to take place. What is learned there may later be extended to the analysis of greater societies. It does not occur to him that the integration and disintegration of groups is not, in fact, the essential process of history.

¹¹ Note that we refer to the disintegration of the *social system*, not of the group. The latter, as we shall see, would have little meaning.

determine when the integration of a group has succumbed to an intolerable strain. This is a conclusion that is drawn after it has been ascertained that the form of integration of the group is no longer what it once was but has become something discernibly different. Consequently, the hope of the sociologists that they may ultimately be able to produce a systematic history rests on the ability to predetermine the charismatic centers around which future societies will organize themselves.

But it is just this claim which is inherently impossible. For the historian is on unassailable grounds when he insists on waiting for the ultimate event before attempting to define the nature of those purposes to which a society dedicates itself in crucial moments of history. If the sociologist were able to predetermine accurately the nature of a new charismatic ideal capable of reorganizing society in a new framework, he would be not a scientist but a historic genius. For one of the essential characteristics of such an ideal, according to the sociologists themselves, is precisely the mysterious quality of attraction which makes a man a leader and an idea a collective passion. Before the presence of that quality can be established—or should we say, postulated—it is necessary to ascertain its ultimate effect, the triumphant achievement of a historically crucial innovation.

If, then, this is the only basis upon which sociology could undertake to provide a systematic theory for historiography, the task would seem in the nature of the case impossible. But let us not incontinently abandon the hope of applying a sociological theory and method to the problem of history. All we need discard is the particular sociological theory which is based on the assumption of the integral group as its fundamental category of explanation. There still remains the possibility of using the other tools of sociological analysis—the concepts of function and system and the desideratum of a logically exhaustive classification of factors—in such a way as to isolate in a logically ade-

quate manner the specifically historic processes and phenomena.

Such a systematic approach to history must be fully aware of what it can and what it cannot legitimately undertake to accomplish. That unforeseen and, in principle, unforeseeable creative innovations occur in history is not a deduction that must be derived, as we have done, from the attempt to apply the sociology of the integral group to history. It is an observation directly made by anyone who considers history, however superficially. Not only, or primarily, the framework of the integral group is radically altered by creative innovations but also, and above all, the traditional frameworks of the several realms of the historical system of culture. Such innovations cannot, in principle, be predetermined by any systematic analysis of historic purposes, since the new purpose that must be formulated in order to interpret the new cultural framework is, in a very specific sense, an emergent. It does not arise as the logical answer to a problem stated in clearly defined terms known in advance. Rather does it “solve” such problems by transcending them, by altering their terms: it renders them obsolete by introducing new factors and hence a new situation. Only after these are ascertained by the historian who has patiently awaited the ultimate event can the attempt be made to formulate rationally a chain of purposes that could interpret the event.¹⁴

But what we require is a sociological method which, unlike the sociology of the integral group, is capable of conceiving and isolating historic processes not only in the radically indeterminate phenomena of crea-

¹⁴ Theoretically, of course, the same rational chain of purposes could be imagined before the event, if we could imagine the emergent new factors and situations that will ultimately alter the terms of the problem as it appears to observers at a given time. But, in theory, our imagination could equally well construct an infinite number of other possible emergent events, none of which may chance to become actually rather than potentially historic. Only in science fiction is any science conceivable which could predetermine which of all possible emergents actually emerges.

tive innovation. If the framework of our analysis is the historic system of culture, not the abstract system of the integral group, we are able to isolate historic processes in the determinate systematic relationships of the various realms of value to each other.

We must distinguish between two types of purposes, both of which determine historic action and both are employed in historical interpretation. The one is a synthetic purpose ascribed to a whole group (culture, civilization, nation, or other social unit) by historians in order to interpret an entire chain of events in the light of the ultimate event, the intuitively observed character of one's own time. Such "purposes" are a *Zeitgeist*, a Renaissance spirit, a Baroque style, a Magian culture, a spirit of capitalism, or a conception of divine purpose, of sin and redemption. The others are particular purposes that can be concretely ascribed to particular men or groups of men at particular times in particular situations: a foreign policy, an urge to emigrate, and so on.

Let us not deceive ourselves. No history can possibly be written solely in terms of the latter type of purpose. Whoever has tried to do so has inevitably been guided by some suppressed imaginative construction of the former kind, even if unbeknown to himself. So, too, systematic historiography—the kind, in short, that we require for our analysis of contemporary affairs—must necessarily fall short (except if it permits itself to become visionary) of the synthetic comprehension which only historic perspective, from the vantage point of the ultimate event, can give us. But what such a systematic historical analysis *can* do is to define the "strains" in the system of cultural values, the problems that arise from the interplay and clash of concretely definable purposes.

To be able to do this systematically, we need the aid of sociological method. But the

sociology of the integral group cannot approach this task; it can conceive history systematically only as the rise and fall of such major charismatic images as can neither be produced nor interpreted in any other way than by imagination and intuition. Sociology, in order to provide a theory for systematic historic analysis, must first of all free itself of that assumption. It must then consider anew a portion of the general theory of action which has been said to form a "system" parallel to the personality system and the social system, namely, the system of culture. The nature and structure of that system have not, however, been adequately examined.

The system of culture, upon re-examination, will be seen, first of all, to be a historic system. This, in itself, distinguishes it sharply from both the personality system and the social system. Then, second, as a system of symbols it will be seen to require a different set of elementary concepts from the personality system and the social system, for it is related not only to history but to the material environment in a way far different from the other two: as a system of realms of value it comprises history and the "external world" in its internal functional relationships and is not related to them as merely external conditions. Thus, a new classification of the elementary concepts of action, based on new definitions of value in which history and the "external environment" are comprehended, is also needed.

When these are provided, sociology may be in a position to give us the theoretical framework—the logically exhaustive classification of functions mutually related within a closed system—required to write history systematically, insofar as this is in principle possible.

HARVARD UNIVERSITY

HISTORY AND SOCIOLOGY: NEW OPPORTUNITIES FOR CO-OPERATION¹

SYLVIA THRUPP

ABSTRACT

Various irrationalities that spring from the social structure of academic life make it difficult for sociologists and historians to enjoy full and up-to-date use of each other's findings. Stereotyped misconceptions of each other's methods persist. A new organ of communication, publishing fresh contributions in fields in which comparative study proves helpful and stimulating to all of us, could bypass the impediments.

While taking an inventory of what history and sociology may have to offer each other, it is pertinent to ask in what context there is likely to be any strong demand for the offerings. The ideal context would be one of pure rationality. There, if history be defined as the search for a synthesis of human experience as reflected in the course of events including the propagation of ideas, and sociology as the search for regularity, for structure, in human relations, we would co-operate actively both in methodological advance and in empirical comparative studies. It would be evident that, since regular types of structure—families, communities—are actors in events, and since events affect their structure, we should all be both historians and sociologists, divided only by our individual decisions to specialize in one period or aspect of human experience. The question of what the one discipline has to offer the other would then be seen as identical with that of ordering our individual reasoning processes, or co-ordinating more happily, as it were, the right hand with the left, and also, as separately organized right-handed and left-handed professions, through comparing and sifting the variety of our findings.

The context is not, however, one of pure rationality. The complex structure of aca-

demic life in which we are held requires much else. Departmental solidarities build themselves around tradition, prestige, rivalry, personalities, and temperament. One may often marvel that rational reflection can proceed at all in this atmosphere of side shows and fireworks. Yet methodological discussion thrives, wherever it has encouragement, and is making headway. It is the second or auxiliary step, the promotion of empirical comparative study, that lags. How this step in turn may find encouragement is the theme of this paper, which seeks to locate the chief obstacles and to map a way around them.

Perhaps the most lethal obstacle is the proselytizing spirit. The terms in which our topic today has been cast, as to how we may learn from each other, leans a little dangerously in that direction. For the proposition that history and sociology may learn from each other is perennially attractive to those who look forward less to learning than to instructing and converting. On one side is a sincerely rooted feeling that whatever may once have been interesting about the dead inevitably dies with them, that written records can be of little use until a uniform system of record-keeping is instituted by social scientists, and that the tendency of historians to be preoccupied with individual experience and achievement, or even with the working of particular institutions at particular times, is unscientific, a waste of life. It follows that whoever still insists on digging into the past should at least work to estab-

¹ A paper prepared for a panel on "History and Sociology: What Can They Learn from Each Other?" at the session on "Sociological Study of Historical Documents" at the annual meeting of the American Sociological Association at Detroit, September 7, 1956.

lish laws of human development. On the other side there are some who feel that sociologists would be better occupied in reading history than in poking questionnaires through keyholes. Admittedly, expression of these feelings is seldom more than semiserious. Like the ponderous verses in which fifteenth-century professors of law and medicine would revile each other as thieves and butchers, they relieve the solemnities of academic routines. Yet behind them is a distrust that hinders co-operation.

To some extent distrust may be allayed by the fuller understanding of the present division of labor between us that methodology is gradually developing. For instance, that long-chewed bone, the issue of the intellectual superiority of a search for regularity, pattern, law, rather than for the particular shape of events and their sequence, is surely by now overripe for burial. The classic distortion of the contrast between science and history that identified them, respectively, with pure generality and pure particularity, and so with the giant growth of natural science, played to sociology's need, as a relative newcomer, for a family affiliation and prestige, is now coming to be modified by admission of the necessary interpenetration and interdependence, in all types of scientific and historical work, of both generalizing and descriptive activity. Economic historians, most of whom now have some theoretical training and have therefore bifocal points of view, have contributed helpful analyses,² but there is still no general appreciation of the diversity of

methods that are employed by those working historians who first deploy fresh historical fact. The examples in so recent a treatise as Patrick Gardiner's,³ supposedly to illustrate the role of generalization in historical writing, are drawn almost exclusively from broad moralizing treatments of well-known phases of political history; they bear no relation to the role of generalizing hypothesis in historical research. As one philosopher, Isaiah Berlin, who is also an intellectual historian, has lately remarked, "The 'logic' of various human studies has been insufficiently examined, and a convincing account of its varieties, with an adequate range of concrete examples drawn from actual practice, is much to be desired."⁴

Distrust arises also from the differences in outlook that correspond to the differing demands made on our professions. It is the age-old business of historians to create and maintain a sense of continuity within the larger units of social life, and to do so in a lively and dramatic fashion. They grow dull on pain of losing their market to novelists and journalists. All schools of Western history save the Marxist have met this demand by exalting the individual as actor, whether in heroic endeavor or in antisocial disturbance.

The public imposes on sociologists a contrary bias. They are paid to exalt the group, to work for the damping-down of all such individual vagaries, no matter how dramatic, as may be inconvenient to the group. Like medical columnists who expound rules for healthful living, they are looked to for advice on how to bring delinquent juveniles, drug addicts, the unhappily married, and criminals into conformity with current norms of adjustment. This practical concern with predictability, and with limited forms of social engineering, makes them feel at home with doctrines of social determinism.

There is here a potential clash between

² See in particular F. C. Lane's concluding chapter in F. C. Lane and J. Riemersma (eds.), *Enterprise and Secular Change* (Homewood, Ill.: Richard D. Irwin, 1953), and *The Social Sciences in Historical Study: A Report of the Committee on Historiography* (New York: Social Science Research Council, 1954). Cf. the view of Malinowski: "The hackneyed distinction between nomothetic and ideographic disciplines is a philosophical red herring which a simple consideration of what it means to observe, to reconstruct or to state an historic fact ought to have annihilated long ago" (B. Malinowski, *A Scientific Theory of Culture and Other Essays* [Chapel Hill: University of North Carolina Press, 1944], p. 7).

³ *The Nature of Historical Explanation* (London: Geoffrey Cumberlege [Oxford University Press], 1952).

⁴ *Historical Inevitability* (London: Geoffrey Cumberlege [Oxford University Press], 1954), p. 5, n. 1.

us, since non-Marxist historians cannot tolerate doctrines of determinism. They regard them as distasteful, unproved, and as symptoms of confusion. It must be realized that in doing so they project them on a far wider stage and take a far more rigid and thoroughgoing view of the matter than many sociologists do, who may feel no need to think further ahead than statistical projection leads. As to the wider stage, those who are accustomed to think primarily in terms of structure and to be uninterested in more than the broad trend of events may see no reason why changes in structure should not be determined or at least roughly predictable with increasing degrees of exactitude. For the historian, since there is interaction between event and structure, determinism would have to apply to both. Valid predictability could never be established for structure alone without reference to the course of events. It might appear to hold, yet reference to events would be bound to show that the explanation involved was in some respects incomplete or mistaken.

Controversy in this area has latterly been warming up, with Berlin and Bock exchanging charges of confusion and timidity.⁵ The more heat that can be generated the better, provided only that some of it can be converted into research energy. For none of the key positions is well supported by empirical demonstration. Historians cannot reject the predictability of structural trends on logical grounds alone. Their skepticism is based in part on encounters in the course of research with numerous cases of unexpected innovation or with rapid changes in the form or function of institutions that could not have been predicted from a knowledge of their structure. Knowledge of this kind is haphazard; there has been no attempt, yet, to make it systematic.

Where, then, can we best look for common ground on which to test our differing views and doubts? At present there is no

great overlap in the type of problems that we choose for research. Historians' knowledge of most of the matters to which the modern sociologist's interest in conformity and psychological adjustment directs him is slight and chaotic, and the majority of sociologists have shown only occasional interest in the national political structures to which historians have devoted so much labor.

One area that is promising, for the reason that we are both strong in it without claiming infallibility, is that concerning the fundamental problems of social stratification. Sociologists have a long lead in the relevant techniques of sampling and measurement and in the practice, essential here because of conflicts in the prevailing theories, of making at least one's major premises explicit. Comparability can thus be insured and controlled. In this connection the codification of basic elements of contemporary social theory by the Parsons school is of great initial service. A member of this school, Bernard Barber, is already breaking the path to which this paper points by co-ordinating a volume of studies of social stratification, being written by historians, set in different centuries. Work such as this, especially when it can be linked with new demographic research and with studies of the influence of different forms of family grouping and of law and custom relating to inheritance, can very substantially extend our knowledge of social systems.

Sociologists, still leading from their own areas of strength, could further offer theoretical analyses and model empirical studies of the more important of the problems of social control that they investigate in the contemporary scene. There would be no difficulty in finding social historians to probe situations illustrating comparable problems and in tracing historical changes in the policies by which they were handled. The results would find an interested audience among legal and political historians and historians of social philosophy. For the argument by which political historians, in particular, criticize sociological generalizations

⁵ *Ibid.*; cf. K. Bock, *The Acceptance of Histories* ("University of California Publications in Sociology and Social Institutions," Vol. III, No. 1 [Berkeley: University of California Press, 1956]).

—that structural trends cannot be projected without reference to events—cuts both ways. Any full understanding of the course of events depends on a clear view of the structure of social relationships. In the field of medieval studies this has long been accepted as axiomatic.

There is no question that able sociologists are eager to promote such forms of comparative study. A start has been made. They can hardly, however, be expected to exert the long effort that will be required to produce results on any very impressive scale without moral support from their own profession. An audience of historians is not enough. Yet will the average sociologist join the audience? Will he be afraid, if he is seen reading a journal of "Comparative Studies in Society and History," of being thought unscientific, antiquarian, a deviant in his profession, maladjusted? Positive and plausible reasons may be given for ignoring the new type of enterprise. Knowledge of a single society, it will be urged, is sufficient for the perception of regularity in human affairs and for a fair sampling of their basic forms. Again, given the variety of the American scene, it is only by long concentration on special problems that sociologists can usefully advise policy-making agencies in the manner expected of them.

Despite the truth of these assertions, sociologists cannot honestly say that they deal only with the contemporary scene and its special problems. In their capacity as university teachers they discourse on society in general. The illustrations in their elementary texts and introductory courses are culled freely in space and time. But whereas there is scrupulous care for the accuracy of information taken from research reports on contemporary matters, there is little or no concern to verify, by reference to works of up-to-date scholarship, the accuracy of such information given as purports to relate to the past. Statements abound that are pseudo-historical, that have as little contact with fact as a ballet dancer with earth.

We are here faced with a very obstinate tradition that tends to be self-perpetuating.

Students brought up on such a diet naturally believe, out of loyalty and self-interest, that it is the best available. To persuade them that historical material can ever rival, in depth of suggestive detail, the content of a questionnaire or interview report is next to impossible. Most are unable to look into historical data, for their training has never required them to do so. At best they will look into translated extracts from the writings of Max Weber but probably without realizing that even Weber's powerful mind was poorly informed as to oriental societies and that, in every field he touched, a half-century of fresh research has supervened since he thought out his classic generalizations.

To a lesser degree, a similar situation has ruled in American anthropology. Here, however, antihistorical prejudice arose out of the extreme difficulties of reconstructing the histories of preliterate peoples. Now that some of these difficulties are being surmounted, the prejudice is in full retreat. The new journal, *Ethno-History*, is founded on realization that much misinterpretation has crept into North American ethnology through neglect of historical interaction between Indian and white and is devoted to correcting the perspective.

If it may be shown that misinterpretations creep into structural studies through too summary dismissal of historical influences, perhaps a parallel appeal would find a hearing among American sociologists; the lengthening time perspective that they are inheriting as one academic generation succeeds another is preparing the way. It is implicit in a number of revisions of previously accepted theory; for example, in the revision of theories that too sharply differentiated urban and rural structures and attitudes. The new stress on the persistence in each of similar elements of popular culture will be congenial to historians.

A journal of comparative studies may therefore have a better chance of modifying attitudes of indifference to history if in addition to widening the sociological horizon in areas of strength, as suggested above, it probes weaknesses. The problems involved

in taking adequate account of historical influences that may be at work in any situation chosen as the starting point of a piece of research are indeed a source of trouble to us all. Historians have no foolproof solution. Ideally, they aim at becoming familiar with all aspects of the culture of a period before singling out particular matters for investigation. This gives one hunches as to the points at which events, ideas, structures of relationship, are taking a genuinely new turn, modifying or breaking with regularities and directions set in the past. Hunches are tested by wide general reading and some check of the primary sources of the preceding period. But these are counsels of perfection that in practice are often bypassed as too laborious. Specialists in a period can develop enthusiasm for a priori judgments about its antecedents—judgments that will undergo intermittent flurries of revision with changing intellectual fashions. European historians of the sixteenth century, for example, now realize that they have too long been prone to interpret anything they observe as a new development without looking fully enough into its medieval antecedents. Communication between specialists in different periods, which should correct such tendencies, is hampered by the habit of presenting fresh findings in a manner that may be intelligible only to one's immediate fellows, that is, with a degree of allusiveness that is analogous to a private technical language. This baffles other types of specialist, repels the layman, and delays criticism of the general perspective or framework.

Another means of assessing what historical influences may be operative at a given starting point, a more summary means, has long been popular both among modern historians and in sociology. Devised by eighteenth- and nineteenth-century scholarship, it rested on a faith that the kind of society then developing in western Europe and America was the destined goal, or at least a necessary stage, perhaps desirable, perhaps undesirable, in human evolution. The more striking contrasts that were felt to prevail as between East and West, modern and me-

dieval, European and American, were shaped, not into measured yardsticks of progress, but into a kind of litmus stick for ascertaining whether a given society or period was in a state of appreciable progress or relatively stagnant. If the former, all trends were assumed to be running toward individualism, increasing differentiation and complexity. If the latter, custom and tradition, interpreted as fixed irrational resistances to change, reinforced always by religion, would explain all.

We are all now handling such hypotheses with more caution, guarding against their reification in typology and model construction. But we have not yet exhaustively retested our stock of sociological and historical impressions for fallacies that cruder forms of the old hypotheses may have admitted. Comparative study could economically speed up this house-cleaning.

Comparative study can of course be no more than a tool in the service of hypothesis. Without fresh imagination we can do no more than dig ourselves into aimless labyrinthine researches. To be obliged to consider our material, however, in the light not only of its immediate context and of theory derived from this but of queries and hypothetical generalizations arising from someone else's consideration of an apparently similar problem in some different context is itself a stimulus to the imagination, and the experience need make no extra demands on that scarce and precious asset, our time. Indeed, our time could, as it were, be stretched further. There is no occasion to entangle ourselves in cumbersome co-operative projects in which everyone tries to out-talk everyone else in selling his own point of view, in which the stenographic record alone demands foundation financing. All that is essential is the linking of researches already in progress through outline clarification of a series of representative problems that test interaction of event, idea, and structure. Outline clarification should proceed both on the plane of theory and in the empirical forms that the problems assume in different

contexts. The more important relevant variables will then be progressively better understood as each problem is pushed into perspective through being considered in many different contexts.

The kinds of hypothesis that historians are best fitted to contribute to such work are those that probe intercultural contacts and historical continuities in search of common origins for recurrent attempted solutions of human quandaries. Some sociologists may prefer to branch off on other lines and to essay historical researches of their own for confirmation of their theses. They will find they need technical help. Historical research is a complex social enterprise in which one has to lean on technical aids, developed

gradually through many generations of scholarship. Without these the most brilliant mind could make egregious blunders, as would any historian who essayed statistical calculations without adequate mathematics.

Perhaps, before long we will begin demanding that doctoral candidates both in history and in sociology each complete a minor thesis in the other's field. Differences that now sometimes bulk so large between us would then within a generation shrink to the dimensions of stray differences between individuals. We are unlikely to become so rational as to give up, altogether, our side shows and our fireworks.

UNIVERSITY OF CHICAGO

ATTITUDES TOWARD WORK IN AN INDUSTRIAL COMMUNITY

GLADYS L. PALMER

ABSTRACT

A unique combination of lifetime work histories and answers to a variety of attitude questions by a random sample of residents of Norristown, Pennsylvania, provides the framework for analyzing workers' concepts of success and their attitudes toward jobs. Although future occupational attachments cannot be predicted from views about the current job or from vocational aspirations, certain differences emerge between young and old, single and married, men and women, whites and non-whites, and manual and non-manual workers. Concepts of success, especially with regard to achievement in a work career, may change in the life-cycle, and work attitudes may depend on the age at which a career is established.

Recent studies of workers' concepts of getting ahead or of success in life, and attitudes toward work versus leisure in relation to attitudes toward their current jobs, have raised important questions about work as a way of life as well as a means of livelihood.¹ The discrepancy between the American dream reflected in the Horatio Alger myth and workers' experience in mass-production industries led Chinoy and Guest to study automobile workers' aspirations and their work experience.² Both writers conclude that the concepts of "success" of automobile workers have narrowed either to immediate gains in material possessions or to advancement in pay or job assignments. Chinoy suggests that a man's aspirations are likely to contract around the age of thirty-five. Both writers find that some form of self-employ-

ment is desired as an escape from the automobile-assembly line and that workers may transfer their original aspirations to their children.

Friedmann and Havighurst, by studying older workers in five occupational groups of differing grades of skill, specifically set themselves the task of finding out the degree to which work has recognized meanings in addition to those of earning a living.³ They conclude that workers of lower skill and socioeconomic status are more likely to see their work as having no meaning other than that of earning money but that all five groups contain some members for whom work is a source of interesting and purposeful activity or intrinsic enjoyment. Morse and Weiss find that work serves non-economic as well as economic functions and that the functions differ for men in middle-class and in working-class occupations.⁴ For the latter, keeping occupied is apparently more important than interest in the job or a sense of accomplishment, and only a third of this group would continue in the same type of work if they could afford not to work. As in other studies, the thing most desired by

¹ Ely Chinoy, *Automobile Workers and the American Dream* (Garden City, N.Y.: Doubleday & Co., 1955); Eugene A. Friedmann and Robert J. Havighurst, *The Meaning of Work and Retirement* (Chicago: University of Chicago Press, 1954); Robert H. Guest, "Work Careers and Aspirations of Automobile Workers," *American Sociological Review*, XIX (April, 1954), 155-63; Nancy C. Morse and Robert S. Weiss, "The Function and Meaning of Work and the Job," *American Sociological Review*, XX (April, 1955), 191-98; Charles R. Walker and Robert H. Guest, *The Man on the Assembly Line* (Cambridge, Mass.: Harvard University Press, 1952).

² Chinoy's study is based on intensive interviews with 62 male production workers of varying ages and seniority in a midwestern automobile-assembly plant; Guest's, on interviews with two samples of hourly production workers in an eastern automobile-assembly plant.

³ The sample studies covered steel workers, coal-miners, printing-trades craftsmen, department-store sales personnel, and physicians and were limited to older or retired males (except salespersons), located in Chicago or Illinois.

⁴ These conclusions are based on data for men in a national sample survey made by the Survey Research Center of the University of Michigan, from the analysis of which self-employed persons, clerical workers, and government employees were excluded.

manual workers is to go into business for themselves, the type of business being frequently unspecified. Despite variations in the degree to which the work provides satisfactions other than as a means of livelihood, this study finds a general tendency for individuals to like what they are currently doing.

The sense of frustration reflected in the studies of men in automobile-assembly plants may be an extreme case, because relatively high rates of pay and seniority privileges tend to keep them attached to plants that have limited transfer and promotion opportunities. Scattered evidence from other occupations yields a more complex picture of satisfaction with the job and workers' views of the function of work. An opportunity to analyze data bearing on these questions for a general population sample⁶ is found in a study of residents of Norristown, Pennsylvania, a non-satellite but an industrial community in the Philadelphia Metropolitan Area. Norristown is an old industrial center with diversified economic activities; it is also the political and social center of an extensive agricultural and semi-industrialized hinterland. A large proportion of the community's residents are factory employees, but the study covers all types of occupations, both men and women and white and non-white workers.

In a broad social survey of Norristown, Pennsylvania, data gathered included con-

⁶ The Norristown sample consists of 926 randomly selected households (a 10 per cent sample), in which one adult in each household was interviewed in 1952 for a complete work history and answers to an extensive battery of attitude questions; a total of 797 schedules with complete work records was obtained. Of this total, 517 persons eighteen years of age or over were in the labor force at the time of interview, 23 men had retired from the labor market, and 257 women were non-workers. All but 42 of the women who were not in the labor force in 1952 had been employed at some time prior to that date.

Selecting one adult per household on a systematic basis does not yield a properly weighted occupational distribution for workers in the city, since relatively as many women as men tend to be included by this procedure. Nevertheless, there is no bias with respect to the selection of the individuals,

cepts of success, attitudes to currently held jobs, vocational plans when starting to work and if starting again now, and what vocational advice would be given to one's children. The interview questions were purposefully non-directive or open end in character in order to secure replies that reflected initial reactions and as much local color as possible.⁶ At this experimental stage unstructured responses may yield useful clues to the complex nature of the phenomena, especially when lifework histories and other factual data are available to check the expression of attitudes with what eventually happened.

either as representing their occupations or as representing worker attitudes; moreover, the general profile of the sample occupational distribution is correct and so are the proportions of manual and non-manual workers to the total:

	PER CENT DISTRIBUTION	
	1950 Census	1952 Sample
Total employed.....	100	100
Professional workers.....	9	7
Managerial workers.....	7	11
Clerical and sales workers.....	20	18
Craftsmen.....	16	16
Operatives.....	30	30
Service workers.....	10	13
Laborers.....	8	5

The industrial distribution of employment for the sample workers in Norristown is also close to the structure of employment, as reported in the 1950 Census of Population:

	PER CENT DISTRIBUTION	
	1950 Census	1952 Sample
Total employed.....	100	100
Agriculture, mining.....	..	2
Construction.....	6	5
Manufacturing:		
Durable goods.....	21	20
Non-durable goods.....	23	22
Transportation, communications, utilities.....	8	9
Trade.....	16	17
Financial and business services....	5	3
Personal services and entertainment.....	7	8
Professional and related services..	11	11
Public administration.....	3	3

- ⁶ a) Of all the jobs that you have ever had, which job did you like best? — Why? —
Which job did you like least? — Why? —
b) Do you like your present job? — Why? —
c) What did you hope to become when you left school or started work? —
d) If you had it to do over again what would you want to be? —
e) How would you try to achieve it? —
f) If you had a son (or daughter if interviewee is female) starting out now what would you

This study corroborates some or all of the findings of the other studies cited but, as might be expected, shows that the labor force of an industrial community has a much wider range of attitudes to success in life, to vocational plans, and to jobs than do workers in selected occupations. In broad terms, attitudes differ between old and young, single and married, men and women, white and non-white, and those employed at different levels of skill, as well as between persons not currently in the labor force, and those at work.

CONCEPTS OF SUCCESS

The responses to the question what "being successful" means range widely as to specificity and ideas or ideals. Some commonly accepted clichés naturally appear, such as "money, just money" or "being the son of a millionaire," but, on the whole, Norristown residents apparently have relatively modest goals, in which security of income or savings, achievement in a job or work career, or a generally happy and comfortable life predominate. Both workers and non-workers are fairly articulate on the bringing-up of children who "turn out well" or on happy family relationships and friendships, as one measure of success. In one form or another, however, most replies reflect the theme of a thirty-two-year-old crane operator of Czech extraction, who said in answer to this question:

I can tell you exactly; I've often thought of it. I want to have the home I've always dreamed about and a new car. Send the kids to college and have a good bank account. That's my goal. I don't want a million dollars or a lot of land, but I'd like to take a few weeks' vacation without worrying about the money.

advise him (her) to become? — Why? —

g) People often talk of "being successful." When you talk of "being successful," what do you mean by it? —

h) What sorts of things are most important for getting ahead today? —

The writer is indebted to the Behavioral Research Council of the University of Pennsylvania for access to the information obtained.

Owning a home, money in the bank, and a "good" job with "decent" pay or steady income reappear again and again as the visible signs of a successful life. For some persons, happiness, reputation in the community, moral standards, or good health are added as goals. For others, achievement in a work career is emphasized, usually in general terms of advancement or recognition on the job.

These generalizations should not be taken to mean that all Norristown residents consider themselves successful.⁷ Many housewives who emphasize "making money" as the way to success give such comments as, "We have it hard trying to get along," "We just get by," "We had some hard luck and lost our business." Reminiscent of Chinoy's findings is the comment of a forty-five-year-old automobile-assembly worker with considerably higher earnings than the average in Norristown: "We don't have anything to look forward to except working the rest of our lives."

On the other hand, a more optimistic view can also be found, even if one excludes representatives of the high-income brackets or persons with work records like Horatio Alger's. A number of cases reflect the theme in the statement of a sixty-year-old freight conductor who says, "I think I made a success. I perfected my work; I was able to buy my own home in the early days. I was lucky enough to get a good wife. What more can you expect?" Even more typical of Norristown workers are the criteria of success given by a fifty-six-year-old worker earning \$38 a week at a state hospital, namely, "being able to live nicely, own a car and television, and owe no bills." Then he said, "We have things nice, and we don't owe nobody."

Some married men conceive of success as being a "good provider for the family," and

⁷ In choosing the following illustrations, we excluded from consideration men over sixty-five who had retired from other kinds of work but could not live on their pensions and did janitorial or odd jobs and sometimes apologized for having to accept aid.

a few note with appreciation that their wives are "good managers." Most wives appreciate that their husbands work hard to make ends meet, but occasionally there are comments such as these: "My husband is an incompetent provider" or "When you marry, you have to give up ideas of success."

All the threads of current problems in everyday living in an industrial community are reflected in these comments. The struggle to save a dollar here and there and to get out of debt is a recurring theme. There are a few who think that "if you can walk

ers; among workers, women stress non-economic goals to a greater extent than men.⁸

When one compares the social characteristics associated with attitudes, one finds emphasis on achievement in one's job or work career appearing in the responses of younger, single workers, and those in non-manual occupations (Table 2). Emphasis on economic security, as might be expected, is associated with a higher average age and a higher proportion in manual occupations. Within this group, emphasis only on "making money" is associated with the lowest aver-

TABLE 1

PERCENTAGE DISTRIBUTION OF CONCEPT OF SUCCESS OF WORKERS AND NON-WORKERS BY SEX

CONCEPT OF SUCCESS	Total	WORKERS*		Total	NON-WORKERS*	
		Men	Women		Men	Women†
Total						
No.....	446	290	156	226	18	208
Per cent.....	100	100	100	100	100	100
Emphasis on economic security....	49	53	39	39	33	40
Security of income, savings.....	22	25	15	15	11	15
Security of income, savings, combined with other goals except achievement in job.....	19	20	17	19	16	20
Accumulation of "money".....	8	8	7	5	6	5
Emphasis on achievement in job or work career.....	31	29	34	26	17	26
Advancement in job.....	17	16	18	17	17	16
Advancement in job combined with other goals.....	14	13	16	9	..	10
Emphasis on non-economic goals...	20	18	27	35	50	34
Happy family relationships.....	4	2	8	11	6	11
Other goals‡.....	16	16	19	24	44	23

* Persons eighteen years of age or over. Excludes 71 workers and 54 non-workers with no reply or uncodable response.

† Includes 12 single women most of whom had retired from jobs.

‡ Emphasis on good health, good citizenship, high moral or religious standards, and other aspects of personal relationships (alone or in combination with economic and non-economic goals).

down town and hold your head up because you owe no bills, you are successful." There are others who emphasize "being a good Christian" or other moral and religious goals, and there are some who say that success is "peace of mind."

Any statistics about these attitudes conceal their variety and the complex situations depicted but may yield some generalizations. The variety of goals mentioned by most Norristown residents as measures of success in life have been classified in Table 1 according to the major emphasis of the wording of the response. Retired men and non-working women emphasize non-economic goals to a greater extent than work-

age weekly earnings and a relatively high proportion of non-white workers. A minority stresses non-economic goals. This emphasis is associated, too, with relatively low earnings and, to a limited extent, with marriage and family responsibilities but is chiefly noteworthy because it more frequently reflects the attitudes of women than of men, except in the case of the retired.

In general, older workers in Norristown are more articulate, or perhaps one should

⁸ Men mention non-economic items as frequently as women, but their general emphasis is economic. In a count of all specific items mentioned by both sexes, those which could be classed as economic outweigh the non-economic.

say that they have thought through their philosophies of life to a greater extent, than younger persons. At any rate, their comments are likely to be more pungent. To the extent that differences by age reflect changing attitudes during the life-cycle, there are certain trends worth noting (Table 3). The proportion of workers who emphasize achievement in work as a measure of success declines sharply and the proportion who em-

achievement in a work career and to other criteria of success over a lifetime might well be worth further study.

JOB ATTITUDES AND VOCATIONAL ASPIRATIONS

The evidence on attitudes to currently held jobs corroborates that of other studies; that is, that the great majority of workers say they like what they are doing. Only a

TABLE 2
SUCCESS AS RELATED TO PERSONAL CHARACTERISTICS AND WORK EXPERIENCE
PER CENT OF TOTAL IN EACH ATTITUDE GROUP WHO ARE:*

CONCEPT OF SUCCESS	Total†	Single	Married with Children under 18	Non-white	Under 35 Years of Age	In Manual Occupations‡	Women	MEDIAN YEARS OF SCHOOL-ING	MEDIAN WEEKLY EARN-INGS, 1952§
Total.....	446	19	41	10	33	63	35	10.9	\$60
Emphasis on economic security.....	215	13	43	10	28	67	29	11.0	62
Emphasis on achievement in job or work career.....	139	23	40	12	40	59	38	11.0	62
Emphasis on non-economic goals.....	92	25	36	8	36	61	45	10.8	53

* The percentages in each column should be compared with that for the total in the top row.

† Excludes 71 workers with no reply or uncodable response.

‡ Includes craftsmen, operatives, service workers, and laborers (using Census classifications).

§ For 423 wage and salary workers reporting earnings.

TABLE 3

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PERCENTAGE DISTRIBUTION OF CONCEPT OF SUCCESS BY AGE GROUP

CONCEPT OF SUCCESS	TOTAL	AGE (IN YEARS)			
		18-34	35-44	45-64	65 and Over
Total					
No.....	446	149	106	153	38
Per cent.....	100	100	100	100	100
Emphasis on economic security.....	49	40	54	53	44
Emphasis on achievement in job or work career.....	31	38	28	29	21
Emphasis on non-economic goals.....	20	22	18	18	35

phasize economic security rises sharply at age thirty-five. This finding supports Chinoy's hypothesis that aspirations are likely to change around age thirty-five. At age sixty-five and above, emphasis on non-economic goals almost doubles in importance, and emphasis on economic security declines.

We do not know whether these trends by age reflect differences in expectations or in value-judgments during the life-cycle, but there is sufficient evidence that the reason for changes in the attitudes of workers to

tenth of Norristown workers express any dislike of the job held at the time of study, and for some it is only a dislike of certain aspects. Over three-fifths of all Norristown workers mention the nature of the work and the remainder like the job for other reasons, such as wages and working conditions or the character of associates and employers. For 70 per cent of all workers who indicate a preference among all jobs ever held, the current job is the one liked best; as one man notes, "The last is always best."

Comparisons with personal characteristics or work experience (Table 4) indicate that in the minority who dislike something about their current jobs there tends to be a relatively higher proportion of women, of non-whites, of young and single workers, and of workers with the least experience and the lowest earnings. In terms of the life-cycle, dissatisfaction with jobs may be associated with the attempts of younger workers to launch themselves on a career, but it is not wholly a phenomenon of early adaptation to jobs. Also, relatively as many self-employed persons dislike their jobs as are in the sample. This is significant because some other studies report that self-employment is frequently considered by workers as a desirable "escape" from unsatisfactory jobs.

Slightly less than a fifth of all workers in Norristown originally planned to go into the type of work in which they were employed in 1952. The vast majority are divided roughly into those who originally intended to try a different type of work and those who had had no plans or only very general ideas. The latter include women who said they wanted "to get married" and both men and women who wanted to "earn money." For the most part, however, this relatively large group "never gave it a thought" and had had "no particular ambition" or "no plans." Differences are not marked between the personal characteristics and work experience of those with no plans and those who had a specific but different occupation in mind when they started (Table 4). Most distinctive is the small group of workers who had originally planned to go into their current type of employment. They are making relatively high weekly earnings in predominantly non-manual occupations, and they are a better-educated group which includes relatively more men than women and more whites than non-whites.

Regardless of original plans, 31 per cent of Norristown workers would go into the same kind of work in which they were employed in 1952 if starting again. However, 41 per cent would enter a different kind of work, and 28 per cent would have no plans

or only general plans, such as getting more formal education, "learning a trade," or trying to get into business for themselves. Again, those who would try the same kind of work as currently held are weighted by workers with relatively more education and experience, earning higher wages or salaries in predominantly non-manual occupations. It also includes a relatively high proportion of the self-employed.

The largest single group, those who would prefer a different type of work if starting again, does not differ much from the group with no plans, but it is noteworthy for other reasons. Supplementary data show that a large majority of them like their current jobs and that three-fifths mention the nature of the work as a reason. Although predominantly manual workers, four-fifths of them, if starting again, would try to get into white-collar, largely professional, occupations. Yet these workers do not now have the educational qualifications for professional work, and their earnings will not provide them with the capital required for going into business for themselves (Table 4). Their desires may represent wishful thinking rather than genuine expectations for the present or future, since two-thirds would like to work in the occupations of *original* vocational aspirations. Some of them mention as desirable the professional occupations with whose representatives they are likely to come in contact, such as doctors or lawyers, whose standard of living and working conditions they can appraise. There is occasional comment on the quality of cars, the "ease" of the work, or the prices charged for professional services as reasons for the choice.

General attitudes to self-employment are revealed in the study. Fourteen per cent of all workers in Norristown were self-employed⁹ in 1952. In addition, 5 per cent of the total had had some experience in being in business for themselves but were not so employed at the date of study. Almost half of all reasons given for discontinuance of

⁹ Including some craftsmen and service workers as well as proprietors classified as managerial workers.

TABLE 4

JOB AND VOCATIONAL ATTITUDES AS RELATED TO SELECTED PERSONAL CHARACTERISTICS AND WORK EXPERIENCE

JOB AND VOCATIONAL ATTITUDES	PER CENT OF TOTAL IN EACH ATTITUDE GROUP WHO ARE:*					MEDIAN YEARS OF SCHOOLING	MEDIAN YEARS AT PRESENT OR CLOSELY RELATED OCCUPATION BY AGE		MEDIAN WEEKLY EARNINGS, 1952†
	Total‡	Single	Married with Children under 18	Non-white	Under 35 Years of Age		18-44 Years of Age	45 Years and Over	
Total.....	501	18	41	11	34	10.9	6.4	25.7	\$60
<i>Attitude to present job:</i>									
Likes present job—mentions nature of work.....	323	17	41	8	33	11.6	7.3	26.1	64
Likes present job—mentions other reasons.....	128	20	40	15	34	9.6	5.4	23.0	56
Likes and dislikes various aspects or dislikes present job.....	50	22	40	16	40	10.2	3.4	13.1	49
<i>Original vocational plans:</i>									
Same as present work.....	94	22	38	4	36	12.3	9.3	33.2	72
No plans, general plans, unknown....	205	19	37	11	32	9.9	5.2	22.0	59
Different from present work§.....	202	18	43	13	35	11.1	6.2	19.5	56
<i>If starting again:</i>									
Would go into same work as present..	154	17	36	6	32	12.0	7.9	36.1	71
Would have only general plans, no plans.....	139	20	43	8	37	10.0	4.9	16.4	55
Would go into different work§.....	208	18	43	16	32	10.7	6.4	22.2	57

* The percentages in each column should be compared with that for the total in the top row.

† Excludes 16 persons not reporting on attitude to present job (mostly unemployed).

‡ For 423 wage and salary workers reporting earnings.

§ Different occupation or industry specified.

self-employment reflect business failures or insufficient capital, the inability to "make a living" or a dislike of the hours and working conditions required to make a living that way. The latter point is also mentioned as a reason for dislike of jobs held in 1952 by some self-employed workers. As one Lithuanian groceryman expressed it, "The grocery business is slavery." We agree with Morse and Weiss that one reason for the emphasis on the desirability of self-employment is that it does not necessarily require additional educational qualifications, and, if the capital were available (implicit in the questions asked in the Michigan survey), it would be the most likely choice of manual workers.

We do not know what workers anticipate in self-employment, but one of the apparent advantages is the desire to be "independent" or to work at one's own pace. Some housewives, for example, note this as a reason for preferring homemaking responsibilities to paid jobs. Certain types of selling jobs offer this advantage, and, in factories, repair mechanics often note that "I am my own boss and make up my own schedule" and that "I plan what I can do." This sense of working at one's own pace is also found in some driver-salesman and truck-driving jobs, which generally appeal to men who dislike the discipline of a factory and prefer "outside work." As a former factory worker, who had been employed as a truck-loader since 1944, said, "It's outside work mostly—you can do what you want—holler or sing—you're not closed in." For every mechanic or craftsman who finds a factory job "fascinating," "always something new," there are others who find it monotonous and would like to control their own pace. While these findings support some of Chinoy's and

Guest's observations about workers on automobile-assembly lines, their incidence in a labor force varies with the occupational and industrial structure of employment in the community.

VOCATIONAL ADVICE TO CHILDREN BY PARENTS

Asked a hypothetical question about the vocational advice they would give their sons or daughters, workers in the sample divide almost equally between those who say they would give only general advice or none at all and those who would suggest specific occupations or types of work. Thirty-one per cent of workers would leave it to the child to decide, and various sage comments are made on the importance of knowing a child's aptitudes or preferences. This group includes a few who feel that they had been subjected to undue influence by their own parents and still resent it. In 17 per cent of the cases any advice given would be general in nature, such as urging the desirability of securing as much formal education or specialized training as possible. Twenty-seven per cent would advise their children to go into specific occupations differing from their own. Nine per cent would advise going into a specific occupation which appeared on their own work record, and 16 per cent would suggest apparently unrealized aspirations. While the transfer from parents to children of unrealized aspirations to education or work experience is reflected in these results, it is noteworthy that almost a third of the total make very positive statements about the desirability of leaving such decisions to the young people concerned.

The desirability of formal education is rather generally emphasized by Norristown workers. Not only does it appear as a form of vocational advice to children; it is also suggested as a way of achieving the respondents' own vocational aspirations and of "getting ahead" or being successful. Sixty per cent of the total mention the importance of education in one or both of these connections.¹⁰ Only a few state that education is *not* important and that it is experience, per-

¹⁰ The average level of schooling in Norristown is not low. Residents in the Norristown sample who were twenty-five years of age or over in 1952 had completed 10.6 years of schooling on the average. Comparable figures in 1950 for the city of Philadelphia, the Philadelphia Metropolitan Area, and the nation were 9.0, 9.5, and 9.3, respectively (*1950 Census of Population, U.S. Summary: Detailed Characteristics of the Population* [Bulletin P-C 1], pp. 1-236, 448-49).

sonality, or some other qualification that counts.

STRENGTH OF WORK ATTACHMENTS

The answers to questions about liking jobs and vocational aspirations may also be viewed as possible measures of the strength of current work attachments. The high degree of positive response on liking current jobs does not provide a good measure because too many persons change jobs even if they like what they are doing at a particular point in time. A priori, one might assume

when they start, and others have plans which for various combinations of reasons are not realized. The amount of occupational "choice" exercised in work careers thus appears relatively limited. Yet many workers stay in the same or a closely related occupation, in one industry, or with one employer for substantial parts of their working lives. One cannot infer that all such workers are "trapped" in their occupations, because too many state that they like the work (Table 4).

Preliminary findings on promotion are

TABLE 5
PERCENTAGE DISTRIBUTION OF AGE OF STARTING PRESENT OR
RELATED OCCUPATION, BY AGE AND BY
ATTITUDE TO JOB

AGE OF STARTING PRESENT OR CLOSELY RELATED OCCUPATION*	TOTAL 35 TO 64 YEARS OF AGE	PRESENT JOB LIKED BEST One Job Only†	PRESENT JOB LIKED BEST More than One Job	PREVIOUS JOB LIKED BEST, PRESENT JOB DISLIKED, AND OTHER‡
Total				
No.	299	34	178	87
Per cent.	100	100	100	100
Under 25.	32	52	37	14
25-34.	29	15	31	31
35-44.	24	18	21	35
45-64.	15	15	11	20

* For workers with interruptions for other types of work or time out of the labor force, age was computed for date of starting last continuous block of time at present or a closely related occupation. The major exception was that time spent in military service for those who returned to the same job or occupation was included in the computation of age at starting.

† Respondents' interpretations accepted, except in a few cases where workers' records showed employment at different kinds of work for more than one employer.

‡ Includes 9 workers who like nature of work but dislike certain aspects of present job (such as location or shift) and 6 workers with uncodable response.

that workers who mention the nature of the work as a reason for liking current jobs might have strong occupational attachments and would wish to go into this kind of work if starting again. But, apparently, more of this group would revert to their original vocational plans and go into a different kind of work if they were starting over. Nor do original vocational plans afford much assistance as predictors of future types of work for a community's labor force, since only 19 per cent of the total are in the same kinds of work as originally planned.

It is clear from the evidence that no meaningful index of attachments to present work can be derived from these responses. Many workers have no vocational plans

that relatively few individuals in Norristown succeeded in moving up the occupational ladder from the bottom to the top, although larger numbers moved from one level of skill to another in the course of a lifetime. A substantial number of workers, however, remained at the same level of skill, although they may have changed employers or job assignments and enjoyed advancement in the form of increased earnings.

While the specific occupation or general level of skill attained may be the most important influence in the character and degree of work attachments in the labor force, the age of starting in work is correlated with attitudes to the job (Table 5). Three-fifths of all workers who were thirty-five to sixty-

four years of age in 1952 had started a continuous period of time at their current types of work by age thirty-five. For those with positive attitudes to current jobs, however, establishment in a career by age thirty-five is more clear cut. This evidence is by no means conclusive, since the sample is too small to control further for age or for level of skill, but it suggests that attitudes to jobs need to be tested in the context of work careers and life-cycle influences. Nor do the findings tell us whether workers tend to remain in jobs they like or whether those who stick long enough with a job or type of work learn to like it. New data and possibly new analytical techniques are needed to answer these questions.

No single set of schedule questions on attitudes to jobs, occupations, or vocational aspirations yet devised has succeeded in providing measures that have much predictive value for occupational attachments. This is true of the Norristown survey, the data of which raise as many questions as they answer. What is clear from the evidence is that the vast majority of workers like their current jobs, but, judging from their past experience, many are likely to change jobs in the future. Relatively few workers have realized their original vocational aspirations, although they may persist during their lifetime and, in some instances, be transferred to their children. For the majority of work-

ers there is little relationship between the presence or absence of vocational aspirations and the number of years spent in a specific occupation or type of work. Perhaps the answers lie in different approaches to workers' attitudes and in improved knowledge of labor markets, the institutional framework of work career patterns, and the transferability of skills or experience, as revealed in work records.

To the extent that relatively sharp differences at varying ages reflect changes in the attitudes of individuals through the life-cycle, important breaks occur among Norristown workers around ages thirty-five and sixty-five. Several types of evidence suggest that work attachments and expectations as to achievement in a work career are fairly well established by the age of thirty-five. Above that age there is more concern with economic security. While neither of these concerns ever completely disappears, there is evidence of some realignment of the relative importance of economic and non-economic goals around the age of sixty-five. Relatively little is known about the lifetime processes of adaptation to the labor market and to ways of earning a living and concomitant changes in value-judgments or expectations and goals in life. This would appear to be a challenging field of research for students of the sociology of work as well as of the labor market.

UNIVERSITY OF PENNSYLVANIA

THE EVALUATION OF WORK BY FEMALES, 1940-50¹

SANFORD M. DORNBUSCH AND DAVID M. HEER

ABSTRACT

Ecological correlations were computed between level of income and female participation in the labor force. Supporting the hypothesis that white females were changing their evaluation of work, the previous negative correlations for white women now tended to be positive. For non-white women the same tendency was beginning to be found, particularly among young adults. Thus the values of non-white women apparently are following the same pattern of change as those of white women but with a time lag of more than a decade.

In a previous paper it was demonstrated that the universal negative correlations observed between participation of women in the labor force and the level of income in a city were not true for white females eighteen to nineteen and twenty to twenty-four in 1940.² The latter group showed a significant positive correlation, this being the first time this had been found in any group of supplementary workers. The conclusion of the paper stated:

White women now often work by choice in the period between school and marriage or childbirth, and northern cities provide many suitable positions. The changing definition of work may explain the positive association for young white females. The different patterns for non-white females may reflect a divergence in values or a lack of job opportunities. Regardless of the underlying explanation, the labor economists must necessarily revise their theory to place it within a social setting.³

This paper seeks to explore further the tentative explanation of the 1940 findings; namely, that a change in the evaluation of work may be occurring among women. Any

such trend should be reflected in 1950 in the participation of women in the labor force. Accordingly, it was hypothesized that the correlations between labor-force participation and income should be even more positive in 1950 and extend to more age groups than in 1940.

The 1940 Census was taken toward the end of the depression, while the 1950 data reflect a period of economic prosperity. With jobs available for most persons desiring to work, the 1950 experience more directly reflects the evaluation of work by females. It thus provides a good test of the *post hoc* explanation of the 1940 findings.

The 1950 Census gives rate of participation in the labor force by race only for those cities and Standard Metropolitan Areas (S.M.A.) containing 50,000 or more non-white population. Thus the new study is based on the figures for the 27 cities and 39 S.M.A.'s containing 50,000 or more non-white inhabitants. The previous study was based on 50 cities. The use of S.M.A. data makes it possible for the first time to determine whether the inclusion of suburbs changes the relationship between median income of the area and participation in the labor force.

Because of the fact that a racial breakdown is not provided unless the non-white population is 50,000 or more, the present sample of cities and S.M.A.'s is biased. Sixty-four per cent of the S.M.A.'s included in the present study are located in the South. By way of contrast, only 36.5 per cent of the total number of S.M.A.'s in the

¹ This research was sponsored by *Fortune* magazine and the Laboratory of Social Relations, Harvard University. A preliminary statement of the major findings, without supporting material, appeared in Daniel Bell, "The Great Back-to-Work Movement," *Fortune*, LIV (July, 1956), 92-93.

² Sanford M. Dornbusch, "Correlations between Income and Labor-Force Participation by Race," *American Journal of Sociology*, LXI (January, 1956), 340-44.

³ *Ibid.*, p. 344.

continental United States as large or larger than the smallest S.M.A. included in this study are in the South. Furthermore, while 48 per cent of the cities included in this study are in the South, only 32 per cent of the total number of American cities as large or larger than the smallest city in the sample are there. This bias is no greater than in preceding studies. For example, the 1940 study had 46 per cent of its cities in the South, while the unbiased proportion would have been only 24 per cent.

In the 1940 Census, data on income were not classified by race. Therefore, in the ear-

advantage provided by the racial breakdown of income.⁵

More detailed age groupings are used in this study than in the 1940 paper. Greater specificity of age groups now seems desirable in the light of the age differentials found in 1940. Because of this interest in particular age groups, there is no need for the use of a standardization process to provide correlations for the total of all ages.

The conclusions of the present study are based on three sets of correlations. In the first set, rates for white females' participation in 1950 for various age groups are cor-

TABLE 1
KENDALL'S TAU OF 1949 MEDIAN MALE INCOME AND 1950 FEMALE LABOR-FORCE
PARTICIPATION RATES BY AGE AND BY RACE FOR
39 S.M.A.'S AND 27 CITIES

AGE GROUP	S.M.A.'s			CITIES		
	White	Non-white	All Races	White	Non-white	All Races
14.....	.18	-.29	-.13	.20	-.07	.27
15.....	.16	-.50	-.12	.29	-.25	.16
16.....	.05	-.55	-.02	-.02	-.34	-.01
17.....	.06	-.51	.07	.01	-.37	.05
18.....	.27	-.05	.47	.10	.03	.33
19.....	.33	.06	.50	.10	.09	.44
20-24.....	.34	.08	.41	.14	.13	.27
25-29.....	.22	-.12	-.03	.19	-.20	.03
30-34.....	-.06	-.20	-.31	.08	-.22	-.16
35-39.....	-.08	-.20	-.37	.10	-.23	-.11
40-44.....	-.003	-.22	-.32	.10	-.27	-.17
45-49.....	.12	-.31	-.22	.21	-.26	-.07
50-54.....	.18	-.29	-.18	.15	-.38	-.16
55-59.....	.18	-.38	-.12	.16	-.33	-.09
60-64.....	.18	-.20	.06	.12	-.52	-.07

lier study, median annual income of all experienced male workers who worked twelve months in 1939 was related to the participation of total females in the labor force and of white females and non-white females. The use of male income prevented circular reasoning.⁴ The current study has the advantage of having data on male income by race. Accordingly, for the first time the measure of income is appropriate to the group whose participation in the labor force is being investigated. The only deficiency of the 1949 data on male income by race is that it is not limited to men who worked twelve months. This inadequacy is not of sufficient importance to counterbalance the

related with the median income in 1949 of white males, having income, in each of 27 cities and 39 S.M.A.'s. In the second set, the participation of non-white females is correlated with the median income of Negro males with income. Finally, in the third set, the rates of total female participation are correlated with the median income of males of all races (see Table 1). The measure of correlation employed in the present study, and also in the previous study, is a measure

⁵ The rates of female participation in the labor force by age and by race are found in *Census of Population: 1950*, Vol. II: *Characteristics of the Population*, Table 66. The data on median income of males with income subclassified by race are found in Table 87 of the same volume.

⁴ *Ibid.*, p. 341.

of rank correlation developed by Kendall, tau.⁶

Whereas in 1940 only two age groups among white women showed positive correlations, with the others negative, there is not a single strong negative correlation for white women in 1950. This is true whether the analysis is done for S.M.A. or city. In addition, the correlation for every age group is more positive in 1950 than in 1940.

Among non-white women the change is no less striking. The earlier pattern of universal negative correlations now breaks down for non-white women eighteen, nineteen, and twenty to twenty-four. It is surely not coincidental that these are the very age groups which were strongly positive among white women in 1940. It is as if the pattern of changing values first appears among young adult women, later spreading to other ages. The full-time employment pattern in 1950 provides no basis for emphasis on lack of job opportunities as the explanation of the white-non-white differential. Non-white women are, therefore, following their white sisters in the changing evaluation of work, but with a time lag.

Non-white women still hold to the older

values more tenaciously than white women. For every age group the non-white female correlation is more negative than the white correlation. Added to this is the fact that the correlation for non-white young adult females moved toward zero rather than becoming positive. Then, barring another depression which might reverse the present trend, a clear prediction for 1960 is indicated. The non-white female correlations of participation in the labor force and income will become more positive for most of the age groups. They will not, however, become strongly positive. Rather they will move by 1960 toward no correlation between income and participation in the labor force.

With divergent samples and changed economic conditions it is obviously difficult to isolate the crucial variables in this change in the pattern of participation in the labor force. The consistency of findings in the study of 1940 and 1950 is a powerful argument for the plausibility of the explanation advanced here, but a large-scale correlational study such as this cannot go as far to explain the situation as a set of individual work histories. We hope that eventually such detailed analyses will test this hypothesis.

HARVARD UNIVERSITY

⁶ Maurice J. Kendall, *Rank Correlation Methods* (London: Griffin & Co., 1948).

ECONOMIC MOBILITY AND SOCIAL STRATIFICATION

GABRIEL KOLKO

ABSTRACT

Since 1929 the economic mobility of selected occupations in the income deciles, based on their average annual incomes, has remained relatively stable. The highest income decile is still composed primarily of professionals, managers, and businessmen, though a larger proportion of clerical workers can today be found in the lowest-income categories. Because of the wide inequalities in income between the deciles, symbols of economic status have not been equitably distributed. Thus the widely held image of America as an increasingly middle-class society based on a general distribution of high economic status symbols finds little justification.

During the 1930's the social and political ramifications of the changing economic status of the American white-collar class evoked large-scale theoretical generalizations from social theorists. In 1934 Hans Speier suggested that the white-collar worker was being proletarianized.¹ A year later, Lewis Corey, in *The Crisis of the Middle Class*, prophesized that the economic unification of the blue- and the white-collar worker would precipitate political unity, leading toward the achievement of a Marxist society. Since World War II, however, most students of mobility in the United States have shifted their concern to investigations of individual or family mobility between occupational classes or have accepted the assertion that, as a result of a post-New Deal income redistribution, the economic mobility of the white-collar class has been not so much downward as the economic position of the other classes has been upward.² Thus, consumption and economic status have extended to the masses as more people in more occupations gain access to high economic status symbols. A view which once concentrated on status and economic disequilibrium and poverty among classes and the consequent potential for radical social change has been widely rejected in favor of

the view that a high level of prosperity is shared by most workers in a full-employment economy and that no new major social tensions can be anticipated as a result of the economic decline of classes.

Several years ago, Hatt and Foote maintained that in recent years "net occupational movement has been towards the jobs of higher prestige."³ In part this was due to the attempt in many occupations to professionalize jobs by changing titles so as to secure prestige: "janitors" and "caretakers" thus become "building engineers." Probably more important was Hatt and Foote's thesis that rising prestige entails higher income.

Given the existing data, what can we legitimately assert about the economic position of specific occupations and the broad occupational classes in relation to one another since the depression and the New Deal? Is the average ditch-digger, for example, in the same income decile—the same tenth of the income recipients—in 1949 as he was in 1939, a year of high unemployment? Further, has the economic position of the clerical, skilled, and other occupational classes become equalized, along with the mass lifestyles which income equalization makes possible?

In a recent study by Herman Miller, of the Bureau of the Census, 118 occupations, employing about three-quarters of the American males earning \$100 or more in 1940, are ranked in income deciles on the basis of their

¹ Hans Speier, "The Salaried Employee in Modern Society," *Social Research*, February, 1934.

² See, e.g., the recent work of W. Lloyd Warner and Natalie Rogoff. For a critical study of the income redistribution thesis see this writer's "The American 'Income Revolution,'" *Dissent*, Winter, 1957, pp. 35-55.

³ Nelson N. Foote and Paul K. Hatt, "Social Mobility and Economic Advancement," *American Economic Review*, XLIII (May, 1953), 370-71.

median incomes in 1939 and 1949. Self-employed male workers and workers receiving large amounts of income in kind are excluded by Miller, and his decile placements take no account of income from dividends, rent, interest, etc., usually going to certain occupations.⁴

From Miller's data, of the 118 occupations listed, 56 moved into different income deciles in 1949 as compared to 1939, 25 going downward and 31 upward. Yet, save for 15 occupations, none moved more than one decile, up or down. Of these 15, 9 moved down and 6 moved up the income scale. An even more detailed breakdown reveals that, among the 14 major professional and semi-professional occupations listed, 4 moved upward and 1 downward. Among the 10 salaried managers and officials listed, 2 moved up and 2 moved down by one decile. Three of the eleven listed clerical and sales occupations moved downward by one decile. Of the 28 skilled-worker occupations listed, 14 moved upward (4 by two deciles), and only 4 moved downward. Of the 26 listed semiskilled workers, 6 moved up and 6 moved down the income scale. Among the 19 listed unskilled workers, 5 moved up and 4 down. Most significant of all was that, among the 10 listed service occupations, 5 were downwardly mobile by at least two deciles while none moved up. In 1949, as in 1939, the highest three income deciles generally consisted of professional, managerial, clerical, sales, and skilled workers, though not all clerical and skilled workers, by any means, were in the highest-income third. The major change in economic mobility has been among service workers, who are rapidly moving downward, and to a lesser extent among upwardly mobile skilled workers. Despite their slight downward mobility, male white-collar workers generally remained among the highest three-tenths of the income-earners, and the lowest three-tenths were still composed of service, unskilled, and semiskilled workers.

Among the various broad occupational

⁴ Herman P. Miller, *Income of the American People* (New York: Wiley, 1955), pp. 55-57, 166.

classes it would seem it is still true, in economic terms at least, as Emil Lederer asserted over forty years ago, that

vertically, salaried employees are still more differentiated than wage earners. Although the economic, social and cultural distance between the unskilled, irregularly employed hourly laborer at the bottom of the proletarian pyramid and the skilled, highly qualified, organized, steadily employed worker are enormous, they cannot be compared with the gulf separating a clerk in a country store from an officer in a banking institution.⁵

In 1935-36, 2.2 per cent of the non-relief clerical workers fell in about the lowest fifth of the consumer units as compared to 10.5 per cent among the "wage-earning" group (skilled, semiskilled, unskilled and service workers). Similarly, about 21 per cent of the non-relief clerical, 35 per cent of the business, and 38 per cent of the professional workers qualified for the highest-income decile, while only 7 per cent of the non-relief wage-earning group qualified.⁶ In 1954, with blue-collar labor in a more advantageous position because wage rates kept pace with inflation through collective bargaining and there was full employment, only 2.6 per cent of the clerical and 13.5 per cent of the sales workers qualified for the highest 14 per cent of the spending units (\$7,500 and up), while 6.5 per cent of the skilled workers, 2.2 per cent of the semiskilled, and 0.5 per cent of the non-farm and mine laborers and service workers qualified. Thirty-four per cent of the professional, 31 per cent of the managerial, and 37 per cent of self-employed workers remained among this top group. While it would thus appear that nearly the same ratio between the clerical and manual groups has remained while both are economically downwardly mobile, nevertheless the percentage of manual workers, as a whole, in

⁵ Emil Lederer, *The Problem of the Modern Salaried Employee: Its Theoretical and Statistical Basis* (New York: Columbia University Press, 1937), pp. 4-5.

⁶ National Resources Committee, *Consumer Incomes in the United States* (Washington, D.C., 1938), pp. 26 and 95.

the lowest fifth of the spending units has remained relatively stable despite full employment, while a very substantial sector of the clerical and sales workers has become depressed; in 1955, 15 per cent of the clerical and sales workers could be found among the lowest 23 per cent of the spending units (\$2,000 or less), whereas only 9.5 per cent of the skilled, 17 per cent of the semiskilled, 27 per cent of the service, but 37 per cent of the non-farm and mine laborers could be found there.⁷

Because of seasonal unemployment, strikes, illness, etc., few attempts have been made to compute the average annual earnings of non-professional workers in specific occupations. Harold Clark, in his pioneering study of *Life Earnings in Selected Occupations in the United States*, adopted a technique for computation which a cross-check with Census data convinces me is as accurate as can be hoped for at the present time.⁸ For periods of relatively full employment he multiplied average weekly wages by 51 and deducted 10 per cent. This procedure is valid only for blue-collar labor, which is much more affected by unemployment than are other occupational groups. Pursuing Clark's procedure for the depression period, I have deducted 20 per cent. Reliable data on the annual incomes of certain professional and all railroad workers are available since 1929, as are data on the average annual income for all occupations in 1939 and 1949. In addition, information on the lowest and highest income received by each decile of the nation's consumer units in 1918, 1929, 1935-36, and all the post-World War II years is available. By placing the average annual salary or wage income of each occupation in its appropriate decile for each year, it is possible to examine the trends there in economic mobility (Table 1). While the income ranges are based on income from dividends,

⁷ Data taken from *Federal Reserve Bulletin*, June, 1955, p. 615, and Bureau of the Census, *Statistical Abstract of the United States, 1956* (Washington, D.C., 1956), pp. 312-13.

⁸ Harold F. Clark, *Life Earnings in Selected Occupations in the United States* (New York: Macmillan Co., 1937), pp. 132-33.

interest, rent, etc., as well as salaries and wages, enough is known about ownership of these assets to indicate that they assume proportions of significance almost exclusively among the occupations already in the highest-income decile because of wages and salaries alone. In 1952, for example, 44.8 per cent of the high administrative executives, 12.8 per cent of the professionals, 19.4 per cent of the operating supervisory officials (managers, department heads, etc.), and 11.2 per cent of the sales personnel owned stocks in any amount, while only 7.6 per cent of the clerical workers, 4.4 per cent of the skilled workers, 1.4 per cent of the semiskilled workers, and 0.2 per cent of the unskilled workers owned any stock.⁹ In early 1955, for example, less than one-half of 1 per cent of the clerical, sales, skilled, semiskilled, unskilled, and service workers owned more than \$25,000 worth of stock, whereas 5 per cent of the managerial and 2 per cent of the professional workers owned stocks in amounts of \$25,000 and up.¹⁰

Ignoring the important fact of income inequalities within the broad occupational classes discussed earlier in this paper, Table 2 illustrates the relative median income position of the major occupational classes since 1939.

It should be noted that the occupational classes most susceptible to unemployment generally showed the greatest percentage increases in their incomes between 1939 and 1954, thus somewhat narrowing but by no means eliminating the gaps in income among manual and white-collar groups. This equalizing trend, which also occurred during World War I, can by no means be thought of as an economic factor now independent of the business and employment cycle.¹¹

Generally, the downwardly mobile occu-

⁹ Lewis K. Kimmel, *Share Ownership in the United States* (Washington, D.C.: Brookings Institution, 1952), p. 98.

¹⁰ *Federal Reserve Bulletin*, June, 1955, p. 622.

¹¹ For a more extensive development of this viewpoint see Philip W. Bell, "Cyclical Variations and Trends in Occupational Wage Differentials in American Industry since 1914," *Review of Economics and Statistics*, November, 1951, pp. 329-37.

TABLE 1

POSITION OF SELECTED OCCUPATIONS IN INCOME DECILES, 1918-52
ON BASIS OF AVERAGE ANNUAL INCOMES*

Occupation	1918	1929	1935-36	1949	1951	1952
Lawyers.....		10	10	10	10	
Physicians.....		10	10	10	10	
Dentists.....		9+	9+/10	10	10	
Full professors (large state university)...	10	9+	9+/10			9++
Associate professors (large state university).....	9	9-	10			9-
High-school principals†.....		10	10	10	10	
High-school teachers†.....		8+	10	9-	8+	
Elementary-school teachers†.....		7	9-/9	8	7	
High-school principals‡.....		9	10	9+	9	
High-school teachers‡.....		7-	8+	7	7-	
Elementary-school teachers‡.....		5	7	6	6-	
Social workers.....		6	8	6-		
Ministers.....		7	8-	5		
Nurses.....		5-	5-	5		
Engineers.....		10	10	9		
Railroad engineers (skilled).....	10	9-	10	10	9+/10	9+/10
Railroad firemen (skilled).....	8+/9-	7+/8-	9	9-	8+/9-	9-
Railroad conductors (managerial).....	9+	8+	10	9+/10	9/9+	9/9+
Railroad baggagemen (clerical).....	8	7+	9	9	9-/9	9-
Railroad brakemen and flagmen (semi-skilled).....	8	7+	8+/9-	8+	8+/9-	8+/9-
Railroad clerical and station employees.....		5	8-	7-	7-	6+
Railroad maintenance-of-way employees (unskilled).....		3-	5	6-	5+	5+
Unskilled workers (by industry):						
Automobile.....		4	5+	5+		
Chemical.....		4	4+	5		
Machine and machine tools.....		3	4	5-		
Furniture.....		3-	3	3		
Iron and steel.....		4	3	5-		
Paper and pulp.....		3	3+	5-		
Semiskilled and skilled workers (by industry):§						
Automobile.....		5	6+	6		
Chemical.....		4+	6-	6		
Machine and machine tools.....		5-	6	6/6+		
Furniture.....		4	5+	4-		
Iron and steel.....		6-	5+	6-		
Paper and pulp.....		5	5+	6-		

* Income deciles are ranked as follows: 10 equals the highest decile, 8 equals third highest decile, 1 the lowest decile, etc. A plus or minus sign indicates the highest or lowest third within the decile.

Data for lawyers, physicians, and dentists are taken from *Statistical Abstract, 1956*, Chart 380; data for educators are taken from Beardsley Ruml and Stanley G. Tickton, *Teaching Salaries Then and Now* (New York: Fund for the Advancement of Education, 1955); data for the other professions are taken from Clark, *op. cit.*, and from Census data given in Miller, *op. cit.*; railroad data are from Harry E. Jones, *Railroad Wages and Labor Relations—1900-1952* (New York: Bureau of Information of the Eastern Railways, 1953); data on workers' income by industry taken from M. Ada Beney, *Wages, Hours, and Employment in the United States, 1914-1936* (New York: National Industrial Conference Board, 1936), and data for male workers given in Miller, *op. cit.*, pp. 173-76.

Decile income range for 1918 is computed from data given in Wesley C. Mitchell *et al.*, *Income in the United States, 1909-1919* (New York: Harcourt, Brace & Co., 1921), I, 132-35.

For 1929, data are from Maurice Levin *et al.*, *America's Capacity To Consume* (Washington, D.C.: Brookings Institution, 1934), p. 96.

† Cities of over 500,000.

‡ Cities of 30,000-100,000.

§ Average of combined incomes. Such averages are open to question as to their reliability in showing mobility, but lack of better data demands they be made.

pations have shown no decline in their proportion of the labor force. Service workers, with a higher share of downwardly mobile occupations than any other occupational class, grew from 9.6 per cent of the work force in 1910 to 10.1 per cent in 1950.¹² Clerical and sales workers, with a large section of downwardly mobile occupations, grew from 10.5 per cent of the work force in 1910 to 19.3 in 1950. Non-farm and mine laborers, a generally upwardly mobile class still in the lowest-income deciles, declined from 11.6 per cent

from 11.7 per cent in 1910 to 13.8 per cent in 1950. Semiskilled workers, having about an equal amount of upward and downward mobility, grew from 14.1 per cent of the work force in 1910 to 21.1 per cent in 1950.

It is not, therefore, true, as Foote and Hatt claim, that "net occupational movement has been towards the jobs of higher prestige. . . . There can be no doubt that expanding occupations on the whole are those of higher prestige levels."¹³ Certainly if income and economic position are used as the criteria of prestige, a thesis finding support from Max Weber to Émile Benoit-Smullyan, the optimism represented by Foote and Hatt has no empirical justification. The rapid growth of the clerical and sales occupations, with their important economically declining groups, the mixed picture among the rapidly expanding semiskilled workers, the economically declining occupations among service workers, and the mixed picture among laborers more than offset the upward mobility among skilled and professional workers. Taken as a group, all the downwardly mobile occupations from Miller's table maintained almost their identical percentage of the American work force in 1949 as in 1939, and the same is true for the rising occupations.

Yet the highly paid locomotive engineer, for example, can consolidate his economic status in a far more noticeable manner than most clerical and sales workers, for prestige in our acquisitive society is still very much based on the ability to exhibit one's economic wealth, with sophistication or without. Status and prestige can be dependent to a significant degree on assumed non-economic qualities, such as education, function, etc., but Benoit-Smullyan's concept of "status equilibration"—that one's prestige and political power eventually reach a level corresponding to his economic position—is reasonable in the light of present knowledge of buying habits among occupational groups. Certainly we know that the occupations with high prestige are also those with high income. Of the 90 occupations evaluated for

TABLE 2

INDEX OF RELATIVE MEDIAN WAGE OR SALARY
INCOME OF THE MALE EXPERIENCED CIVILIAN
LABOR FORCE, BY OCCUPATIONAL CLASS,
1939 AND 1954*

Occupation	1939 (100 = 1939 Clerical Wage)	1954 (100 = 1954 Clerical Wage)
Professional, technical, and kindred workers	127	131
Managers, officials, and proprietors, except farm Farmers and farm mana- gers	150	140
Clerical and kindred work- ers	26	35
Sales workers	100	100
Craftsmen, foremen, and kindred workers	91	102
Operatives and kindred workers	92	114
Service workers, except private household	72	90
Farm laborers and foremen Laborers, except farm and mine	59	75
	22	25
	47	63

* Computed from data in Bureau of the Census, *Current Population Reports, Consumer Income*, Series P-60, October, 1955, p. 24.

1935-36 facts are from Bureau of Labor Statistics, *Handbook of Labor Statistics* (Washington, D.C., 1942), I, 125.

For 1949 all postwar income ranges from *Federal Reserve Bulletin*, July, 1954, p. 700.

in 1910 to 6.1 per cent in 1950, while all farm occupations, generally very low paid, continued their historic decreases. The rising professions grew from 4.6 per cent of the work force in 1910 to 8.7 per cent in 1950. The generally rising skilled workers grew

¹² All statistics from Gladys L. Palmer and Ann R. Miller, "The Occupational and Industrial Distribution of Employment, 1910-50," in *Manpower in the United States*, ed. William Haber et al. (New York: Harper & Bros., 1954), p. 87.

¹³ Foote and Hatt, *op. cit.*, p. 370-71.

prestige in a survey by the National Opinion Research Center, specific income information is available for 55 of them.¹⁴ Of the 41 occupations having a high prestige ranking of 75 or above, average annual income data for 1949 is available for 28 of them. Of these, 21 fell in the highest income decile, four in the second highest decile, and only three in lower deciles. Because of the strong weighting on the part of the poll's directors, 23 of the 24 occupations with highest prestige were in the professions or management, the sole exception being a skilled worker, the railroad engineer. Of the 27 occupations earning prestige scores under 75 for which I have income information, only 6 were in the second highest decile and 4 in the third highest decile, the remainder falling in the lower groups. Two of the 6 skilled occupations listed ranked higher in prestige than any of the clerical and sales occupations.

Considering the occupational composition of the highest income decile, there seems to have been only slight change from 1939 to 1949. Of the 10 occupations with the highest average salary and wage incomes in 1939, 9 were still among the highest paid 10 in 1949, while 1 ranked eleventh. The 2 highest paid occupations in 1939, salaried managers and officials in manufacturing and finance, insurance, and real estate, were also the highest paid in 1949.

In 1939 it required an average of \$1,333 to maintain a family of four at the Bureau of Labor Statistics (BLS) "health and decency" standard.¹⁵ The budget must obviously be reduced for smaller families and raised for larger (60 per cent of the households in 1950 were composed of three persons or less), but it nevertheless serves as a good index to economic standards afforded by average annual incomes in the United States, exclusive of credit spending. In 1939,

of the 94 male clerical, sales, skilled, semi-skilled, service, and unskilled occupations listed, 63 reported an average income of less than the BLS minimum of \$1,333.¹⁶ Only 3 of the 11 clerical workers fell below this level, while 11 of 28 skilled occupations reported average incomes of less than \$1,333. The remaining occupational classes fell almost entirely below this level. In 1949 the BLS level, affording essentially the same prewar standard of living qualitatively and quantitatively with the addition of a six- to nine-year-old car for nearly three-quarters of those receiving the requisite income, required a national average of \$3,608.¹⁷ In 1949, male workers in 84 or the same 94 occupations earned an average annual wage and salary income of less than \$3,608, thus including almost all the above-mentioned occupational classes about equally. Even the income dividing the highest paid quarter of each of the 94 occupations left from the lower three-quarters 36 below the BLS level in 1939, but 58 in 1949. In both instances clerical, sales, and skilled workers were naturally less affected than semiskilled, unskilled, and service workers. "Prosperity," therefore, has been substantially dependent on the radical increase of working wives and credit since World War II.

The quantitative difference in income between the income deciles undermines the possibility of mass adoption of the major items of living and life-styles involving economic status symbols. In 1918 the lowest income needed to qualify for the highest income decile was four times greater than the largest income within the lowest income decile, but since 1929 it has been about seven and one-half times greater. Since World War II the average dollar income of the highest decile has been from twenty-five to thirty times greater than the average dollar income of the lowest decile; and since 1918 the lowest dollar income of the richest decile has

¹⁴ National Opinion Research Center, "Jobs and Occupations: A Popular Evaluation," in *Class, Status and Power*, ed. Reinhard Bendix and Seymour M. Lipset (Glencoe, Ill.: Free Press, 1953), pp. 411-26.

¹⁵ *Monthly Labor Review*, November, 1939, p. 1166.

¹⁶ Miller, *op. cit.*, pp. 173-86, provides the relevant data for calculations.

¹⁷ *Monthly Labor Review*, February, 1951, p. 153. For an extensive discussion of recent trends in standards of living see Kolko, *op. cit.*, pp. 46-51.

been from two to two and one-half times that of the dollar income dividing the lower half of the income recipients from the upper half. In the postwar period the average income of the highest decile has been about twice as high as the average income of the second highest. The urban family of four in 1949 not in the top three and five-tenths of the spending units was unable to maintain the BLS's minimum health and decency standard on their incomes, much less buy luxury goods made for the mass. In 1935-36 it was necessary to be in the highest four-tenths of the spending units to maintain this level, though in both cases credit is not taken into account.

Different occupations within the same income categories, it is true, have substantially differed in their expenditures in order to meet differing consumption needs according to their functions and attitudes. But, by and large, available data on consumption by income category indicate that equality of consumption (i.e., "massification") does not exist in any real sense in the United States today. The overriding limitation is income.

The automobile, which accounts for about half of the value of durable goods sales and is a major status symbol, has become a link in the massification thesis. Car ownership spread from about 55 per cent of the consumer units in 1935-36 to 59 per cent in 1941 and to 70 per cent in early 1956. Yet only 29 per cent of the 11 per cent of all consumer units earning less than \$1,000 in 1955 owned cars, while among the top 14 per cent of the consumer units (\$7,500 plus) 94 per cent were car owners. Between the two poles existed a situation reflecting income inequality—a similar situation to that in 1936 and 1941.¹⁸

But even among the automobile owners the value of the car is further structured according to income class, expensive highly

regarded cars worth \$2,000 or more in early 1955, for example, being overwhelmingly monopolized by those with incomes of \$7,500 and over. Fewer than 3 per cent of those earning less than \$5,000 in 1954 owned cars worth \$2,000 plus, while 9 per cent of those in the \$5,000-7,500 group and 29 per cent of those earning above \$7,500 owned cars in this category.¹⁹ In 1953, for example, members of the top 31 per cent of the spending units purchased 68 per cent of the new autos, the great majority of lower-income purchasers restricting themselves to cheaper used models.²⁰

Since a larger proportion of the clerical and sales workers than skilled and semi-skilled workers can be found in the lowest and highest income deciles, it is natural that net assets, liquid assets, consumption of goods, and debt among the clerical and sales workers should reflect their position in the income distribution range. But, despite the fact that, as Erich Engelhard once wrote, "[white-collar] employees do their utmost to preserve the accepted standard of living" in order to maintain prestige, the process of equilibration between economic position and styles of life is irresistible."²¹

One of the major causes of the significant rise of American consumption in the post-World War II period has been credit. Styles of life in America over the last twenty years have reflected not only the objective income position of the occupational classes and increases in real income but their willingness to exploit credit mechanisms in order to duplicate the standard of living idealized in the mass media. Generally, credit, full employment, and a higher average real income have meant greater consumption for more Americans than in the prewar period, though the extent of this change has been very much

¹⁸ *Federal Reserve Bulletin*, May, 1955, p. 477.

²⁰ *Federal Reserve Bulletin*, June, 1954, p. 581, and July, 1954, p. 701.

¹⁸ *Federal Reserve Bulletin*, June, 1956, p. 567, and August, 1956, p. 820. Also see National Resources Committee, *Family Expenditures in the United States* (Washington, D.C., 1941), pp. 1 and 4, and Bureau of Labor Statistics, *Family Spending and Saving in Wartime* (Washington, D.C., 1945), p. 86.

²¹ *The Salaried Employee* (New York: Columbia University, 1939), p. 58. For an extensive discussion of the theory of equilibration see Weber's "Class, Status, Party," and Émile Benoit-Smullyan, "Status, Status Types, and Status Interrelations," *American Sociological Review*, April, 1944.

exaggerated by most commentators. In 1935-36, 12 per cent of all farm families, 26 per cent of all professional, business, managerial, clerical, and salesworkers' families, and 30 per cent of all manual workers' families were indebted for instalment purchases, the frequency of debt for manual workers in the middle and highest income brackets being higher than in the equivalent income earners among other non-farm workers.²² By early 1956 all occupational groups had radically increased instalment debts, though now the manual occupations far outstrip other occupations. While only 18 per cent of the farm families owed instalment debts in early 1956, 44 per cent of the clerical and sales workers; 47 per cent of the professional, semiprofessional, and managerial workers; 56 per cent of the unskilled and service workers; and 62 per cent of the skilled and semiskilled workers were obligated for instalment purchases.²³ In early 1956, as well, the manual non-farm workers had committed themselves to greater fixed payments as a percentage of their disposable incomes than clerical and sales workers. Six per cent of the clerical and sales, 10 per cent of the skilled and semiskilled, and 14 per cent of the unskilled and service workers were committed to fixed payments amounting to 40 per cent or more of their disposable income, though the situation was more equal on lower percentages of fixed payments.²⁴

Less committed to the institution of credit than other occupational classes, the clerical and sales workers as a whole tend to save more. In every postwar year they have had fewer non-savers than either the skilled and semiskilled (taken together) and the unskilled and service workers' categories. In 1947, 6 per cent of the professional, 10 per cent of the managerial and self-employed, 12 per cent of the clerical and sales, 22 per cent

of the skilled and semiskilled, and 48 per cent of the unskilled and service workers were totally without liquid assets, while 42, 40, 23, 18, and 5 per cent of the same groups, respectively, held \$2,000 or more in liquid assets.²⁵ In 1955, by which time the over-all liquid assets holdings had rapidly deteriorated, 6 per cent of the professional, 7 per cent of the managerial, 13 per cent of the clerical and sales, 30 per cent of the skilled and semiskilled, and 51 per cent of the unskilled and service workers were without liquid assets, while 38, 41, 19, 14, and 11 per cent, respectively, held \$2,000 or more.²⁶ In early 1955, 12 per cent of the professional, 11 per cent of the managerial and 5 per cent of the clerical and sales workers owned stock valued from \$1,000 to \$25,000, whereas only 1 per cent of the manual non-farm workers owned stocks in such amounts.²⁷ Similarly, the net worth of clerical and sales workers was generally higher than that of manual laborers: 16 per cent of the professional, 17 per cent of the managerial, 42 per cent of the self-employed, and 5 per cent of the clerical and sales workers had a net worth of \$25,000 and up in 1953, while 3 per cent of the skilled and semiskilled and less than 1 per cent of the unskilled and service workers fell in this category. And, despite their concentration in lower-income brackets, there were substantially fewer clerical and sales workers than skilled and semiskilled workers with a negative net worth in 1953.²⁸

In homeownership the skilled and semiskilled workers (taken together) in the postwar period have barely edged past the clerical and sales workers, though the ownership rate among unskilled and service workers is about one-third less than both. But the value of the clerical and sales worker's home is significantly greater than any of the manual workers', in part because of the once advantageous prewar income position of most clerical and sales workers and the higher

²² Blanche Bernstein, *The Pattern of Consumer Debt, 1935-36* (New York: National Bureau of Economic Research, 1940), pp. 28-33.

²³ *Federal Reserve Bulletin*, July, 1956, pp. 702 and 706.

²⁴ *Ibid.*

²⁵ *Ibid.*, July, 1948, p. 770.

²⁶ *Ibid.*, June, 1955, p. 619.

²⁷ *Ibid.*, p. 622.

²⁸ *Statistical Abstract*, 1956, p. 307.

proportion in the top income deciles and also because the clerical and sales workers generally spend less for furniture and appliances than skilled and semiskilled workers. In 1950, 17 per cent of the professional, 10 per cent of the managerial, and 4 per cent of the clerical and sales workers owning their own homes valued them at \$20,000 and up, while only 1 per cent of the skilled and semiskilled, and less than 1 per cent of the unskilled and service workers owned homes of such value.²⁹ Thus the clerical and sales worker attempts to stake his claim to prestige with his house, and it is only in this area that his style of life is markedly higher than the "skilled and semiskilled" workers', though between the professional-managerial workers and clerical workers, as between the clerical and unskilled workers, the gap in life-styles, consumption, and income is very wide indeed.

But the generally greater average income of the skilled and semiskilled worker, as well as his greater willingness to utilize credit, taken together in a period of full employment invariably means that the economic symbols of status are no longer monopolized by the clerical, professional, and managerial workers. In 1949, 16 per cent of the professional, 19 per cent of the managerial and self-employed, and 10 per cent of the clerical and sales workers purchased new autos, while only 7 per cent of the skilled and semiskilled and 3 per cent of the unskilled and service workers purchased new cars. In 1955, 18 per cent of the professional, 22 per cent of managerial and self-employed, 15 per cent of the clerical and sales, 12 per cent of the skilled and semiskilled, and 4 per cent of the unskilled and service workers bought new cars. Within each of these classes it is probable that new-car purchasers were high-income earners who, economically at least, had more in common with each other than with those of their own class with lower incomes. But, for every new car purchased by a clerical worker, another purchased a used car, while two to three times as many skilled and semiskilled workers, and three to five

times as many unskilled and service workers, purchased used rather than new cars.³⁰

If one generalization can be made on the recent trends in the economic position of the broad occupational classes, it is that little supporting evidence can be found for the widely accepted notion that America over the past decade has developed a permanent economic middle class and an economically classless society. The income gap *within* each of the broad occupational classes is wide enough to in itself warrant the assertion that economic inequality is still very much an aspect of the American social structure. Nor do the somewhat narrower income differentials *between* the occupational classes demonstrate a basic change in the economic structure since the New Deal, for the changes that have occurred are historically associated with periods of full employment and inflation, a state of affairs one can hardly call permanent for any indefinite period. If certain of the clerical and sales occupations have shown a downward economic mobility, or certain of the skilled workers an upward mobility, it would be pure conjecture to declare that a stable state will be found at some middle ground and that dynamic changes in the income structure will end. If one grants that the movement of an occupation by one decile is of no major significance, one may also safely assert that relatively little has occurred to the relative economic position of most occupations since the depression, save for a section of the service and clerical workers. Perhaps the most significant generalization possible is that contemporary theories of occupational and class behavior and mobility based on apocalyptic Marxist analyses and status quo-oriented theories of mass society are equally inadequate in interpreting economic mobility and social relations.

NEW YORK CITY

²⁹ *Federal Reserve Bulletin*, December, 1950, p. 1598.

³⁰ *Ibid.*, August, 1952, p. 863, and August, 1956, p. 817. Any superficial survey of the growing literature on today's luxury market should convince one that conspicuous consumption of the most pronounced sort is still very much a part of American life and undoubtedly is growing among the rich (see, e.g., *Wall Street Journal*, October 16, 1956, p. 1).

THE STATUS OF THE MARINE RADIOMAN A BRITISH CONTRIBUTION

PETER H. MANN

ABSTRACT

The marine radioman in the British Merchant Navy has officer status, but, because of the special nature of his role, his status does not fit easily into the general officer hierarchy of the average merchant ship. It is suggested that in both the formal and the informal social structure of the ship's company there are many factors which result in the radio officer's being odd man out.

In Jane C. Record's recent article¹ on the status of the marine radioman in the U.S. Merchant Navy, she gave a picture of the struggle that took place before the radioman gained full acceptance as an officer. In this article a contribution from Britain is offered by a sociologist who was a radio officer in the British Merchant Navy from May, 1943, to January, 1947, a period covering both war and peace.

It is not intended to give a comparative description of the trials and tribulations that the British marine radioman underwent in achieving officer status. Suffice it to say that the problems were similar to those of his American colleagues and that officer status was secured. This article will rather turn to an analysis of the British marine radioman's position as it is found *on the ship*. With this contribution it is hoped that the more personal matters of status and role can be brought forward.

THE FORMAL STRUCTURE

Since the actual size of a ship's crew will affect its detailed structure, we shall concentrate on the ordinary tramp ship of approximately 8,000 tons. The hierarchy here described will be rendered more complicated by the special functions to be fulfilled in larger ships (particularly passenger liners), but as a basic structure it is general throughout the Merchant Navy. The master of the ship is aptly described by his title—he is indeed the master in all things at sea. While the chief engineer is his opposite number in the engineers' hierarchy, the fact that the

master holds *over-all* control places him above the chief engineer. Thus the master, who is a deck officer by training, raises the status of the deck officers under him above that of the engineer officers. From this there is always a recognized superiority, rank for rank, of deck officers over engineer officers. This is fortified by two circumstances. First, the equivalent to the first officer (chief officer or the "mate") is the *second* engineer, and so on down to third officer and fourth engineer officer. Added to this, equivalent ranks of deck and engineer officers keep the same watch, but the control of the *ship* is held by the deck officer for the captain, while the engineer officer holds the engine-room watch for the chief engineer. Since orders normally go from the bridge to the engine room, and only in most exceptional circumstances the other way, the superiority of status of deck over engine room is rarely disputed with any seriousness. Moreover, deck officers do much of their training at sea as cadets or apprentices, while most engineers serve their time in engineering works ashore; and, although the third officer will be certificated, the fourth engineer will probably be only a time-served apprentice. It is important to have this picture of the status position regarding mates and engineers (as they are known on board), since the radio officer must be placed somewhere in relation to this status system. In peacetime the normal tramp carries only one radio officer, and his formal position is equivalent to that of second mate or third engineer. If we take these three men as being *formally* of equal rank, we may now consider where the radio officer differs importantly from his deck or engine-room equals. First, there is an inter-

¹ "The Marine Radioman's Struggle for Status," *American Journal of Sociology*, LXII, No. 4 (January, 1957), 353-59.

esting difference in the status symbols of the bands of gold braid worn on the uniform cuffs by these officers. Some shipping companies have their own braid systems, but the general form set down by the Board of Trade gives the second mate two straight bands with a diamond² in between, and the third engineer the same, only with purple cloth backing the gold braid. All this braid is $\frac{3}{8}$ inch thick. The radio officer, however, wears two *wavy* bands of braid with a diamond in between, and his braid is $\frac{1}{4}$ inch thick. Thus his symbol of status is different in both design and thickness—an example which is perhaps slight but nevertheless interesting in showing the overt difference in uniform alone.

The second point, and one of major importance, is that the radio officer is a specialist who works alone. Deck and engineer officers all have their own departmental officer hierarchy, with the petty officers and men beneath them, but the radio officer has no one, apart from the cabin and table stewards, to whom he can give any orders at all without permission from some other officer. For example, if the wireless aerials must come down in port for cargo work, then the radio officer must ask a deck officer for men to be put to do this work. If one thinks, then, of an officer as a person in authority or one who leads other men, the radio officer is neither: he is simply a one-man department. In wartime, when all ships carried three radio officers, or on larger ships, where at all times more than one radio officer is carried, the position does not alter greatly because there are never any members of the radio department who are not officers.

On the ships with a single operator a further difference arises, in that the radio officer does not keep normal watches of four hours on, eight hours off, as do the deck and engineer officers. His watches are normally four two-hour watches, with three two-hour periods between them, and these

watches will be geared to Greenwich Mean Time, which may well not be ship's time. The radio officer is therefore changing his hours (by ship's time) quite often, and he does not fall within the regular pattern of ship's watches.

A final point, of great importance, is that most seagoing radio officers are not employed by the shipping company, which employs all other members of the ship's crew. A small number of shipping companies do employ their own radio officers, but the vast majority hire the wireless apparatus and the operators from wireless companies. Once at sea, this is of little day-to-day importance, but it further stamps the radio officer as "different," in that he is not a "company's man" and the general feeling aboard is that the radio officer is responsible to the master only.

It may now help to round off the picture of the formal structure by considering the function of the radio officer at sea. He is concerned primarily with communications to and from the ship. He deals with shore-to-ship, ship-to-shore, and ship-to-ship messages of all types, from general weather bulletins and accurate time signals (for chronometers) to personal messages, such as birthday greetings from the cabin boy to his mother in Liverpool. In times of danger the radio officer is expected to continue transmitting until the last possible moment before taking to the boat which carries the portable transmitter and receiver. The duties, therefore, range from the everyday mundane to the very rare heroic. The point of note is that in much of his work he is unseen and unheard, and in such cases as the cabin boy's telegram to his mother he is, in fact, a servant to everyone on board. The bulk of the messages that he transmits are from other people (chiefly the master), and hardly any messages he receives are for himself. He is therefore essentially an intermediary. In operating direction-finding and radar equipment he is working with the deck officers, but once again his function is to supply information; he does not navigate. Finally, it should be noted that on British ships he is not the ship's electrician, this

² The diamond indicates that an officer is certificated, but, since no formal sanctions are ever applied, an uncertificated third engineer could wear the diamond with equanimity.

post being normally held by the third engineer when no electrician is carried.

THE INFORMAL STRUCTURE

One good pointer to a person's informal status is to consider the friends he makes. Judging from the writer's observations, the marine radio officer tends more toward friendships with the deck officers than with the engineer officers. In the modern ship the wireless cabin is close to, if not on, the bridge, and the radio officer's own cabin is likely to be in the bridge accommodation near to, if not among, the deck officers' cabins. A further point arises that in tankers or "two-island" ships in which officers' accommodation is split up, the radio officer is nearly always with the deck officers, with regard to both the wireless cabin and his own cabin. Also, on some ships, the engineers have a "working mess," where they can eat without having to clean up if they are on watch. This separation emphasizes the fact that the engineers have a "dirty" job, while the deck officers and radio officers have "white-collar" jobs. To venture what can be no more than a hypothesis: if the social backgrounds of deck, engineering, and radio officers could be analyzed, the deck and radio officers would be from higher social-class families than the engineers.

A particular factor which is often a reference point in the ascription of status is a person's age. Here the marine radio officer is once again the odd man out. A peacetime radio officer studies ashore for a certificate issued by the Postmaster-General, and his training normally takes about a year. When he is qualified and holds the "Second Class" certificate, he is competent to act as single operator on most normal cargo ships.³ Further study for the "First Class" certificate

will allow him to take charge on larger ships. But the important point is that since training can begin at the age of sixteen, the radio officer can be sole operator at a very early age. And whereas there is a fairly regular age structure for the hierarchy of deck and engineer officers, the radio officer may be anything from about six years younger to forty years older than his equivalent among the deck officers. Thus the present writer was of an equal age with some cadets he sailed with, and, as sole operator, found his friends to be third mates in most cases.

The place of work of the radio officer—his wireless cabin—also has an effect so far as informal status is concerned. The radio officer never has to endure the extreme cold of the bridge in a North Atlantic gale or the overpowering heat of the engine room in the Persian Gulf. Add to this the fact that he has a sedentary occupation which enables him to smoke, read, and write letters when things are quiet, and it is not surprising that he is often considered to have one of the "softest" jobs on the ship. In addition, the radio officer normally has quite a lot of free time in port. The engineers have to do their repairs and maintenance in port, while the loading and unloading of cargo may result in longer hours for the deck officers than they have at sea. For the radio officer whose equipment has no defects, there is very little to keep him on board in port, and this often arouses envy from his colleagues, coupled with feelings that "Sparks" cannot be so important if he has so much spare time. In some companies radio officers may be required to help with clerical duties, such as tallying cargo, in port, but even here the radio officer is an auxiliary and is working outside his own field.

From the details given above it will be seen that the role of the marine radio officer makes it impossible for him to have a status that fits neatly into the formal hierarchy of the ship's company. Informally, his status is what he makes of it himself, and of this his age is a major determinant.

³ During the war a "Special" certificate, taking six month's study, enabled operators to go to sea as assistants only. The present writer went to sea as third radio officer on a cargo ship of 8,000 tons at the age of sixteen and took his second-class certificate after the war ended. At nineteen, he was sole operator. Had he remained at sea, he could have been sole operator on the same size of ship at his retirement at the age of sixty-five.

DIRECTIONS FOR RESEARCH IN GROUP BEHAVIOR

EDGAR F. BORGATTA AND LEONARD S. COTTRELL, JR.

ABSTRACT

Recent studies of group behavior have pointed to a pressing need for systematic empirical research on which to base theoretical formulations. Among directions for future research discussed are: the identification of general dimensions in terms of which any interaction field may be described and the determination of the syntality and personality components of these dimensions; the systematic study of theoretically important variables; a more sophisticated exploration of the phenomena of social perception; and the induction of changes in manifest social behavior patterns.

The analysis of social interaction fields as a focal interest of sociology has been more talked about than implemented. However, efforts in recent years to give theoretical and methodological content to such terms as "interpersonal relations," "situational fields," and "dynamics of small groups" probably mean that the concept of social interaction will be given more substance. The purpose of this paper is to indicate on the basis of our appraisal of current research on groups some promising directions for such research in the immediate future.¹

A crucial problem which has recently begun to receive research attention has been that of the classification of qualities of the group as a group.² This interest reflects the

frequent recognition that certain qualities in the social aggregate are emergent rather than mere additive properties of the individual members. The term "syntality characteristics" has been popularized by Cattell to identify these emergent qualities and to indicate the analogy with personality characteristics.

The importance of exploring and isolating the independent qualities emerging in group behavior will be given increasing recognition. A recurrent exercise in sociology has been to state some definition of the group and then proceed to say whether or not various kinds of aggregates are groups. It should be obvious that any definition of a group either specifies or implies certain dimensions in terms of which a collectivity is described. Cutting points on measures of these dimensions are more or less arbitrary and suited to the needs of the particular theoretical or empirical problem. Consequently, the identification of general dimensions in terms of which all collectivities or interaction fields are described, classified, and compared not only would be immensely helpful but is now technically feasible.

We have discussed elsewhere some of the cumulative knowledge of the problems of classifying of "interaction fields."³ Here we wish to emphasize some problems which require immediate attention. Let us define a syntality characteristic as one associated with the social aggregate which cannot be accounted for through additive parallel measures of the individuals, and a personal-

¹ Revised version of a paper presented at the 1956 annual meeting of the American Sociological Society. This paper stems from a project in applications of social psychology conducted at Russell Sage Foundation under the direction of Leonard S. Cottrell, Jr.

² See E. F. Borgatta and L. S. Cottrell, Jr., "On the Classification of Groups," *Sociometry*, XVIII (1955), 665-78; E. F. Borgatta, L. S. Cottrell, Jr., and H. J. Meyer, "On the Dimensions of Group Behavior," *Sociometry*, XIX (1956), 223-40; R. B. Cattell, *Description and Measurement of Personality* (New York: World Book Co., 1946), "Concepts and Methods in the Measurement of Group Syntality," *Psychological Review*, LV (1948), 48-63, and "New Concepts for Measuring Leadership, in Terms of Group Syntality," *Human Relations*, IV (1951), 161-84; R. B. Cattell, D. R. Saunders, and G. F. Stice, "The Dimensions of Syntality in Small Groups," *Human Relations*, VI (1953), 331-56; and R. B. Cattell and L. G. Wispe, "The Dimensions of Syntality in Small Groups," *Journal of Social Psychology*, XXVIII (1948), 57-78.

³ Borgatta, Cottrell, and Meyer, *op. cit.*

ity characteristic as one associated with the characteristic performance of the individual (independent of any given social interaction field). Thus, in isolating syntality characteristics, we must always take care not to ascribe to an aggregate properties which in fact belong to its members.

It can be seen that identification of either pure syntality or pure personality characteristics will be extremely difficult except in a very limited number of cases. Preliminary experience demonstrates that the task to which the investigator must first address himself is the isolation of the dimensions by which groups may most economically be classified, essentially the parsimonious set of independent factors descriptive of group phenomena regardless of their "purity." This done, attention can then be directed toward identifying the syntality and personality components involved in each dimension.

Exploration of the distinction between syntality and personality characteristics leads quickly to several important problems for research. Many replicating studies are needed to build upon and improve those currently available which deal with the classification of group characteristics. The skill of Hemphill and Westie in utilizing available theoretical formulations for selecting variables to characterize groups should be combined with the methodological capabilities represented in Cattell's work. One primary objective of research should be that of identifying group characteristics. Another should be the differentiation of the syntality and personality components. A third is to examine at the personality level characteristics associated with the given syntality characteristics. This third task is essentially that of "explaining" group performance as the result of interaction of personality characteristics.

Many more delicate research questions will arise. For example, drawing from available theory on socialization processes, we find the somewhat self-evident generalization that personality characteristics are formed in interaction with the relevant so-

cial fields, which amounts to saying that certain kinds of syntality characteristics may be manifested in changes in certain personality characteristics. Furthermore, such changes in personality may promote the emergence of additional syntality characteristics. The distinction of syntality and personality, thus, seems to lead to the possibility of dealing with social emergence in terms of both classes of variables, and the crucial problem of temporal sequence and process analysis comes to the fore.

Since we suggest the need for replicating studies in this area, we may point to several directions in which prior work needs to be extended. First, the ingenuity of our research workers will have to focus on problems of access to some of the social data which we wish to consider. Measures relevant to the description of group behavior include manifest products of the group, observed activity and ratings of behavior, interratings of members, self-ratings, and individual response tests. It is at this point that the most inclusive battery of social and psychological tests becomes relevant. But the availability of a large number of measures itself points to the necessity for refinement and reduction of the many to the relevant few. Second, studies will need to be directed to test the limits of generality of classifications of social interaction fields. Research thus far has been quite limited in scope, dealing with ten-man experimental groups, with three-man experimental groups, or with campus groups. New research should correspond to the limits of the prior ones. Generality needs to be tested by examining collectivities *within* homogeneous qualitative categories of theoretical importance such as are represented by three-man groups and the ten-man groups or in other classes like nuclear families, as fraternities. But generality must also be tested by examining collectivities in a more inclusive sense which cuts across these qualitative categories. In these tasks we are in the fortunate position of being able to draw on the field of personality, and many of the mistakes made in classi-

fying personality characteristics may be avoided.

We give a high priority to the identification of dimensions in terms of which any interaction field may be described and the determination of the personality and syntality components of these dimensions. Another type of problem, however, which should be highly fruitful is the systematic investigation of a single theoretically important variable.

One of the important characteristics which Simmel discusses in his formal sociology is the size of the group as a factor in the quality of interaction. Size for Simmel was not merely a simple continuous variable running from smallest to largest; he also referred to discrete (qualitative) categories which did not fit the continuum. Size has been considered important in many recent studies of small groups, and about seventy-five articles are listed on this topic in a recent annotated bibliography.⁴ However, a review of the literature reveals several important gaps in the knowledge of the effect of size. The primary shortcoming is that size is not considered systematically except in one report.⁵ Bales's treatment of size as a factor in the quality of interaction in groups of from two to seven. All the data descriptive of the quality of interaction were examined in relation to all sizes in the range. Why was this important? Simply because the data indicated that Simmel's idea that size operated both as a continuous quantitative variable and as a discrete qualitative variable was well supported. Certain aspects of interaction were found to vary directly with size, and there were other kinds of effects which could easily be masked or unexplained if not viewed in the complete series of sizes. There was a most pronounced odd-and-even effect. And there were unique characteristics apparently related to specific sizes.

Assuming that additional systematic research will support Bales's findings, what

⁴ A. P. Hare, E. F. Borgatta, and R. F. Bales (eds.), *Small Groups* (New York: A. A. Knopf, 1955).

bearing does the distinction between these sources of variance associated with size have on past, current, and future research? First, it should caution those who do research on size not to make loose interpretations about it without adequate data. For example, size as a factor when comparing groups of two and four persons will be most difficult to interpret.⁶ The group of two has some unique characteristics which we can identify simply by stating that, to define a majority, there must be unanimity. The four-man group has the characteristic of being capable of splitting into two equal opposing factions. Both two and four are even numbers. Now, when groups of these sizes are studied, what interpretations may be made about size as the variable? Caution is needed to assure that differences between groups of two and four are not associated with unique properties of either size. Similarly, caution is needed in inferring that the effect would persist if other sizes were included, since only groups of even size are used. Would the same results persist if only groups of three and five were studied instead? Would they persist if only groups of two and three were studied? Would they persist if groups of two to ten were studied? Obviously, this limitation may also hold for the published studies which compare groups of five and twelve and other combinations.

As a further extension of this criticism, consider the studies which match an individual in his performance alone (*a*) with his performance in a group or (*b*) the performance of the group in aggregate. For example, in a number of studies problem solving by the individual is compared with that of the two-man group. One study compares individual performance with that of the two-man and the four-man group and with indi-

⁵ R. F. Bales and E. F. Borgatta, "Size of Group as a Factor in the Interaction Profile," in Hare, Borgatta, and Bales (eds.), *op. cit.*

⁶ Since we wish to have our remarks interpreted generally, we do not report all references to studies of size. However, examples stated hypothetically throughout the paper refer to actual studies for which citations are available.

vidual performance in groups of these sizes. Exactly what is being measured here beyond the statement of the experimental conditions? How far may generalizations be made? Let us raise a question about these studies by citing the common assertion that no individual acts alone but, even when temporarily isolated, acts in a social field. At what point in size does the group approach the characteristics of the general social field to which the individual responds when isolated? Or does this ever happen? Is the performance alone similar to the performance in the large group? If so, many theoretical and research questions in the together-and-apart experiments are very poorly stated. The "social factor" studied in them may be a misidentification, and the factor involved may have something to do with the uniqueness of certain sizes of small groups.

Size as a variable is explored in Bales's research, but any research is confined to workable limits, and these define the points of further exploration. The range of groups from two to seven is obviously limited. Consider two simple extensions: As groups increase beyond relatively small sizes, (a) the effect of size may operate simply as a continuous variable, and (b) it may be proportionately less per unit increase. These and related hypotheses need to be carefully explored. For example, we have available at least one important contrary suggestion, namely, that there is a qualitative difference between groups which are larger and groups which are smaller, the division being around twenty-five or thirty. In particular, characteristics of mass action are suggested as associated with the larger groups, and characteristics of primary interaction with the smaller groups.

The comments thus far have been on studies of size as a variable, but the immediate implications of the assumption of size as an important quantitative and qualitative variable lead further. If a study is exploring the relationship among a set of variables and size is controlled by using groups of only one size, generalizations from the findings are limited. As we have mentioned

earlier, each such study demands replication, utilizing groups of other sizes to establish the generality of results. Generality cannot simply be implied or asserted. The caution is universal, for generality may not be implied or asserted in regard to any variable which has been held constant by dealing only with a homogeneous group.

It is at this point that we may indicate the convergence of the systematic study of one variable with the problem of classification of variables discussed as our first direction for research. Bales's research deals with casually brought together groups of paid college students as subjects. Are the properties explored in these groups replicated in other kinds of groups? Note that our wording here is completely ambiguous: *other kinds of groups*. We are asserting that there are kinds of groups other than those which are brought together in the laboratory, but we would have difficulty in providing the established theory and research to indicate the exact kinds of limitations. Exploring these limitations points to further emphases: The naïve statement of the limitation is the contrast between laboratory groups and "real" groups or groups in "real situations." Why is this a naïve statement? Because even preliminary exploration of the matrix of social interaction shows, for example, that the classification of groups should be possible on the basis of a relatively limited number of variables. If the same set of variables underlies the classification of laboratory groups as underlies some of the commonly designated groups in our social system, then the distinction is not between "real" and "not real": rather, as we have noted, it would be a profile difference on the general dimensions of all interaction fields.

We may here emphasize a contrast to Bales's study. Suppose as critical readers we look at Bales's study and imply that the conditions of his observation are unique and that the differences in quality of performance found with size as a variable are restricted to these conditions. How are these conditions unique, and where would we demand the replication of study? We could not

insist that the laboratory groups were not groups in not having emergent qualities. There are already a few studies which indicate quite the contrary. Whatever the processes involved, there is considerable evidence which suggests that, when persons interact, qualities which must be ascribed to the combination rather than to additive properties of the individuals arise very quickly, even as quickly as the members perceive and assess each other. (Here we use the term "assess" to refer to that constant process of "sizing up" in which we are perpetually involved, the perception of cues which allow us to order our relationships according to the system of expectations and values we possess at any given time.)

If characteristics which arise in the combination precipitate in the laboratory groups quickly, we may very well wish to examine other similar groups before examining a class which is distinctly different. Many of the occasions of social interaction in which we participate are casual and casually brought about: the exchange with the person on the library steps, the meeting with classmates, the joint wait outside the boss's office, the brush with the door-to-door salesman, and so on. Many relationships which presumably have stable characteristics, such as the fraternity, the friendship circle, the work group, etc., have fringe relationships as well as established intimate ones. Even the family has many *distant* relatives, brought together on occasion. It is not unreasonable to propose that, in this regard, Bales's groups which are casually brought together are relevant to the common situations one faces in daily life. What may be peculiar in Bales's groups is that statuses are not assigned; they develop out of the backgrounds of the individual members in interaction. But how different would the results of Bales's study have been if each group was made up of persons from the same fraternity? From the same work group? From the same family? Each such question indicates a possible replication, but each such replication is not equally important.

Here the point of contrast appears to arise not in the source of subjects but in the existence of certain defined status positions in the interacting groups. We immediately enter into another set of theoretical and research problems. In regarding the family, it seems expedient to think of statuses of father and mother, husband and wife, parents and children, etc., as qualitative distinctions. Not to do so would be to ignore the obvious, for associated with each of the statuses are certain specific and general expectations of behavior. But, again, there are many kinds of combinations of statuses included in the definition of family as is clearly seen in the common qualifying adjectives such as "nuclear," "extended," "broken," "childless," etc. While particular combinations manifest qualitative discrete characteristics, it is possible that the interaction patterns which arise among those holding the statuses are capable of being described in terms of the same variables as are the experimental student groups. As a matter of fact, it seems far-fetched to hypothesize otherwise.

To reiterate: to choose a specific focus, such as the nuclear family, for comparison with the experimental student groups is to contrast a relatively undefined social structure with one in which specific statuses are designated. If we are to establish generality of the findings in the experimental groups, we expect replication in cliques and friendship circles, in peripheral family relations, and in other situations where rigid fixed status structures do not exist, and we expect at least continuity with and partial replication in fixed and polarized structure situations such as the nuclear family. Consider the limitations of extension, however. The two-person, husband-wife pair may not be very different from any other dyadic pair, except for a legal and moral definition which binds the pair. Within that dyad there may be the same distribution of superordination and subordination, of support and rejection, etc., which characterizes the interaction of other pairs. However, the interaction in certain categories may be different. For ex-

ample, because of the legal and moral bond it may be that the married pairs are capable of tolerating higher degrees of tension and antagonism than other dyads. But in terms of a systematic analysis of social interaction this may be the proper way to identify the contrasted groups, that is, by the relative presence or absence of certain variables rather than in terms of our common-sense language.

If we consider a variable such as size of the nuclear family, we note a parallelism to the experimental student groups, but, as we have suggested, it is a matter of continuity rather than exact replication. When a child is added to the marriage dyad, many differences become evident in the interaction of the dyad, and the differences are progressive. As the child becomes a social person, the characteristics of the three-person interaction noted in the student groups may be approached. Another child makes four in the group, and, as the children become social persons, characteristics of the student groups again may be approached. Obvious analogies to the experimental findings occur. For example, breaking the family into subgroups is commonly identified in the language in such concepts as "struggle between generations" and "father-son" and "mother-daughter" pairs, and in some circles even "father-daughter" and "mother-son" affinities. Certainly strong pressure exists for examining the same propositions dealt with in the laboratory in more structured situations such as the family represents. Experimental variation of structure in the laboratory should provide additional information which would carry the continuity of the experimental student groups to the nuclear family.

In the course of our discussion we have not dealt with the question of *how* emergent structures come into being, although indicating the dynamic interplay of syntality and personality characteristics. One direction for exploring how emergent structures arise and change is the process of constant assessment in which we participate, and the substantive field which we identify as inter-

personal perception. This theoretical interest is one of the oldest in sociology, having strong roots in the work of Spencer, James, Baldwin, Cooley, Mead, and others and covering such well-worn terms as "sympathy," "projection," "empathy," "assumed similarity," "insight," "social sensitivity," and "social perception."

Again, research on social perception in social interaction is of recent vintage, drawing its impetus from the work of Dymond in 1948,⁷ when the first systematic attempt was made to organize the theoretically relevant concepts into research measures. If the concept of the self as derived in Mead's formulations of socialization and social learning is considered, the basic framework for studying perception in interaction is provided. The self refers to the perception of ego by others and their responses to him and the response of ego to those with whom he interacts. Both ego and other must be considered simultaneously in dealing with problems of personal perception.

More recently Cronbach and Gage⁸ have indicated some shortcomings in the formulations made by Dymond, outlining the statistical dependence of measures of accuracy of perception, actual similarity and assumed similarity of persons. Cronbach has proposed one major avenue out of the artificial problem of measurement through a technique considering components ignored in previous attempts at measurement. For example, he isolates four components in the score for common accuracy of perception as (a) elevation, or the way ego uses a response scale; (b) differential elevation, or the ability of ego to judge deviations of a person's elevation from the average of others; (c) stereo-

⁷ R. Dymond, "A Preliminary Investigation of the Relation of Insight and Empathy," *Journal of Consulting Psychology*, XII (1948), 228-33, and "A Scale for the Measurement of Empathic Ability," *ibid.*, XIII (1949), 127-33.

⁸ L. J. Cronbach, "Processes Affecting Scores on 'Understanding of Others' and 'Assumed Similarity,'" *Psychological Bulletin*, LII (1955), 177-93; N. L. Gage and L. J. Cronbach, "Conceptual and Methodological Problems in Interpersonal Perception," *Psychological Review*, LXII (1955), 411-22.

type accuracy, or the ability of ego to predict the average elevation of others; and (d) differential accuracy, or the ability to order others in regard to the qualities involved. Each of the last three components can itself be broken into two parts, one having to do with dispersion and one with correlation.

It becomes quickly evident that handling problems of personal perception is not a simple matter, and naïve measures are henceforth ruled out of order by this work. But the immediate result of the development of more sophisticated measures reduces much of the previous work to dubious value. Hence, an important direction for future research is the replication of previous research with more sophisticated measures.

It appears reasonable that measures of interpersonal perception may be associated with the individual members of a collectivity and *also* with the emergent characteristics of the group. It is vital to make concrete the knowledge about perception as to understanding both emergent group behavior and change in personality characteristics; one important aspect of this latter, of course, is socialization. But the very fact that perception and communication of content are closely tied together raises a corollary caution on considering perception in general. Indeed, another pressing task is the development of replicating studies emphasizing *perception of what*. It cannot be assumed that a person who is accurate in perception in one content (or situation) will be accurate in another; but, similarly, it cannot be assumed that a generalized quality of accuracy does not exist. However, demonstrating that some persons are more accurate in one situation while others are in another does not exclude the possibility that some persons are more accurate in most situations.

There is a unity in these three directions of research. They point to the location and isolation of the relevant variables in the field and deal with problems of emergence and of change. An additional area for research presents itself in the question: What level of description is sociology achieving? The ability to describe and understand social phe-

nomena can be said to reach maturity only when sociologists can go beyond this, demonstrating theory through purposive manipulation of variables.

Review of the literature on research on induced and purposive social change reveals an appalling void. The studies of so-called changes in attitude rarely are more than verbal. In the field of psychotherapy there is practically no evidence that any of the techniques, from the somatic to the esoteric, has any effect on the personality of the individual. In educational research, dealing with *re-education* rather than growth and development, there is little evidence of change associated with techniques other than *practice*. Thus, another important direction for research revolves around merely demonstrating the inducing of change in persons or social aggregates. This suggests, however, that there are more delicate problems involved. For example, because a given characteristic of personality is associated with a given characteristic of syntality does not imply that persons placed in social collectivities high in the syntality characteristic will develop the personality characteristic. Unfortunately, behavioral scientists do not know how to induce change, either of specific characteristics or of change in general. At this juncture it seems that about the best we can say is that, if we put people through a set of motions, some of them derive the meaning generally ascribed to the set of motions—this is sometimes called “role practice”—but so ambiguous a generalization carries little weight and is older, certainly, than William James’s famous formulation.

In conclusion, we can point to the encouraging fact that theoretical formulation and methodological sophistication have shown notable advances in the last few years. Moreover, machines for the complex processing and analysis of the voluminous data generated in research in even the smallest interaction field are now available. On the one hand, we can avoid futile speculation and debate through a greater ability to test hypotheses; on the other, this forces attention to theory.

RUSSELL SAGE FOUNDATION

CHARACTER AND SOCIAL ROLE

MICHAEL S. OLMSTED

ABSTRACT

In a series of ratings by undergraduates at a woman's college using Riesman's character typology, inner-directedness was ascribed by 10 per cent of the cases to the "average college girl," by 24 per cent to "most of the boys you know," by 33 per cent to close friends, by 38 per cent to one's self, by 42 per cent to the respondent's mother, and by 64 per cent to her father. An individual's ratings of others tended to be in the same direction as her self-rating, though this does not appear to reflect generalized bias. Age, sex, "primariness," and "structuredness" are involved in such estimates in addition to the character traits which are ostensibly the basis of the ratings.

This paper describes an exploratory study of Riesman's¹ character typology of inner- and other-direction as understood by undergraduates at a woman's college. The data consist of ratings in these terms by students of themselves and of a few other persons and groups.

The purposes of the study were three. First, there was the question of how applicable Riesman's concepts actually are for a non-professional audience—in this case, college undergraduates. Second, there was the problem of how these undergraduates, using this conceptual apparatus, would perceive their social world. How would they rate various people and what would be their relative positions on a scale of directedness? Finally, this study aimed to raise certain questions about what these perceptions or ratings suggest as to the relation of character structure and social roles.

Students in introductory courses in sociology and in social science at X College, who had read and discussed *The Lonely Crowd*, were presented with a short questionnaire which read as follows: "In terms of inner- and other-directedness, how would you judge the following? 1. The 'average' X girl? 2. Your particular friends at X? 3. Most of the boys you know? 4. Yourself? 5. Your mother? 6. Your father? 7. David Riesman?" For each person to be rated the respondents were given five categories from

which to choose? "Very inner, Somewhat inner, Somewhat other, Very other, and Can't say."² Out of a total of 190 questionnaires collected at the end of the allotted class time, 53 were from Freshmen, 117 from Sophomores, and 7 from Juniors, with 13 unspecified.

It is quite clear that this study can and does tell us nothing about people or Americans in general. The sample is not and was not supposed to be a cross-section of anything outside the X College environment, of which, however, it was, on the whole, fairly representative. Students taking these courses were not necessarily enrolled because of a passion for social analysis but merely as a means of satisfying college distribution requirements. (The few students who did not bother to turn in the questionnaire are not of great importance.) Whether the 190 represent the "new generation of college youth" is very dubious, though they may be representative of girls of Ivy League colleges drawing students from middle- to upper-class backgrounds.

Moreover, it is obvious that this sort of questionnaire is not designed to discover whether the persons rated are "really" inner- or other-directed but only whether in the respondents' judgments they are *thought* to be so. The questionnaire assumed that, having read a substantial portion of *The*

¹David Riesman, Nathan Glazer, and Reuel Denney, *The Lonely Crowd* (New Haven, Conn.: Yale University Press, 1950).

²Despite this attempt at confinement, many respondents gave ratings between the categories. In Tables 1 and 2 these have been labeled as "Borderline."

Lonely Crowd, the student would have a general idea of Riesman's categories and that within any one respondent's set of answers there were relatively stable criteria for assessment; but it did not assume that any two respondents would necessarily define the categories in identical fashion. The ratings were thus subjective or projective, and the results constitute a study of perceptions of a certain part of the social world.

Finally, the respondents were given no opportunity to rate themselves or others as "tradition-directed," because, for this particular sample, tradition-directedness was an almost purely academic concept, quite inapplicable to anyone they were being asked to rate. There are grounds for ques-

that, in comparison with the seventh question, the other six proportions *are* small. In comparison with this distribution, the distribution of responses to the first six questions appears to take on considerably more validity.

Table 1 shows the discrepancy between the distribution of responses to question No. 1 and to question No. 4. It would appear that the college average is more other-directed than our sample. How is this difference to be interpreted? The simplest and most obvious explanation is that the previous assertion of representativeness is wrong and that the girls who marked and returned their questionnaires leaned relatively heavily to inner-direction as com-

TABLE 1
RATINGS OF DIRECTEDNESS
(*N* = 190)

PERSONS TO BE RATED	DIRECTEDNESS							
	Very Inner	Border- line	Some- what Inner	Border- line	Some- what Other	Border- line	Very Other	Can't Say
1. Average girl.	4	1	14	7	97	3	56	8
2. Particular friends.	7	0	55	13	102	2	8	3
3. Boys known.	5	1	40	9	98	3	22	12
4. Self.	8	0	64	18	83	0	7	10
5. Mother.	12	1	66	7	81	0	18	5
6. Father.	26	1	96	8	47	0	5	7
7. Riesman.	17	0	33	1	17	0	24	98

tioning this assumption, but they appear to be more theoretically interesting than practically important.

The results of the questionnaire may be seen in Table 1. There are relatively few NADK responses ("Can't say"), even if borderline responses are added thereto. Twenty-eight out of 190 could not or would not place themselves in one of the four categories; for other ratings the borderline plus NADK responses constituted an even smaller proportion of the total. It may be considered relatively small for two reasons: for one thing, the questionnaire does not provide many alternative answers to questions which are either very vague (e.g., Nos. 1, 3) or very complex and personal, questions such as make the respondent throw up her hands in despair. The second reason is

pared to the college total. Thus they perceive accurately the nature of their position in the immediate social world. We may examine this explanation more closely by making a breakdown of responses according to the self-ratings given. Table 2 shows the distribution of responses for the two subsamples (defined in terms of those rating themselves as very or somewhat inner-directed and those rating themselves as very or somewhat other-directed).

It will be seen from this table and from Figure 1 that the average girl is rated about the same by both subsamples and that for one of these subsamples—the other-directed one—the respondents themselves are close to the average girl, while for the other subsample self and average girl ratings are at the opposite ends of the continuum. In the case of the former subsample there is no

problem of representativeness; in the inner-directed subsample there would be no problem of representativeness *if* this subsample were not so large. If, in actual fact, the average girl were highly other-directed, then a small minority of inner-directed girls might well be aware of their own unrepresentative position. But if, as is actually the case, this minority were not small and instead constituted some 40 per cent of the total (72 out of 162), then the position of the average girl itself is affected. The choice of explanations

there is no way of making a definitive choice, at least not one based solidly on the questionnaire. Experience at the college in question inclines one toward the second possibility. That students frequently lament in private the conformist and superficial qualities they took to be typical of the vast majority results not so much from their minority status in the college community as from their apparent inability to break out of their isolation and to recognize the many, many others who also resented group pressures to

TABLE 2
RATINGS OF DIRECTEDNESS, BY SELF-RATINGS*

PERSON TO BE RATED	DIRECTEDNESS							
	Very Inner	Border-line	Some-what Inner	Border-line	Some-what Other	Border-line	Very Other	Can't Say
Self-rating: Inner-directed (N = 72)								
1. Average girl.....	1	0	6	2	33	1	24	5
2. Particular friends.....	6	0	38	2	25	0	0	1
3. Boys known.....	3	0	22	2	31	1	7	6
4. Self.....	8	0	64	0	0	0	0	0
5. Mother.....	6	1	29	2	26	0	8	0
6. Father.....	16	1	35	4	14	0	2	0
7. Riesman.....	9	0	9	0	8	0	9	37
Self-rating: Other-directed (N = 90)								
1. Average girl.....	2	1	8	1	52	1	23	2
2. Particular friends.....	0	0	9	2	71	2	6	0
3. Boys known.....	0	0	12	1	60	2	12	3
4. Self.....	0	0	0	0	83	0	7	0
5. Mother.....	4	0	26	0	48	0	10	2
6. Father.....	7	0	46	0	32	0	2	3
7. Riesman.....	7	0	24	1	5	0	12	41

* Does not include 28 cases where self-rating was indeterminate. Analysis of other ratings in these 28 cases shows a distribution of ratings that runs slightly more than average to the inner-directed end of the continuum, particularly in the case of ratings of parents.

thus seems to be either (a) that the sample tested has an unusually large contingent of inner-directed girls who just happened to be the introductory courses or (b) that the sample itself is roughly representative of the college population but both inner- and other-directed girls do not perceive the collegiate world accurately, neither type recognizing that some 40 per cent of their colleagues are or consider themselves inner-directed. In this view the inner-directed girls are not aware of their own numerical strength; like the protagonist in Chester-ton's *The Man Who Was Thursday*, they see their position as much more atypical than it actually is.

As between these alternative explanations

conform. As Riesman has suggested, what these people need is a union.

Whether the second explanation is acceptable or not, it does suggest at least one further generalization—that persons who are known only indirectly and “secondarily” by a respondent tend to be perceived as more other-directed than they “really” are. This hypothesis, in turn, suggests a possible association between the concepts of inner- and other-direction and the concepts of primary and secondary relationships, an association clearly not an identity but which perhaps deserves more logical analysis and empirical exploration than it has so far received.

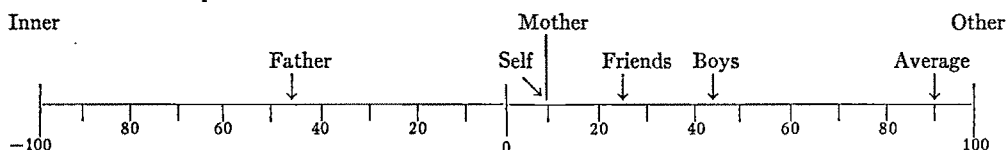
Returning to Table 2 and Figure 1, it will

be noted that, unlike the agreement expressed on the average girl, there is a definite difference when it comes to ratings of particular friends and boys known. In terms of four cell contingency tables all inner-directed girls and all other-directed girls versus all inner-directed friends (or boys) and all other-directed friends (or boys), the chi squares are 56.4 and 11.82, respectively. It is

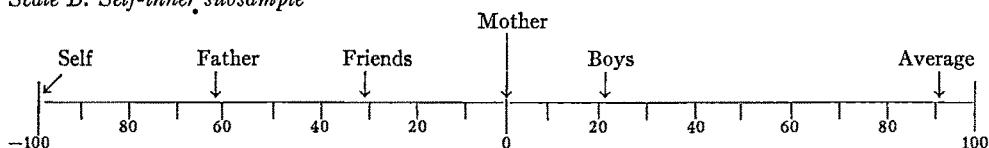
same pigeonhole as they put their fathers. Table 3 shows that this is especially true for the other-directed girls.

Table 3 shows only whether girls put themselves in the same category as their parents; it does not show whether the daughters thought of themselves as being on the same side of the inner-other dividing line as their parents. Consideration of the

Scale: A Total sample



Scale B: Self-inner subsample



Scale C: Self-other subsample

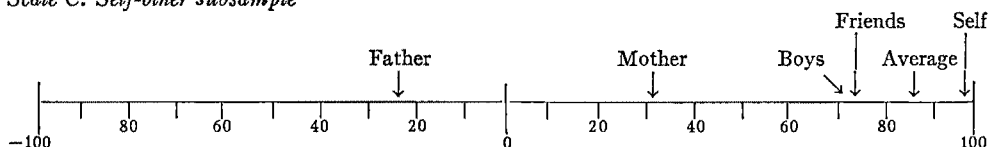


FIG. 1.—Relative positions on inner-directed-other-directed continuum; for total sample and for self-inner and self-other subsamples. (Defined in terms of scores where extreme or "very" categories are multiplied by a factor of two, and the inner-other dividing line is conceived of as a zero point. For convenient but only rough comparison with Scales B and C, the total sample scores used on Scale A have been divided by 2. Scale A is not strictly comparable, because it contains 28 cases where no self-rating was given, cases which could not, therefore, be included in either subsample. The scores on Scale B have been multiplied by a factor of $5/4$ [i.e., $90/72$] to make it comparable with Scale C.)

most unlikely that chance could account for the fact that inner-directed girls know friends and boys who are relatively more inner-directed than are the friends and boys known by the other-directed girls: are they not "birds of a feather"?

The relation of self-ratings to ratings of parents, and the implied question of family influences, can be dealt with only in a crude and preliminary fashion, limited, as we are here, by the questionnaire data. In the first place, girls are likely to rate themselves in the same one of the four categories as they rate their mothers, more often than in the

TABLE 3
SIMILARITY IN RATING SELF AND MOTHER
AND SELF AND FATHER
BY SELF-RATINGS

Self-rating	Same Category as Mother	Different Category from Mother	Same Category as Father	Different Category from Father
Inner-directed . .	30	42	33	39
Other-directed . .	49	37	28	58

appropriate lines in Table 2 shows that daughters do tend to put themselves on the same side of the fence as their parents—except in the case of other-directed girls and their fathers. Combining degrees of directed-

ness (the "very" and "somewhat" categories) for both parents taken together yields a fourfold table which shows that, on the whole, inner-directed girls have inner-directed parents and other-directed girls have other-directed parents ($P[\chi^2 = 8.82] < .01$). These questionnaire data, then, reflect a family influence that might have been expected on general sociological grounds.

What can we gather from the questionnaire about the relative influence of mothers and fathers on the directedness of their daughters? Unfortunately, not a great deal which is unexpected or unambiguous. Part of the direction of parental influence seems to depend on whether one is speaking of inner- or other-directed girls. Table 4 shows the position of the daughter relative to each parent: inner-directed girls lean slightly to the "inside" of their mothers and perhaps of their fathers, and other-directed girls lean to the "outside" of both parents, particularly as compared with their fathers. This follows from the relative positions on the inner-other continuum which the several parties occupy: since mothers tend to be more other-directed than fathers, any girl who rates herself as other-directed will be putting herself closer to the position of mothers in general and farther from the probable position of fathers (see Fig. 1).

As to influence, it is obvious that parents are more likely to mold their children's character than vice versa. Furthermore, if a girl puts herself on the same side of the fence as she does a parent, it is likely that that parent has, for one reason or another, been a model for the daughter. Thus, if a girl leans to inner-directedness, it is presumably because she follows her father's lead more than does the other-directed girl, who presumably is following her mother as model. The data appear not to contradict this notion though scarcely proving it.

On the assumption that there might be some interesting differences among families wherein the mother was rated as more inner-directed than the father, families wherein mother and father were rated the same in

directedness, and families wherein the mother was more other-directed than her husband, a breakdown of the data was made along these lines. Unfortunately, very little emerged that does not follow from and could not be deduced from the rankings presented in Figure 1. Positions of the parents relative to each other do not seem to make much difference in a daughter's pattern of ratings.

On the whole, then, the idea of family continuity in character probably receives support from this study. That the results—specifically, that inner-directed girls tend to have relatively inner-directed parents as

TABLE 4

RELATIVE POSITION OF SELF-RATINGS IN COMPARISON WITH RATINGS OF MOTHER AND OF FATHER, BY SELF-RATINGS

SELF-RATING	SELF-RATED AS:		
	More Inner than	Same Category as	More Other than
Inner:			
Mother.....	38	30	4
Father.....	23	33	16
Other:			
Mother.....	7	49	30
Father.....	2	28	56

compared with other-directed girls—are not simply the product of a tendency for some girls to see everybody, including themselves, through inner-directed glasses, so to speak, and for different girls to peer through other-directed spectacles, is suggested by the absence of such presumed biases in the ratings accorded to the average girl and to Riesman (Table 2). In the latter instance there even seems to be a "reverse twist," opposite to the way in which bias is supposed to act. Girls who rate themselves as other-directed have a slight tendency to rate Riesman as inner-directed, a tendency not shared by the self-confessed inner-directeds. This is, perhaps, to be explained, for one thing, by the other-directed girls' possible conviction that only some sort of strange conscience-stricken, that is, inner-directed, professor would have the audacity to call into question the virtues of co-operativeness. In any case, the explanation of general bias does not appear

to account for the tendencies to familial similarity.

Among the few cases of girls who rate themselves as either very inner-directed ($N = 8$) or very other-directed ($N = 7$) the only substantial difference is in the ratings accorded to particular friends. Very inner-directed girls rate their friends as inner-directed in a ratio of seven to one, while for the very other-directed girls the ratio is one to six. Oddly enough, there is no substantial difference in the ratings of parents for these tiny samples of extremists in self-ratings.

girl rates her father or her mother in what are ostensibly characterological terms, the fact that they are parents, and not just people, may well be the most important component in her assessment. They are persons filling roles which, by social definition, involve the incumbent in the responsibilities of supporting, supervising, and disciplining children. Though perhaps intimately known by a child as confidant and companion, a parent is still the inculcator of morality, the transplanter of one's superego, the basic producer—if not the final inspector and ad-

TABLE 5
COMPARATIVE DIRECTEDNESS AND DEGREE OF PROBABILITY
OF THE ASSOCIATION

COMPARISON	CHI SQUARES (1 d.f.)*		
	For Total Sample	For Self-inner Subsample	For Self-other Subsample
A. Parents (Nos. 5+6) are more inner-directed than contemporaries (Nos. 1+2+3)	97.5	19.65	67.2
B. Fathers (No. 6) are more inner-directed than boys known (No. 3)	60.3	17.9	39.5
C. Men (Nos. 3+6) are more inner-directed than women (Nos. 1+2+5)	35.3	8.42	17.66

* $P(\chi^2_{1 \text{ d.f.}} = 6.635) = .01.$

Implicit in much of what has already been said is the question of the relationship between character type and role. By combining the appropriate categories from Tables 1 and 2, the relationships in Table 5 can be shown to hold. Lines A and B in Table 5 seem to indicate that as between the older and younger generations there is a statistically significant difference in character type. Line C would appear to say the same thing as to the difference of the sexes. It is necessary to put these two propositions in the conditional mode for two reasons. First, the ratings may not be valid indexes of what people are "really" like but only indications of what certain college girls say they are like according to standards which vary with the depth of understanding of the Riesman typology. This is a condition which applies to all the ratings and thus to this whole report. The other reason is that the questionnaire may not be measuring character differences as much as role differences. When a

justor—of the gyroscopes and/or radar sets of the young. A girl's feminine contemporaries and even the boys she knows inhabit a social world whose pressures and currents are familiar to her so that, when she comes to consider *their* behavior, she is likely to be aware of the group pressures. The familial responsibilities of parents, on the other hand, and their roles and statuses in the larger community are either unfamiliar to her or else are a constraining power which puts them in opposition to the desires of the young. Insofar as daughters feel that their parents, especially their fathers, dwell partly in the upper reaches and impose unwelcome demands ("Daddy wants me to get a job in an office this summer instead of working in the day camp"), they may look upon these demands as stemming from an inner-directed conscience. It is correspondingly less likely that they will recognize that the parent may be responding to the demands of *his* peer group which conceivably

run counter to his own desires for, let us say, the promotion of day camps. The example may be far-fetched, but the fundamental point still stands: the great big outside world consists of social facts, in Durkheim's sense, marked by exteriority and constraint and, as it is mediated by parents and men, appears to derive from persons possessed of inner-directed gyroscopes. This appearance of inner-direction, it may be hypothesized, would tend to be the greater the younger the child, the greater his subordination, the less his comprehension of his parents' motivations, and the more the parent is involved in doing things in the big world. The parent who excels in this latter respect would generally be the father rather than the mother—and this brings up the problem of character type and sex role.

In most societies, including our own, the masculine role is more oriented to production than is the feminine, except in the all-important but special case of producing children. Men are likely to be concerned more with the "wheat bowl" than the "salad bowl," with the "hardness of things" rather than with the "softness of people." From the point of view of the suburban daughter, which is what these respondents characteristically are, daddy's going off to the office in the morning and his returning at night full of unnamed frets signifies a wrestling with the hard, impersonal world, even if he is public relations man in a cosmetics firm. Mother, on the other hand, is more directly related to consumption. She may not buy all the food and furnishings without the advice of her spouse, but she certainly spends more time at it than he does. Moreover, she is typically concerned with personal relations—with her children, her neighbors, her husband's associates, etc.—this division of labor being played upon to the point of utter exhaustion in the mass media.

At this point appears a paradox. On the one hand, it has been argued that where a respondent is assessing a person in an "unfamiliar" role, where the standards by which that person operates are not visibly derived from the group—as, for instance,

when a daughter rates a father—then the tendency will be for the respondent to judge him as relatively inner-directed. On the other hand, it was hypothesized earlier that, where the respondent rates persons with whom she is not personally familiar—the average girl—she is inclined to see them as relatively other-directed. Lack of personal familiarity, then, seems to lead to opposite and contradictory effects.

Unless one of these two propositions is wholly fallacious and no reconciliation is necessary, the rapprochement lies in the clarification of what is implied in role rela-

	Primary	Secondary
Hierarchical or organization of roles	(1) E.g., master-servant relationship	(2) E.g., boss-worker relationship
Equal and indeterminate roles lacking formal structure	(3) E.g., companionship	(4) E.g., casual contacts

FIG. 2.—Types of role relationships

tionships. The difference between the two situations may be characterized by two sets of distinctions. In the first place, one of these relationships is "primary" in the traditional sociological sense, while the other is "secondary"; one is the relationship of daughter to father, the other of one girl to a generalized other consisting of some two thousand fellow students.³ In the second place, one of the relationships involves a ranking or hierarchical organization of superordination and subordination, while the other involves a more voluntary and "unbound," as it were, relationship between equals (Fig. 2).

The distinction between cells (1) and

³ The ideal-typical distinction between primary and secondary relationships may be defined, alternatively, in terms of the five pattern variables used by Parsons. A primary relationship would be defined in terms of affectivity, collectivity-orientation, particularism, ascription, and diffuseness; a secondary relationship would be defined in terms of their opposites (see Talcott Parsons *et al.*, *Toward a General Theory of Action* [Cambridge, Mass.: Harvard University Press, 1951], Part II, chap. i).

(3), on the one hand, and cells (2) and (4), on the other, is, of course, well known in sociological literature, while the cross-cutting distinction between cells (1) and (2) and cells (3) and (4) has received considerably less attention—indeed, the presumably exhaustive five-pattern variables of Parsons do not permit of this distinction, applying as they do to the primary-secondary division.⁴ For present purposes cell (1) is the prototype of the daughter-father relationship, and cell (4) is the prototype of the respondent-average girl relationship. It needs hardly be mentioned that the former proposition is highly heuristic and that actually the relations between fathers and daughters in the present context are about as equalitarian as any family system has ever produced. Character ratings of the sort used here suggest that persons in a type (3) relationship (rating of particular friends) would rate one another in most purely characterological terms, while persons in a type (2) relationship would rate each other in most purely role type terms. Furthermore, persons in a type (1) relationship would tend to perceive the other as inner-directed,⁵ while those in a type (4) relationship tend to rate the other

as other-directed. This empirical study, such as it is, at least conforms to these hypotheses, which is all that can be expected of an after-the-fact explanation. Data on how workers rate bosses or how wives rate husbands would help to clarify the picture and reveal faults in the paradigm.

What has been said here about character type and role types does not in any way invalidate the concept of social character. It may serve to suggest some of the difficulties in defining and measuring the characterological variable, but it would be foolish to try to dispense with the concept and so assimilate personality variables to sociological ones. Social character may be thought of as a way of treating psychological factors so as to articulate them with sociological factors, the latter being perhaps best conceived of for these purposes as roles. It is important for social science to bring these levels of analysis, the psychological and the sociological, together—not alone for the sake of conceptual neatness or to provide a kind of *entente cordiale* between disciplines which should not be so separate in the first place, but, more important, to try to discern and analyze that area (which our present system of academic principalities forces us to think of as "borderline") where external social prescriptions enter into and become part of the individual's pattern of reaction. It is a fact of life, and not a mere conceptual confusion, that the roles people fill mold not only their behavior but their *Weltanschauungen* as well and that the organization of personal capacities and desires may influence their social positions.⁶

⁵ This appears to be the case for the subordinate-to-superordinate rating in type (1), though whether it would be true in the reverse direction is dubious.

⁴ Recently, Parsons suggested the desirability of the addition of a sixth pattern variable which would presumably take this distinction into account (see Talcott Parsons, "Some Comments on the General Theory of Action," *American Sociological Review*, XVIII [December, 1953], 618-31). Parsons' latest major publication does embody the superordination-subordination distinction, though in a context somewhat different from the present one. This difference in power is there defined as being a difference between the two parties with respect to "relative importance in carrying out the functional performance of the system" (Talcott Parsons and Robert F. Bales, *Family, Socialization and Interaction Process* [Glencoe, Ill.: The Free Press, 1955], p. 46 n.; cf. chap. ii *passim*). It should not be necessary to point out that the typology set forth above is not intended to be exhaustive of possible role relationships. The presence or absence of a power distinction, while not explicit, is certainly understood in the double use of the familiar form of address in many European languages. The forms *tu* or *du*, for example, may be used for servants as well as for close friends (cf. Kurt Wolff, *The Sociology of Georg Simmel* [Glencoe, Ill.: Free Press, 1950], Part III, "Superordination and Subordination").

⁶ "The role-expectation . . . which is a unit of a system of social interaction, is itself also a motivational unit—an internalized object of the personality of the actor. When a person is fully socialized in the system of interaction it is not nearly so correct to say that a role is something an actor 'has' or 'plays' as that it is something that he *is*" (Parsons and Bales, *op. cit.*, p. 107).

Is what we have been looking at, then, a picture of types of roles or types of character structure? I do not believe this can be determined on the basis of the present data. Whenever one person rates another's "social stimulus value," whether in terms of Riesman's typology or not, we are likely to get data in which role and character factors are intermingled. The fact that we may not be able to say in any given case just how much is role and how much is character need not be discouraging. What we do know, within the limits of the crudeness of concepts and the questionnaires, is how one sort of person views another. To say that this is valueless, since we do not know whether this data re-

flect role or character factors, is like saying that data on child behavior are worthless because we do not know "how much" is due to heredity and "how much" to environment. Contrariwise, to argue that useful analytical abstractions such as "character" and "role" or "heredity" and "environment" are mere burdensome verbalisms is to take a short cut to intellectual extinction. The problem is to say something coherent and reasonably interesting, and this goal is not likely to be attained without some readiness to ruminate on the propriety of concepts and the importunacy of facts.

SMITH COLLEGE

FORMAL ORGANIZATION: DIMENSIONS OF ANALYSIS¹

PETER M. BLAU

ABSTRACT

Impersonal controls in formal organizations, such as assembly-line production or evaluation on the basis of performance records, tend to affect the structure of work groups and reverse the flow of interaction between superior and subordinates. Three methodological problems are discussed: The effects of group structure can be isolated by determining the relationship between X and Y for groups while holding constant the independent variable X for individuals. The quantitative study of complex configurations of interdependent elements involves the internal and external elaboration of a relationship between two major elements. Dialectical processes characterize organizational change, and these processes may be empirically investigated by adapting the panel method to organizational research.

It has been only within the last decade or two that the precise methods of social research developed in interviewing surveys and in observation laboratories have been applied to the study of military services, factories, government agencies, and other formal organizations.² Often, however, the research techniques have been adopted without first having been adapted to a new field of inquiry. Quantification, so important for providing evidence in support of generalizations, has often produced an artificial atomization of the organized social structures under investigation. Not that the members of the organization are conceived as Robinson Crusoes on isolated islands. Quite the contrary, the emphasis is all on human relations, but as atoms somehow suspended in free space. Specifically, human relations are treated in the analysis as though they were

attributes of individuals, and the group structures of which they are component parts as well as the larger organization of which these groups are parts are neglected.

These tendencies are the result of a fundamental methodological problem. Since an empirical study is usually confined to one or two organizations and the investigation of a large sample of organizations is hardly feasible, quantitative evidence for generalizations must be based on the observation of regularities among individual members or subgroups. But, by treating individuals, or even subgroups, as independent units of analysis that can be classified and reclassified according to any one of their characteristics, this procedure necessarily ignores the unique constellation of relationships between groups and individuals in the organization—its *Gestalt*. If, on the other hand, the analysis is focused on the organized whole of interdependent elements, it deals only with a single case and provides no empirical evidence for generalization, no matter how many individuals are observed.

This paper is an attempt to explore this dilemma in the study of formal organizations. For this purpose a secondary analysis of some research findings will be presented first, which is largely concerned with the effect of impersonal mechanisms of control upon the structure of work groups and the flow of communication in the hierarchy. Four dimensions in the analysis of formal organization are suggested.

¹ I am indebted to Joan W. Moore, who helped me with the survey of the literature and contributed important ideas to the analysis, and to the Social Science Research Committee of the University of Chicago, which provided funds.

² The term "formal organizations" is used here to refer to social organizations that are formally established for explicit purposes but to include the informal as well as the formalized aspects of such organizations. Herbert A. Simon, Donald W. Smithburg, and Victor A. Thompson define formal organization as "a planned system of cooperative effort in which each participant has a recognized role to play and duties or tasks to perform. These duties are assigned to achieve the organization purpose" (*Public Administration* [New York: A. A. Knopf, 1956], p. 5).

WORK GROUPS ON THE ASSEMBLY LINE

The distinct difference between having established personal relations with several co-workers and being a member of a work group is clearly illustrated in Walker and Guest's study of assembly-line workers in an automobile plant. Despite the noise and the fact that hardly any jobs on the assembly line require co-operation between workers, most workers have regular social contacts with a few others stationed near by. Indeed, over three-quarters of them consider friendly contacts with fellow workers one of the things they like best about their job.³

Since the workers are strung out along the line, however, the set of interpersonal relations of each differs somewhat from those of everyone else. Tom and Dick are friends, and both have frequent contacts with Harry, who stands between them; but Tom also often talks to three fellows on his right, who are out of Dick's earshot, and Dick has friendly ties with two men on his left, whom Tom hardly knows. There is no common network of social relationships that unites a number of workers and distinguishes them from others by furnishing a socially agreed-upon definition of the boundaries of the in-group. Notwithstanding regular patterns of informal interaction, therefore, work groups do not seem to exist on the assembly line.

This is not merely a matter of arbitrary definition. In the absence of a *shared* set of social relations and a common boundary, there is no single group with which a number of individuals can identify themselves and which, in turn, provides them social support. Perhaps this lack of group support is one of the reasons why assembly-line workers become so quickly and strongly identified with their union.⁴ It may also play a major role in the prevalent dislike of work and the high rates of turnover and of absenteeism on the assembly line.⁵ Indeed, when the same research team in a second

study discovered that work groups sometimes do become established on the assembly line, they also found that absenteeism declined in them.⁶

The men on the assembly line are divided into sections under different foremen. Merely having the same foreman does not give rise to a group structure in the section: the foreman must help to create it. Most important, the foreman must identify himself with his men as a group and "think of himself as also a member of the group";⁷ he sticks up for his men, treats them as equals, and delegates responsibility to them. These things make a foreman a symbol of identification uniting the members of his section. Some foremen, moreover, institute periodic meetings of the entire section. Of particular significance is the establishment of "informal systems of job rotation,"⁸ which not only reduce monotony and make men more satisfied with work but also help to create group boundaries. If the men in a section intermittently change their positions on the line, their social situation is no longer very different from that of other work groups: each, sooner or later, finds himself close enough to every one of the others for informal contacts. Opportunities for recurrent interaction among all members of the section promote a common network of social relationships and a cohesive group.

But how could foremen become identified with the workers in their section and still discharge their managerial responsibilities? Walker, Guest, and Turner argue that a successful foreman must play a dual role, representing both his men and the management. It may also be, however, that assembly-line production itself has a bearing on the problem. The unrelenting movement of the con-

³ *Ibid.*, pp. 62-63, 116-17, 119-20. See also Ely Chinoy, *Automobile Workers and the American Dream* (Garden City, N.Y.: Doubleday & Co., 1955), pp. 62-72.

³ Charles R. Walker and Robert H. Guest, *The Man on the Assembly Line* (Cambridge, Mass.: Harvard University Press, 1952), pp. 67-68.

⁴ *Ibid.*, p. 132.

⁶ Charles R. Walker, Robert H. Guest, and Arthur N. Turner, *The Foreman on the Assembly Line* (Cambridge, Mass.: Harvard University Press, 1956), pp. 132-33.

⁷ *Ibid.*, p. 135.

⁸ *Ibid.*, p. 134.

veyor constrains workers to a certain output, relieving the foreman of responsibility for their productivity. But, of all his duties, it is only the exercise of control over subordinates that benefits from social distance. Thus, the fact that the conveyor system substitutes in part for the foreman as a mechanism of control makes it possible for him to identify himself with the workers without impeding operations.

To be sure, most of the foremen questioned did not think that assembly-line production facilitates their job. The foreman still has to supervise quality, keep the line manned, and tackle problems of adjustment and morale inevitable in repetitive work.⁹

There is quite a difference, however, between the problems the assembly line creates and those the foreman would have to face in its absence. High rates of turnover and absenteeism make the training of new workers and temporary replacements one of his major responsibilities. To ease the extra burden the absence of a worker places on the rest of the section, the foreman must be skilful in redistributing the work load and in negotiating with management for a quick replacement. The foreman must try to reduce turnover and absenteeism by making the work itself less arduous and the situation as satisfactory as possible. In discharging his responsibility for maintaining quality, he sees to it that the workers' tools are kept in good repair and that the materials they need are delivered to them at the proper time. All these tasks involve helping subordinates rather than making demands on them. The major exception is checking on the quality of performance, but even the significance of such checks is altered by the powerful constraint of the moving line.

IMPERSONAL CONSTRAINTS AND THE FLOW OF DEMAND

The impersonal constraints exerted by production-line methods change the flow of demand in the organization. The concept of flow of demand is derived from the concepts of origination of action and flow of work de-

veloped by Arensberg, Whyte, and Chapple in their studies of patterns of interaction among the members of an organization.¹⁰ Whyte shows, for example, that demands in the restaurant flow not only from management through supervisors down to waitresses and cooks but also from customers via waitresses and pantry personnel to the cooks. The fact that demands are made from two different sources often precipitates problems and conflicts, particularly when the person asked to do something considers himself superior to the one making the request.

Usually, demands flow primarily down the hierarchy from management through supervisors or foremen to workers, although staff experts provide an alternative route. The superior directs operations by giving his subordinates instructions and checking their work. Studies reveal, however, that frequent and detailed instructions and close checking of the subordinates' work is not the best method of supervision; on the contrary, such close supervision actually reduces productivity.¹¹ In other words, the flow of demand down the hierarchy, even if there are no conflicting streams, seems to impede effective operations.

Assembly-line production reverses the direction of the flow of demand. It is the conveyor that assures co-ordination and a certain level of productivity, not the directives

¹⁰ See especially Conrad M. Arensberg, "Behavior and Organization," in John H. Rohrer and Muzafer Sherif (eds.), *Social Psychology at the Crossroads* (New York: Harper & Bros., 1951); William F. Whyte, *Human Relations in the Restaurant Industry* (New York: McGraw-Hill Book Co., 1948); E. D. Chapple and Conrad M. Arensberg, "Measuring Human Relations," *Genetic Psychology Monographs*, XXII (1940), 3-147; and Joan W. Moore, "Restructuring Demands in Formal Organizations" (unpublished manuscript, University of Chicago Department of Sociology, 1956).

¹¹ See Robert L. Kahn and Daniel Katz, "Leadership Practices in Relation to Productivity and Morale," in Dorwin Cartwright and Alvin Zander (eds.), *Group Dynamics* (Evanston, Ill.: Row, Peterson Co., 1953), pp. 617-19. Cf. bibliography of the empirical studies on which this summary is based, pp. 627-28.

⁹ *Ibid.*, pp. 31-32.

of foremen. And, where an impersonal mechanism makes most necessary demands on workers, the major task of the foreman is no longer to issue directives but to be a trouble-shooter—to come to his subordinates' aid when they have difficulties. Hence, the typical interaction is initiated by a worker's demand for the foreman's help rather than by a demand by the foreman on the worker. This reversal is also manifest on the next level in the hierarchy. It has been pointed out that staff experts not merely advise management but, in effect, give orders to foremen and operators.¹² Foremen often feel that staff officials interfere with their work by making unreasonable demands,¹³ but the majority of the foremen in the assembly plant studied considered staff and service personnel helpful and felt free to call on them. When management and experts exercise control by planning assembly-line production, there is no need for issuing many directives to the foreman, and so most of the contacts between the foreman and superiors or staff officials come in consequence of his requests for help.¹⁴

The fact that the foreman is cast in the role of adviser and assistant to his subordinates affects interaction even on the occasions when he makes demands on them. He could not maintain this role if, upon discovering imperfections, he would curtly order a worker to improve the quality of his work. Moreover, since the foreman knows that the standardized quality requirements are generally accepted by workers—he thinks they want to do a good job—he is likely to lay the blame for failures upon the changing speed of the line or the worker's inexperience and

not on lack of effort. Thus demands for improvement are likely to take the form of guidance and training rather than commands. Workers, finally, are not so prone to blame the foreman if they have difficulty meeting standards as they might otherwise be, because the speeding line absorbs the brunt of their aggression.

The change in flow of demand engendered by the constraints of the assembly line distributes discretion more equitably between superior and subordinates. Although the demands of superiors are often worded as requests, it is difficult to refuse them—much more difficult than it is to refuse requests of subordinates. Demands that flow downward, no matter how polite, control the conduct of subordinates and restrict their freedom of action. To be sure, the foreman exercises control over workers even if demands flow upward; his counsel and guidance, in effect, influence the conduct of workers. Yet when the flow is upward, workers decide when to call upon the superior; not so when the flow is downward. The superior continues to exercise considerable discretion over granting requests for assistance and over what guidance to furnish, but instead of monopolizing discretion, he shares it with subordinates.

There is a fair amount of evidence that the exercise of discretion and responsibility increases satisfaction at work. Thus, Hoppock finds that, the higher the level of skill and responsibility, the greater is job satisfaction.¹⁵ Katz and Kahn confirm this finding, and they also show that on a given occupational level individuals whose superiors permit them to exercise discretion are more satisfied and less often absent from work than others.¹⁶ Feeling free to bring problems to the supervisor, for one thing, is inversely related to absenteeism. Coch and French

¹⁵ Robert Hoppock, *Job Satisfaction* (New York: Harper & Bros., 1935).

¹⁶ *Op. cit.*, p. 618; and Daniel Katz and Robert L. Kahn, "Some Recent Findings in Human-Relations Research in Industry," in Guy E. Swanson, Theodore M. Newcomb, and Eugene L. Hartley (eds.), *Readings in Social Psychology* (New York: Henry Holt & Co., 1952), pp. 657, 663-64.

¹² See Victor A. Thompson, *The Regulatory Process in OPA Rationing* (New York: King's Crown Press, 1950), esp. pp. 430-33.

¹³ See, e.g., Melville Dalton, "Conflicts between Staff and Line Managerial Officials," *American Sociological Review*, XV (1950), 342-51.

¹⁴ The only foreman for whom Walker, Guest, and Turner (*op. cit.*, p. 91) report quantitative data initiated fewer contacts with his subordinates than they did with him, and many more contacts with superiors and staff personnel than they did with him.

discovered that factory workers who participate in deciding on a change in production accept the change more readily than those without a voice in making it, perform much better once the change is made, and are less likely to quit.¹⁷ These data support the conclusion that the reversal in the flow of demand, since it increases the workers' discretion, contributes to their satisfaction.

The impersonal constraints of the assembly line decrease the worker's discretion on the job, but the upward flow of demand encouraged by these very constraints increases his discretion in his relationship with his foreman. Hence, assembly-line operations reduce work satisfaction at one point while enhancing it at another. This illustrates that a correlation between two "terminal" variables—a formal condition and its ultimate effect in the organization—can be misleading unless the intervening social processes are considered. Moreover, since it is unlikely that demand flows upward in all sections on the assembly line, it is essential to determine the other conditions in the organization on which the reversal of flow depends. In short, to analyze complex configurations, relationships between two variables must be elaborated externally by inquiring into additional necessary conditions as well as internally by examining intervening variables.

MULTIPLE CONSEQUENCES AND CHANGE IN ORGANIZATION

Assembly-line production is not the only impersonal constraint. Evaluation on the basis of published statistical records of performance is another: precise knowledge of how his work compares with others' constrains every employee to try to improve and so exercises control over operations without any direct intervention by superiors. Indeed, the statistical method of evaluation is a more adaptable mechanism of control than the assembly line. It lends itself to being used not only for manual but also for clerical and even professional work, and it

can serve to control qualitative as well as quantitative standards of performance, since a variety of errors and successes can be counted and recorded.

A study of two government agencies reveals that statistical records of performance, too, reverse the flow of demand between supervisor and operating officials.¹⁸ The direct influence the records exert on the performance of officials and the exact knowledge of accomplishments they furnish make it unnecessary for the supervisor to check on subordinates frequently and permit him to let them come to him for advice when needed. In fact, the more a supervisor relied on statistical records in his evaluation, the larger the proportion of interactions between him and them that they rather than he initiated.¹⁹ Even when a supervisor talks to a subordinate about improving his performance, statistical evidence transforms the significance of their conference; what might have been a much-resented critical opinion becomes an offer of help to make a better record. Evaluation on the basis of a record also makes it possible to give officials considerable discretion in the discharge of their duties. A quantitative record of performance and a conveyor belt are each impersonal mechanisms of control which, be the setting the semiprofessional work in the two government offices or the semiskilled work in the automobile plant, appear to have similar effects upon interaction between superior and subordinates.

Quantitative evaluation has a series of consequences in an organization. Its introduction in one government agency, for example, raised productivity, improved the relations between interviewers and their supervisors, and promoted a detached, impartial attitude toward clients. But it also fostered competitive tendencies which interfered with operations, and, in response to the new operating problems, new practices and patterns of interaction were developed.²⁰

¹⁸ Peter M. Blau, *The Dynamics of Bureaucracy* (Chicago: University of Chicago Press, 1955), pp. 33-40, 101-5.

¹⁹ *Ibid.*, p. 232, n. 7.

²⁰ *Ibid.*, pp. 44-68.

¹⁷ Lester Coch and John R. P. French, Jr., "Overcoming Resistance to Change," *Human Relations*, I (1948), 512-32.

The same aspects of the organization that make essential contributions to operations frequently also create conflicts and problems, and the unanticipated consequences of the adjustments instituted to cope with them may, in turn, produce further problems. Hence, there is a continual process of change in the organization.

DIMENSIONS OF ANALYSIS

The methodological problems posed here may be dealt with by distinguishing a structural, an organizational, and a developmental dimension in the analysis of formal organizations. There is at least one other dimension, which can be called "environmental": the analysis of the relationships between formal organizations and other social institutions, for example, of the connections between the economic or political system and formal organizations; of the relations between the organization of unions and that of companies in an industry; or of the role of the culture in personal conduct in the organization. Obviously, restricting the discussion here to the three intraorganizational dimensions does not mean that the environmental one is any less important.

Structural dimension.—The fact that it makes a difference whether workers on the assembly line merely have friendly relations with several fellow workers or whether a group structure has developed among them raises the question of how the distinct significance of social structure can be taken into account in systematic research. Ever since the early writings of Durkheim, and even though he modified his own position later, it has been recommended that the study of social structure confine itself to indexes that are independent of the behavior of individuals, such as the laws in a society, or the group's resistance to disruption.²¹ This procedure, however, entails the danger of

reifying the concept of group structure and ignoring the fact that it refers to a network of social relations between individuals which finds expression in their interaction. In any case, it is possible to investigate the effects of social structure by an alternative method.

This method consists of three steps. First, empirical measures are obtained that pertain to those characteristics of the individual members of the groups that have direct or indirect bearing on their relations to each other, such as group identification, sociometric choices, initiation of interaction, or promotions. Second, the measures that describe individuals in one respect are combined into one index for each group, and this index no longer refers to any characteristic of individuals but to a characteristic of the group.²² Examples of such group attributes are the proportion of members identified with the group, the average number of in-group sociometric choices, the degree of variation in rates of interaction, and homogeneity of interests. Third, to isolate a structural effect, the relationship between a group attribute and some effect is determined while the corresponding characteristic of individuals is held constant. An illustration will make this clear.

To test the hypothesis that the free flow of communication within a work group improves the performance of its members, two kinds of data have been collected in fifty work groups of about ten members each: measures of performance for every individual and the frequency with which he discusses his problems with another member of his own work group. We could investigate whether frequency of discussion and quality of performance are correlated in the entire sample of five hundred. But, if they were, it would show only that individuals who readily discuss their problems with others per-

²¹ See Émile Durkheim, *Rules of Sociological Method* (Chicago: University of Chicago Press, 1938), pp. 44-46. For a recent discussion of the issue see Neal Gross and William E. Martin, "On Group Cohesiveness," *American Journal of Sociology*, LVII (1952), 546-54.

²² On the distinction between empirical measures pertaining to individuals and the corresponding ones pertaining to groups see Patricia L. Kendall and Paul F. Lazarsfeld, "Problems of Survey Analysis," in Robert K. Merton and Paul F. Lazarsfeld (eds.), *Continuities in Social Research* (Glencoe, Ill.: Free Press, 1950), pp. 187-92.

form better, not that the network of communication in a group influences performance. A second test would be to divide the fifty groups into those with many and those with few members who readily discuss their problems and to determine whether average performance in the first category of groups is superior. A positive finding in this case, however, might merely be a reflection of a high correlation between the individual's readiness to discuss and his performance and thus still would not supply unequivocal evidence of the significance of the network of communication for performance. If, on the other hand, individuals are first divided on

they can use in their own work, or their self-confidence may be raised by his often coming to them for advice. Moreover, the observation that others have problems, too, and that they feel free to consult one another probably reduces an individual's anxiety over his own problems even before he starts discussing them.²³ It is, therefore, hypothesized that ego's discussions of his problems contribute to the performance of alters as well as to that of ego. (Of course, every member might alternate between playing the role of ego and that of alter, which means that reciprocity prevails in discussions.) If this is correct, and only if it is, one would actually obtain the finding described, that is, a relationship between the frequency of discussion in the group and performance when the individual's rate of discussion is controlled.

The general principle is that if ego's *X* affects not only ego's *Y* but also alters' *Y*, a structural effect will be observed, which means that the distribution of *X* in a group is related to *Y* even though the individual's *X* is held constant. Such a finding indicates that the network of relations in the group with respect to *X* influences *Y*. It isolates the effects of *X* on *Y* that are entirely due to or transmitted by the processes of social interaction.

A somewhat different structural effect is reported in a study by Stouffer and his colleagues.²⁴ Soldiers who have been promoted have more favorable attitudes toward chances of promotion in the army than those who have remained privates. However, soldiers in outfits a large proportion of whose members have been promoted have *less* favorable attitudes toward chances of promotion than others of equal rank in outfits with fewer promoted members. Thus, the frequency of promotions in a group has an unfavorable effect on these attitudes, while the individual's own promotion has the opposite effect. Being promoted raises the individual's status, but the promotion of many other

TABLE 1

INDIVIDUALS WHO DISCUSS THEIR PROBLEMS	GROUPS MOST OF WHOSE MEMBERS DISCUSS THEIR PROBLEMS	
	Often	Rarely
	Often	Rarely
Often.....	.85	.65
Rarely.....	.70	.40

the basis of their frequency of discussion, and it turns out that within each category of individuals about equally ready to talk about their problems those who belong to groups where frequent discussion is prevalent perform better than those in other groups, then it is demonstrated that the network of communication itself influences performance (see Table 1, where the differences between columns indicate the structural effect of the network of communication on performance). This finding would show that, even when the effect of the individual's discussion rate of his problems on his performance is eliminated, just to be in a group where communication flows freely improves performance—other things being equal.

What would account for such a finding, were it obtained? The fact that an individual discusses his problems with others has consequences for them as well as for himself. He may get specific advice, and, even when he does not, the discussion may clarify his thinking. At the same time the others may learn something from his discussion which

²³ See Blau, *op. cit.*, pp. 105-16.

²⁴ Samuel A. Stouffer *et al.*, *The American Soldier*, Vol. I (Princeton, N.J.: Princeton University Press, 1949), pp. 250-54.

members of his group depresses his status relative to theirs. In short, the promotion of a number of egos decreases the relative status of alters and increases the relative status of egos. Since ego's X has the opposite implications for alters' Y and for ego's Y in this case, the structural effect and that of the corresponding characteristic of individuals are in opposite directions.²⁵

Social norms also have structural effects. Workers who firmly believe that it is wrong to be a "rate-buster" are probably less likely than others to exceed informal standards of output. Even workers who see nothing wrong with rate-busting, however, may work slower than they otherwise would if most members of the group believe rate-busting wrong. The reason is, of course, that prevailing social standards are enforced throughout the group and, therefore, influence the conduct of those who do not fully accept them as well as of those who do. Again, ego's X influences both alter's Y and ego's Y . If the pressure of the group is successful, however, one-time deviants will not only conform to the expectations of the majority but sooner or later incorporate them in their own thinking. Once this happens and virtually all members of some groups condemn rate-busting while hardly any of others do, it is no longer possible to use the proposed method of determining structural effects, for it requires a sufficient number of individuals who reject the norm in groups which, on the whole, accept it and of individuals who accept the norm in groups which, on the whole, reject it. This indicates an important limitation of the method. It reveals only the present, not the past, effects of the normative structure of groups. The prevalence of a normative orientation in a group may have three effects upon deviants: intensify their deviant conduct as a reaction to being alienated from the majority, constrain them to conform against their own convictions, or convert their very thinking. Although all

three are effects of the social structure, the last would not find expression in what has here been called a structural effect.

A different structural effect is illustrated by a finding of Lipset, Trow, and Coleman.²⁶ If the members of a small printing shop are in substantial agreement on political issues, they are more prone to be active in union politics than if there are considerable political differences among them. Not whether the members of a shop are liberal or conservative but their consensus is significant, whatever their political opinion. Wide divergences in political viewpoint among the members of a work group incline them to avoid political topics of conversation to avoid arguments. Sufficient political consensus to provide a basis for cordial discourse, on the other hand, encourages political talk at work, and this stimulates interest in the political activities of the union. In formal terms, if the joint occurrence of X , or of non- X , in ego and alter affects Y , the variance of X in the group will have an effect on Y which is independent of any possible relationship between the individual's X and his Y .

Organizational dimension.—The structure of work groups is, of course, profoundly influenced by the formal organization of which they are parts. Although the analysis of formally established organizations is generally concerned with larger social units than work groups, the distinction between the structural and the organizational dimension is analytical, not one of size. To speak of the interrelations within a social system may refer either to the *social* relations between individuals or groups or to the interdependence of abstract elements in the organization, say, the relationships between personnel policies, supervisory practices, and interaction among workers.²⁷ The term "organizational dimension" is used to denote the lat-

²⁶ Seymour M. Lipset, Martin Trow, and James Coleman, *Union Democracy* (Glencoe, Ill.: Free Press, 1956), pp. 165-67.

²⁷ For a more general distinction between "part" concepts and analytical elements see Talcott Parsons, *The Structure of Social Action* (New York: McGraw-Hill Book Co., 1937), pp. 30-40.

²⁵ Another instance: competitive interviewers in an employment agency produce more than others, but the prevalence of competitiveness in a group reduces its productivity (see Blau, *op. cit.*, pp. 61-65).

ter type of analysis, whose focus is the configuration of interdependent elements in the organization.

As has been pointed out, the establishment of a relationship between two characteristics of organizations is merely a starting point for elaborating it internally as well as externally. Internal elaboration involves a search for intervening variables, without which the finding that an antecedent condition has a certain ultimate consequence cannot be interpreted and may even be misleading. For example, when it was found that the impersonal control exerted by statistical records of performance gave rise to more impersonal and impartial treatment of clients in an employment agency, one might, on first impression, conclude that impersonality is simply transmitted from the exercise of control down to the contact with clients. Actually, complex intervening processes were responsible for the relationship. Performance records stimulated employment interviewers to concentrate on making many placements and induced them to eliminate all considerations that had no bearing on making placements—in short, all personal considerations—in their treatment of clients. This disinterested approach often created conflicts with clients, which made it difficult for interviewers to remain detached and neither become angry at clients nor modify decisions in order to pacify them. However, the practice developed of relieving the tensions generated by these conflicts by complaining or joking about clients in informal discussions with colleagues. These friendly, *not* impersonal, interactions among interviewers rendered conflicts with clients less disturbing, so that it was easier to maintain an impersonal attitude toward them even at the risk of conflict.²⁸

In analyzing an organization, the major independent variables are the formal institutions in terms of which social conduct is organized: the division of labor, the hierarchy of offices, control and sanctioning mechanisms, production methods, official rules and regulations, and personnel policies, and so on. The major dependent variables are the

results accomplished by operations and the attachment of its members to the organization, as indicated by productive efficiency, changes effected in the community (say, a decline in crime rates), turnover, satisfaction with work, and various other effect criteria. To explain the relationships between these two sets of abstract variables, it is necessary to investigate the processes of social interaction and the interpersonal relations and group structures.²⁹ In dealing with these patterns of conduct, psychological processes cannot be entirely ignored. To be sure, the student of organization is not concerned with the effects of psychological characteristics but with those of conditions in the organization on social conduct. However, psychological processes are the *intervening* variables drawn on to explain why social conditions give rise to certain patterns of conduct.³⁰ Statistical records brought about more impartial treatment of clients, for example, because they motivated interviewers to exclude all irrelevant personal considerations from their decisions in making placements. In sum, intervening psychological variables explain why the conditions in the organization lead to given processes of social interaction, and these social processes, in turn, must be examined to account for the relationships between conditions in the organization and the results they accomplish.

The external elaboration of a proposition that one factor influences another involves a search for the other conditions necessary for the observed effect. Granted that impersonal mechanisms of control tend to reverse the flow of demand, on what other conditions does the reversal depend? Concern with the significance of a combination of conditions introduces the conception of organization as a configuration of interdependent elements. It is often assumed that the

²⁹ For a method of testing the hypothesis that a given intervening variable or set of intervening variables accounts for the relationship between two variables see Kendall and Lazarsfeld, *op. cit.*, pp. 147–51.

³⁰ This is the main implication of Weber's concept of *Verstehen* (see Max Weber, *The Theory of Social and Economic Organization* [New York: Oxford University Press, 1947], pp. 87–107).

²⁸ Blau, *op. cit.*, pp. 73–95.

concept of *Gestalt* defies quantitative analysis, but this assumption seems unwarranted. If the concept of *Gestalt* means that the organized arrangement of elements in a larger whole has a significance of its own, not attributable to the specific character of the elements, then analysis of variance furnishes a method for its systematic investigation. Thus it is possible to test the hypothesis implied by Weber that administrative efficiency is the result of a combination of various characteristics in a bureaucracy,³¹ provided that empirical data on these characteristics and on efficiency can be obtained for a large sample of bureaucratic organizations. Although the empirical measures describe only the elements and not their configuration, the significance of the latter would become apparent in the analysis of variance, for this statistical method would supply information not only on the contribution of each characteristic to efficiency but also on the additional contribution made by their combination. The so-called interaction effects would furnish quantitative measures of the significance of the *Gestalt* by abstracting the effects due to the configuration itself from the sum of the effects of its components. While the cost in time and money of such a project is virtually prohibitive, it is nevertheless important to realize that there are no inherent obstacles to the systematic investigation of the complex configurations in formal organizations.

Practical problems, however, cannot be brushed aside. Since it is rarely possible to establish generalizations on the basis of evidence from a representative sample of formal organizations, substitute methods have to be developed, one of which might be internal comparison. The great variations existing in large organizations have not been sufficiently exploited for systematic research. Guided, apparently, by a mistaken notion of *Gestalt*, many investigators are concerned with the "typical" foreman or the over-all pattern of "human relations" in a company instead of deriving limited generalizations from the differences in the organization of the various divisions or departments. In any case, gener-

alizations about the total configuration cannot be supported by evidence collected in only one organization. A second substitute method, which permits making tentative generalizations about total organizations, might be the secondary analysis of a number of empirical studies of formal organizations.³²

Developmental dimension.—Change in the organization is the result of the very interdependence between elements that is often assumed to imply a stable equilibrium. Even if there were a perfect organization with no problems, changes in its environment would soon create some. But internal as well as external conditions generate change in the organization, since innovations instituted to solve one problem, as already mentioned, have a variety of repercussions, some of which are likely to produce other problems.

A mistake we often tend to make is that the world stands still while we are going through the process of a given adjustment. And it doesn't. Facts change, we must keep up with the facts; keeping up with the facts changes the facts. . . . When we think that we have *solved* a problem, well, by the very process of solving, new elements or forces come into the situation and you have a new problem on your hands to be solved.³³

Interdependence entails dilemmas: efficient operation in a large organization depends on many different conditions, and the practices instituted to establish one of these conditions do not remain solely means for this end but have implications for others; and, since the conditions required for optimum operations are diverse, the measures to improve them are often incompatible.

³² For this to be fruitful, however, research procedures would have to be better standardized or, at least, more accurately reported than they usually are. Research centers that regularly conduct studies of formal organizations, such as those at the University of Michigan and at Yale University, have special opportunities for co-ordinating various investigations in the interest of deriving generalizations about organizations.

³³ Mary Parker Follett, "The Process of Control," in Luther Gulick and L. Urwick (eds.), *Papers on the Science of Administration* (New York: Institute of Public Administration, 1937), p. 166.

³¹ *Ibid.*, pp. 329-41.

Thus, effective administration is contingent on uniform adherence to regulations as well as on adaptability to a variety of specific situations, but bureaucratic pressures compelling strict conformity to rules also give rise to rigidities that interfere with the adaptability needed to handle special cases.³⁴ Although evaluation on the basis of accomplished results encourages the responsible performance of complex tasks, it simultaneously engenders anxieties which impede decision-making.³⁵ Assembly-line methods, while increasing productivity, lead to absenteeism and make operations particularly dependent on regular attendance, to boot. Incompatibility of means, not simply lack of administrative foresight, is responsible for recurrent problems requiring adjustment in the organization and thus for its continual development.

The pattern of change in formal organizations can be described as a dialectical development. The process of solving some problems while frequently creating others is also a learning process in which experience is gained. On the one hand, efforts at adjustment shift from one problem to another as new difficulties arise when old ones are resolved. After assembly-line production has been instituted, reduction of absenteeism and turnover replaces technical questions of coordination as the major area of concern. On the other hand, as one type of problem recurs, it does not remain the same type of problem, since cumulative experience changes the orientation of the members of the organization toward it. This is so of problems confronting work groups as well as of those confronting management. Work groups experienced in maintaining solidarity against excessive demands of superiors will be less threatened by a new and unreasonably demanding foreman than groups that never had to cope with the problem, just as management will find it easier to combat absenteeism if it has successfully done so on previous occasions.

³⁴ See Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1949), pp. 153-57.

These processes of development are further complicated by changes in personnel which result in the loss of accumulated experience despite the profusion of written records typical of bureaucratic organizations. Of particular importance is the fact that knowledge of the networks of informal relations can be acquired only through direct experience. Gouldner shows that a new manager's inevitable ignorance of informal relations constrains him to resort to formal procedures in discharging his responsibilities even if he is convinced that informal procedures are more effective.³⁶ Similarly, turnover of personnel on lower levels in the hierarchy undermines the cohesiveness of work groups and threatens informally established co-operative practices.

Conflicts of interests between management and non-managerial personnel, and between other groups, are an additional source of dialectical change. What constitutes adjustment for one group may be quite the opposite for the other, since different interests serve as criteria for defining adjustment, and, when issues between workers and management have been resolved on one level, new ones on a different level often arise. After satisfactory working arrangements have been agreed upon, management introduces new machines, which then create new problems of adjustment for workers. After the union has achieved the right of collective bargaining, it uses it to raise the issue of pensions. Indeed, independent of conflicts between union and management, the successful attainment of an objective stimulates efforts to make further improvements and seek new fields to conquer, and this succession of goals as they are achieved by more ultimate ones is still another force that produces change in the organization.

In sum, dialectical organizational developments are generated by different patterns of change superimposed upon one another.

³⁵ Blau, *op. cit.*, pp. 103-4.

³⁶ Alvin W. Gouldner, *Patterns of Industrial Bureaucracy* (Glencoe, Ill.: Free Press, 1954), pp. 70-101.

The process of adjustment in the organization changes the kind of difficulties that demand foremost attention, since new ones arise as old ones are resolved, and, simultaneously, experience alters the orientation with which problems are approached. Hence, as efforts at adjustment are turned from one problem to another, their effectiveness tends to increase. There are, however, several different spirals of adjustment of this sort rather than a single one, because conflicts of interests between various groupings in the organization produce diverse conceptions of adjustment. When issues created by these conflicts are settled, the conflicting developments continue and new issues emerge. Thus, the existence of divergent dialectical processes gives rise to yet another dialectical process of adjustment—a spiral linking the other spirals, as it were. The complexity of these developments is further increased by two conditions. Changes in personnel, which are disruptive, notwithstanding attempts to preserve continuity through written records and formalized procedures, reduce the level of adjustment. And the additional demands made on the organization as the result of striving for new objectives once old ones have been successfully attained create new problems of adjustment.

The systematic study of these processes of development requires that the time dimension be taken into account in the investigation of formal organizations. To be sure, there are a number of empirical studies of change in organizations; for example, Richardson and Walker trace the repercussions of a change in production methods in a factory;³⁷ Gouldner examines the consequences of a change in management for an industrial organization;³⁸ Selznick analyzes modifications in an organization resulting from its

adaptation to a hostile environment;³⁹ and Michels deals with the changes generated by the very establishment of a formal organization.⁴⁰ But the empirical data for most of these studies were collected at one time,⁴¹ and the patterns of change had to be inferred subsequently either from written records or from other evidence. Although preferable to ignoring change completely, the procedure is far from ideal.

The adaptation of the panel technique to the study of formal organizations would yield reliable evidence on developmental processes. It would involve systematic observation as well as interviewing in an organization at repeated intervals, perhaps a year or more apart. Precise indications of both informal and formal changes would be supplied by this method, and intensive interviewing about the changes discovered and analysis of pertinent records would provide information on the social and psychological processes leading to them. Although continuous observation for several years, permitting the investigation of changes as they occur, is preferable to collecting data at periodic intervals, it is rarely feasible. Studies based on repeated interviews with the same respondents have greatly contributed to accurate knowledge about change in opinions and attitudes.⁴² This panel design, properly adapted to research on formal organizations, may well prove equally fruitful in the systematic study of organizational developments and, specifically, in testing the hypothesis advanced here that such developments are dialectical.

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⁴⁰ Robert Michels, *Political Parties* (Glencoe, Ill.: Free Press, 1949).

⁴¹ My own study is no exception. The investigation by Jaques should be mentioned as a notable exception; change in a factory is analyzed on the basis of continuous observation for two and a half years, although no quantitative data were collected (see Elliott Jaques, *The Changing Culture of a Factory* [New York: Dryden Press, 1952]).

⁴² See, e.g., Paul F. Lazarsfeld, Bernard Berelson, and Hazel Gaudet, *The People's Choice* (New York: Duell, Sloan & Pearce, 1944).

³⁷ F. L. W. Richardson, Jr., and Charles R. Walker, *Human Relations in an Expanding Company* (New Haven, Conn.: Yale Labor and Management Center, 1948).

³⁸ *Op. cit.*

³⁹ Philip Selznick, *TVA and the Grass Roots* (Berkeley: University of California Press, 1949).

WAR BRIDES IN HAWAII AND THEIR IN-LAWS

YUKIKO KIMURA

ABSTRACT

In a sample of Japanese war brides 48 per cent of those with Japanese husbands have very congenial relationships with their in-laws. These were the only war brides in the sample who share the cultural background of their husbands' families. They are in contrast to the larger proportion of war brides who have congenial relationships with their in-laws to be found among the Japanese wives of non-Japanese (72 per cent), European wives of Japanese (60 per cent), and European wives of non-Japanese (55 per cent). Evidently, sharing the cultural background tends to restrict personal relations to the prescribed forms and to hamper spontaneous interaction, while, where cultural backgrounds differ, behavior is determined by the necessity to adjust one's self to another on the basis of the situation at hand. There is then mutual role-taking and hence spontaneous identification of one's self with the other and appreciation of one another.

Sumner's notion of the persistence of the mores and the difficulty of changing them and the widely current concept of acculturation suggest that peoples behave toward one another primarily on the basis of their diverse cultural backgrounds. The assumption seems to be that peoples of like or similar cultures may establish quick and easy relations, while great differences impede and complicate them.

Hawaii is a community which subjects such a view to intensive tests. A peculiarly challenging postwar development there has been the return of local men of various ancestry from service abroad with brides of backgrounds different from their own. This study describes by an inductive analysis how the assumptions above mentioned are called into question. It deals with one aspect, namely, in-law relations, of a more extensive study of war brides in Hawaii now under way. Three hundred and twenty-four war brides have been interviewed since December, 1953. These are being considered under four classes: European wives of Japanese husbands; European wives of non-Japanese husbands; Japanese wives of non-Japanese husbands; and Japanese wives of Japanese husbands.¹

Each group is further divided into three: *good*, those who indicated their marriage to be one of great happiness or satisfaction, most of them expressing their desire to repeat it if they had it to do over again; *fair*, those who indicated that they were getting

along but that their situations could be better; and *poor*, those who are actually very unhappy and who expressed regret or bitterness (Table 1).²

In summary, Japanese wives of non-Japanese are found to represent the happiest group, 75 per cent reporting good and 10 per cent poor adjustment. Next happiest appear to be the European wives of Japanese husbands, 70 per cent of whom report good and 15 per cent poor adjustment. European

¹ There is no available record of the number of European war brides in Hawaii. Those interviewed came from twelve different countries but are treated as one group because of the similarity of their problems in coping with local situations. The number of Japanese war brides is roughly estimated by the Japanese Consulate in Honolulu as approximately two thousand. The number of both European and Japanese war brides interviewed is believed to be sufficiently large to represent a cross-section of their respective groups for the purpose of this study.

For simplification, men of Japanese ancestry are classified as Japanese, and all other men are classified as non-Japanese, throughout this study.

Non-Japanese husbands include Chinese, Koreans, Hawaiians and part-Hawaiians, Filipinos, Portuguese, Puerto Ricans, and a small number of other Caucasians. The Japanese wives of Japanese are the only war brides who share the cultural background of their in-laws.

² In this group the following divorces had been secured prior to the interview: by four of nine European wives of Japanese; by three of fifteen European wives of non-Japanese; and by one of thirteen Japanese wives of Japanese. Others were either seeking divorce or remaining married for financial or religious reasons. One European wife was later killed by her husband who was not Japanese.

wives of non-Japanese husbands rate next in respect to good (51 per cent) and highest in respect to poor adjustment (19 per cent). Just over half of the Japanese wives of Japanese consider their marriage to be neither very happy nor very unhappy.

To what extent does their adjustment reflect their relations with in-laws? In Table 2 "Good" denotes very congenial relations with in-laws, "Poor" very strained relations with them. Under "Fair" are included those who carry on close kinship activities without

TABLE 1

324 WAR BRIDES IN HAWAII BY ANCESTRY AND ADJUSTMENT TO MARRIAGE

ANCESTRY	TOTAL		Good		Fair		Poor	
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent
Wife European, husband Japanese..	60	100	42	70	9	15	9	15
Wife European, husband non-Japanese.....	80	100	41	51	24	30	15	19
Wife Japanese, husband non-Japanese.....	60	100	45	75	9	15	6	10
Wife Japanese, husband Japanese..	124	100	48	39	63	51	13	10

TABLE 2

324 WAR BRIDES IN HAWAII BY ADJUSTMENT TO MARRIAGE, ANCESTRY, AND IN-LAW RELATIONS

ADJUSTMENT TO MARRIAGE AND ANCESTRY	TOTAL		Good		Fair		Poor		No IN-LAWS	
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent
<i>Total:</i>										
Wife European, husband Japanese.....	60	100	36	60	20	33	4	7	0	0
Wife European, husband non-Japanese.....	80	100	44	55	20	25	13	16	3	4
Wife Japanese, husband non-Japanese.....	60	100	43	72	8	13	4	7	5	8
Wife Japanese, husband Japanese.....	124	100	59	48	44	35	14	11	7	6
<i>Good adjustment:</i>										
Wife European, husband Japanese.....	42	100	33	79	8	19	1	2	0	0
Wife European, husband non-Japanese.....	41	100	32	78	4	10	2	5	3	7
Wife Japanese, husband non-Japanese.....	45	100	36	80	4	9	0	0	5	11
Wife Japanese, husband Japanese.....	48	100	43	90	3	6	0	0	2	4
<i>Fair adjustment:</i>										
Wife European, husband Japanese.....	9	100	2	22	5	56	2	22	0	0
Wife European, husband non-Japanese.....	24	100	11	46	10	42	3	12	0	0
Wife Japanese, husband non-Japanese.....	9	100	6	67	2	22	1	11	0	0
Wife Japanese, husband Japanese.....	63	100	15	24	37	59	6	9	5	8
<i>Poor adjustment:</i>										
Wife European, husband Japanese.....	9	100	1	11	7	78	1	11	0	0
Wife European, husband non-Japanese.....	15	100	1	7	6	40	8	53	0	0
Wife Japanese, husband non-Japanese.....	6	100	1	17	2	33	3	50	0	0
Wife Japanese, husband Japanese.....	13	100	1	8	4	31	8	61	0	0

being very congenial with in-laws; those who had in the past very strained relations while living with in-laws but are now enjoying cordial relations; those who are very congenial with one or more of the in-laws while having very strained relations with one of them; and, finally, those who do not associate with in-laws without any friction.

As shown in Table 2, a large majority of the well adjusted, who are enjoying great satisfaction in their marriage, are also enjoying very congenial in-law relations. Conversely, in general, a large proportion of the most unhappy group have very strained in-law relations. A large proportion of war brides as a whole have very congenial relations with them, characterized by spontaneous responsiveness and mutual helpfulness,³ intimate in-group identification and fellowship of kin such as is often revealed in such expressions as "Just like my own mother!" "Just like my own sister!" or in a comment on being treated "just like their own daughter," or in their constant participation in social and recreational activities if they live near each other, but, if not, by telephone calls, letters, vacation visits, etc. There is a great deal of mutual appreciation of, say, personal needs or of abilities, such as culinary skill.

Among those who have fair relations with in-laws, 35 per cent of the European wives of Japanese and 67 per cent of the European wives of non-Japanese do not associate with their in-laws. All other wives, both European and Japanese, go to at least occasional gatherings with their in-laws.⁴

Only seventeen, or 12 per cent, of all the European wives and eighteen, or 10 per cent, of all the Japanese wives have very strained relations with their in-laws. Twelve, or 75 per cent, of the troubles of these European wives and fourteen, or 74 per cent, of the troubles of these Japanese wives are with the

mother-in-law.⁵ Other cases concern sisters-in-law. Only one European wife and one Japanese wife had trouble with other relatives. Only two of the Japanese wives of Japanese husbands have very strained relations with their fathers-in-law. One of these was suffering from mental illness. In general, the father-in-law is described as a very kind person, sympathetic and helpful to the daughter-in-law. However, only a few of them actually intervene in the conflict between their wives and daughters-in-law. None of the European wives who have acute conflict with in-laws associates with them. Three of the four Japanese wives of non-Japanese and three of the fourteen Japanese wives of Japanese who have acute family trouble do not associate with their in-laws, while the rest of them still do so occasionally. The European wives show more independence than the Japanese wives in coping with the situation. Only one European wife of a Japanese, one European wife of a non-Japanese, and one Japanese wife of a Japanese secured divorces almost solely because of trouble with in-laws.

In general, two types of relations with in-laws seem to be manifested, namely, *complementary* and *competitive*. In the case of complementary relations the persons concerned support each other, and in consequence each side is regarded as being important for the welfare of the other. The natural tendency is to desire a favorable situation for the other; one sees the situation from the other's point of view, and this results in the incorporation of the problems of the other in conceiving of the situation at hand. As a result, need is met often in advance of request, stimulating further appreciation of the other and increasing further interaction of the same sort.

⁵ Eighteen, or 13 per cent, of the European war brides and thirty-one, or 17 per cent, of the Japanese war brides have no father-in-law. Eight, or 6 per cent, of the European war brides and nine, or 5 per cent, of the Japanese war brides have no mother-in-law. One hundred and eleven, or 79 per cent, of the European war brides and one hundred and thirty-four, or 73 per cent, of the Japanese war brides have both father-in-law and mother-in-law.

³ The mother-in-law's helpfulness is particularly evident at childbirth.

⁴ Two European wives of non-Japanese men and four Japanese wives of Japanese men were still living with their in-laws at the time of the interview.

A practical situation, as the daughter-in-law's taking charge of the household to relieve an employed mother-in-law or the mother-in-law's taking care of her grandchildren while the daughter-in-law is out at work, establishes a naturally complementary relationship. Complementary relations are characterized by compatibility in personal disposition, interests, tastes, needs, etc.

In competitive relations the persons involved have opposing interests and needs: a situation favorable to the one is regarded as hurtful to the other. For example, if the mother-in-law has depended on her son for financial support, his marriage means her loss in favor of his wife. Very often, then, the mother-in-law will regard her daughter-in-law as an invader of her prerogatives. Similarly, the mother-in-law's exclusive claim upon son or grandchildren is often regarded by the daughter-in-law as an encroachment upon her rights as wife and mother. If the parents-in-law regard the marriage of their son as a continuation of their family, while their son and his wife consider they are establishing a new home of their own, there is a conflict of interests. Living with in-laws in crowded quarters forces the members of the household into a competitive relation with one another for space and comfort, and differences in customs and personal habits are annoying to an exaggerated degree.⁶

Table 2 reveals the startling fact that among the Japanese wives of Japanese the proportion having very congenial in-law relations is small (48 per cent), while the proportion suffering acute conflict is comparatively large (11 per cent) in spite of their being the only war brides with the same cultural background as the husband's family. This is in contrast to all other war brides. Is

⁶ In all instances severe conflict began while the couple was living with in-laws, especially when crowded. However, living with in-laws alone does not necessarily cause frictions: from 86 to 95 per cent of all the war brides who have very congenial relations with their in-laws have lived with them; indeed, nearly half of them did so for from six months to eight years.

there a greater element of conflict between those of the same culture?

The complaints often expressed by Japanese war brides indicate they differ from their in-laws in the definition of what is desirable or not. For example, the former consider reading books and articles about household responsibility or the rearing of children desirable for a housewife, while the latter consider such a thing a waste of time and unproductive. To the Japanese in-laws an additional member of the family should mean an additional producer of family income. The parents-in-law, keenly conscious of their hard work and sacrifice in raising their sons, remind their daughters-in-law of it constantly. In Hawaii, the words "filial piety" are much more used than in Japan, and the parents strongly emphasize the children's obligation⁷ and look forward to a daughter-in-law who will fulfil this expectation.

In contrast, none of the Japanese war brides interviewed considered their cardinal duty after marriage to be service to their parents-in-law. All of them came to Hawaii with the intention of establishing a home for themselves and their husbands. Speaking of their marriage as "international," all of them regarded their marriage as with "Americans" regardless of the latter's Japanese ancestry.⁸ Moreover, their own parents in Japan had urged them to be independent of their Hawaiian in-laws, making unfavorable remarks about the emigrants to Hawaii and complaining that the marriage is not a compliment to their family or lineage. Thus, the Japanese war brides had had an unpromising preparation for life with their husbands' families.

⁷ All the Japanese war brides said that their parents almost never used the words "filial piety" and that their parents consented to the marriage on one condition, namely, that their daughters would be happy.

⁸ While in Japan, most of them prepared themselves to live like "Americans" by learning to speak English and attending the classes sponsored by the American Red Cross or other organizations in American custom and etiquette including table manners, American housekeeping and cooking, child care, etc.

The mother-in-law who herself came to Hawaii as a young bride decades ago often compares the poverty and hard work of her early married years to the comfort and household conveniences as well as to the various gestures of consideration from her son which her daughter-in-law enjoys. Considering her present comfortable living as a result of and reward for her years of sacrifice, the mother-in-law sees her daughter-in-law as undeserving of the luxury she did not work for. In extreme cases the mother-in-law may refuse to let her daughter-in-law use the modern appliances. To the mother-in-law who recalls her drab working clothes, plain appearance, her role of submission to a dominating husband, her self-denial, etc., her daughter-in-law's American clothes and makeup, her independence in thinking and acting, are not only unexpected and puzzling but offensive and "American" and completely unbecoming a Japanese daughter-in-law.

The traditional role of a daughter-in-law is to be subordinate to her mother-in-law as an apprentice in housewifely duties. From the standpoint of the daughter-in-law, however, her mother-in-law is not qualified to assume a normal role of a mother-in-law. In general, where the in-laws have a high economic standard, there is greater inclination on the part of the war brides to identify themselves with the former. Most of those who have expressed great satisfaction in their marriage have in-laws who are well established economically. In most instances, however, the Japanese war brides are conscious that they have married down into a lower class. Recognizing their superiority to their in-laws and disapproving of the habits and manner of living of the latter, they show little inclination to be identified with them. On the other hand, the in-laws are conscious of the fact that their daughter-in-law knows about things Japanese and is therefore in a position to criticize them. There is constant awareness of each other's approval or disapproval on the basis of their common cultural background.

The most conspicuous problematic ele-

ment in the common cultural background is the language. All the Japanese war brides speak standard Japanese. In contrast, their in-laws, being from agricultural areas, had very little opportunity for the daily use of standard Japanese either in Japan or in Hawaii. Moreover, their original dialects have further deteriorated by being mixed with various languages in Hawaii. The Japanese language, especially the spoken language, uses an intricate system to differentiate among social classes and hierarchical statuses. An expression normally used in addressing a person of a lower class, if directed at one of a higher class, is taken as a sign of impropriety or ignorance. Because the majority of war brides consider themselves as of a better social class than their in-laws and more highly educated than they, they find the language used in addressing them by their in-laws insulting to the point of being unendurable. Not only the absence of intricate differentiation in the language but also some of the commonly used expressions are considered vulgar or extremely uncouth. Hence there is very little disposition on the part of daughters-in-law to learn to use the language of their in-laws; thus they appear to remain aloof.

The process of their Americanization is slow. While many of them regret their inability to speak English freely, they do not feel a real necessity to learn to speak it. Because of their insufficient English, they cannot become part of the larger community, yet they do not wish to be classed as first-generation Japanese. They have very little in common with the Hawaiian-born Japanese, even though of the same age. If Japanese war brides have established a reputation for gathering by themselves, criticizing their in-laws and the backward local Japanese, and nostalgically speaking of Japan, it is an indication of their isolation from both the Japanese community and the larger community.

In contrast, a large proportion of the Japanese war brides of non-Japanese (72 per cent) are enjoying very congenial in-law relations. Most have become so intimately a

part of their husbands' families and kin and are so thoroughly carrying on social activities with them that they do not seem to be interested in making friends outside. Aware that they are new members of their husbands' groups, without knowledge or experience of their respective cultures, they are disposed to look to their in-laws for assistance and advice. Their exclusive identification with their husbands' families is reinforced by their desire to be beyond the gossip of the Japanese community, especially of the first generation, the majority of whom still disapprove of intermarriage of the Japanese. The non-Japanese in-laws, too, seem to prefer not to associate with the local Japanese, especially the first generation, and apparently regard their daughter-in-laws' parents in Japan as different from the local Japanese.

The fact of their being with non-Japanese and the necessity of speaking English have encouraged them to participate in the community, for instance, the parent-teachers' association. Such experience in turn reinforces their desire to become part of the larger community. They look forward to eventual naturalization as a matter of course.

If, however, relations with their in-laws are strained or if they have no in-laws, Japanese war brides are quite isolated. If they are willing to face the searching questions as well as the pity or contempt of the local Japanese, they can mingle with them, but, more often, they avoid them. If they live in a cosmopolitan area such as a veterans' housing project, they become neighborly with non-Japanese residents in similar circumstances, such as war brides from European countries, and may eventually enjoy with them a very intimate and helpful relationship similar to kinship.

European war brides married servicemen from Hawaii without much awareness of marrying into Chinese, Filipino, Hawaiian, or Japanese families. After they arrived in Hawaii, they began to realize it and the fact that it was now their lot to become a member of the ethnic group in question. The ex-

periences of the European wives of Japanese and of non-Japanese are very similar, the difference being a matter of degree rather than kind. For this reason, and for comparison with Japanese war brides who have Japanese in-laws, the discussion which follows will be concentrated on European war brides who have Japanese in-laws.

As Table 2 shows, a much larger proportion of the European wives of Japanese have good relations with in-laws than do the Japanese wives of Japanese—60 against 48 per cent. In general, the in-laws in Hawaii expected to have difficulty with a daughter-in-law from Europe, and, when they did not, they felt great relief and began to appreciate the girl and to compare her with troublesome daughters-in-law from Japan. They spoke openly of their preference for the European daughter-in-law, claiming that the Japanese war bride is apt to assert her superior education and look down upon her parents-in-law, whereas the European girl overlooks shortcomings. Without knowledge and experience of the Japanese culture, the European war bride has no basis for criticizing the propriety of her in-laws' behavior; she assumes that everything they do is simply the Japanese way. She and her in-laws, moreover, have to depend upon English, a foreign language for both sides. Therefore, as long as the purpose of communication is achieved, both sides are satisfied without being critical of each other's speech.

From the beginning, the in-laws expected the daughter-in-law from Europe to behave independently on the following grounds: their conception of the white race as of a higher social class than they, their lack of experience with members of the white race as part of the immediate family, the daughter-in-law's unfamiliarity with the Japanese culture and hence with the traditional role of a Japanese daughter-in-law, and their own unfamiliarity with her language and culture.

On the other hand, recognizing her in-laws' inadequacy as spokesmen for things American, the daughter-in-law looks to other people for assistance in becoming

American. To that extent her Japanese in-laws lose prestige in her eyes. Moreover, the fact that almost all the European war brides speak and understand English better than their Japanese in-laws enables them to be more versed in American culture despite their recent arrival, and this naturally makes them independent.

If their in-laws are well established economically and in a position to assist them in their adjustment in Hawaii, however, then the European war brides tend to identify themselves with their husbands' families more fully and thus become more completely incorporated. Those who have very congenial relations with their in-laws seem to associate with the latter more exclusively. Usually, the Japanese in-laws find their European daughter-in-law to be unsophisticated and therefore are more at ease with her than with local Caucasians. The fact that European war brides are still in the process of learning about America and speak whatever kind of English is prevalent in the family reduces their in-laws' self-consciousness.

When does a strained relationship arise? When the in-laws regard their European daughter-in-law as they do a Japanese daughter-in-law and expect her to accept the traditional role, her non-compliance is disturbing. And when the European war bride

and her in-laws are placed in competition with each other, conflict arises.

In general, European war brides do not wish to remain in the entirely Japanese neighborhood where their in-laws reside, although practically all of them have acquired a taste for Japanese food and other cultural items. They strive to move into better middle-class cosmopolitan residential districts. Fundamentally, European war brides aim to become part of the larger American community. Almost all take their eventual naturalization as a matter of course, and those who have been naturalized are proud of their American citizenship.

Sharing the same cultural background tends to restrict relationships with in-laws to the forms prescribed and to hamper spontaneous interaction. In contrast, where there is no common cultural definition of role, relations are defined by the necessity of adjustment to each other in the situation at hand. Since situations are characteristically fluid, they must take each other's role effectively in order to see the situation from the other's standpoint as well as their own, and this involves a dynamic process of interaction with mutual adjustment as a product of a common effort. Acceptability or desirability depends upon the ability of each to achieve a sympathetic identification with the other.

UNIVERSITY OF HAWAII

LETTERS TO THE EDITOR

TYPOLOGIES OF SOCIETIES

April 3, 1957

To the Editor:

The interesting and valuable article by Freeman and Winch, "Societal Complexity: An Empirical Test of a Typology of Societies" (*American Journal of Sociology*, LXII [1957], 461-66), calls for comment. Clearly, any effort to validate typologies, even though they may already have been validated in certain respects, is to be most heartily commended. Consequently, *nothing* hereinafter said should be construed as disparaging in any way of either the intent or the specific techniques of validation displayed in the article mentioned. Given the assumptions, the achievement has much merit.

The assumptions, however, seem open to closer examination than Freeman and Winch apparently gave to them. One assumption is that the "polar ideal types" lumped together in the first paragraph differ "only in the emphasis which each places on some specific aspect of social patterning. They . . . consequently will be considered . . . as concepts referring to societal complexity." On the basis of this, the validation proceeds, and the outcome is to show that a large number of societies that can be loosely labeled in terms of one or another of the "polar ideal types" mentioned do indeed show degrees of complexity yielding a unidimensional continuum ranging from minimum to maximum.

Had Freeman and Winch examined their assortment of "polar ideal types" with scholarly acumen, it may be that they would have come to apply their major assumption more discriminately. At least, the present writer is inclined to think so where, whatever may be said about folk urban, *Gemein-*

schaft-Gesellschaft, etc., the constructs designated as "sacred societies" and "secular societies" are concerned. It is true that as I myself used these in published form as early as 1930 (reprinted in 1938), following suggestions by Robert E. Park in 1928, the Freeman and Winch assumption seems warranted, and this likewise holds true of the earliest explicit written statement by Park (1925) that I have thus far been able to discover. It should be noted, however, that in 1928 Everett C. Hughes, in his article, "Personality Types and the Division of Labor," used "sacred" and "secular" to designate societies *in their value-system aspects* and that several of these societies or subsocieties referred to as having markedly sacred traits were *quite* complex. Hughes's references to the Chicago Real Estate Board make this obvious.

Then, in 1932, I sketched an array of personality types, functioning in societies ranging from simple to complex, that could be viewed as manifesting these or those sacred or secular traits regardless of the relative complexities of the societies in question as defined wholes. Next, after a rather considerable lapse of time, intensive research on Nazi German phenomena made it apparent that quite complex societies may manifest strongly sacred characteristics (Becker and Myers, "Sacred and Secular Aspects of Human Sociation," *Sociometry*, V [August, 1942], 207-29, esp. pp. 211 and 214-15). Nevertheless, at this time (1942) the vitally important step of subtyping sacred and secular societies had not been taken; it was not until 1948, in fact (Becker and Hill [eds.], *Family, Marriage, and Parenthood* [1st ed.], pp. 19-40, but esp. p. 21, n. 16), that not only Nazi Germany but also Soviet Russia and several

other nations—complex enough in all conscience—were listed as societies having strongly sacred value systems. Thereafter other subtyping was carried out, and various articles incorporating the results were published in 1950 in my collected essays, *Through Values to Social Interpretation*.

In other words, for a rather considerable length of time “sacred” and “secular” have been used as (1) high-level abstractions applicable to societies incorporating the appropriate general value systems and (2) with reference to an array of more concrete subtypes *pinning the abstractions down to substantial amounts of empirical evidence*.

Following this, there was published, in 1956, my article, “A Sacred-Secular Evaluation Continuum of Social Change,” *Transactions of the Third World Congress of Sociology*, V, 19–41. The article may be worth inspecting with regard to “continuum,” for this is *not* couched in terms of societal complexity. Finally, after a preliminary published formulation in 1956, there appeared in Becker and Boskoff (eds.), *Modern Sociological Theory in Continuity and Change* (1957), a chapter entitled “Current Sacred-Secular Theory and Its Development,” presenting two major subtypes of sacred societies, the proverbial and the prescriptive, and two of secular societies, the principal and the pronormless. Of these, the prescriptive variety, definitely sacred, is empirically manifest in Franco Spain, Communist Russia, and several other contemporary societies beyond question markedly complex. In essence, these four subtypes had already been outlined *as long ago as 1948* in the chapter that formed part of the Becker and Hill volume noted above.

To sum up: No useful scientific purpose is served by tossing all so-called “polar types” into a grab bag, as Freeman and Winch have done. In the present writer’s opinion we need, in addition to technical proficiency, a revival of critical scholarship and close conceptual analysis. Obviously, from what has been said above, this would involve attention to the dates when formula-

tions have been significantly altered. Science does not stand still.

HOWARD BECKER

University of Wisconsin

REJOINDER

May 1, 1957

To the Editor:

Professor Becker’s central point seems to be that the “sacred-secular” dichotomy is not the same as “societal complexity.” If our purpose had been to trace the proliferation of nuances with respect to this and related distinctions, he would have a case. Our purpose, however, was to cut through the phrasing of the discursive essayists and to determine whether or not some underlying dimension of societal organization could be demonstrated empirically. Actually we merely made one passing reference to Becker’s dichotomy along with a number of other familiar distinctions. Moreover, even though Becker is scornful of our conception of his dichotomy, he provides justification for our usage in the only reference to his work which we cited (*Social Thought from Lore to Science* [Boston: D. C. Heath & Co., 1938], Vol. I, chap. i). After noting that “all preliterate communities may be called sacred communities” (p. 10), Becker presents the sacred society as “vicinally, socially, and mentally” isolated (p. 39). Moreover:

... the isolated sacred society is economically self-sufficient; there is no foreign trade or any other opportunity for the intrusion of pecuniary valuation and the development of detached attitudes. Inasmuch as there is no trade, the division of labor is simple. . . . The form of the kinship group is that of the large family. . . . Rational science is unknown [pp. 39–40].

Not only is the completely secular society accessible but it “is highly differentiated economically; it has a complex metropolitan economy, with a territorial as well as an occupational division of labor” (p. 41).

If the content of "sacred" and of "complex" are both given by definition, then it is possible logically to discover the distinction between them and also the area of overlap, if any. In our treatment, however, we accepted the idea that "sacred" was given by definition but offered the term "complexity" to denote the empirical dimension of societal organization which we felt that we had discovered. Within this usage, then, we submit that the distinctness between "sac-

red" and "complex" is a matter to be tossed off neither by Becker's reference to his own definition nor by a negative example or two—rather, the question becomes one of determining empirically what degree of association may exist between the two.

LINTON C. FREEMAN

ROBERT F. WINCH

Syracuse University
Northwestern University

HIGHER DEGREES IN SOCIOLOGY, 1956

According to reports received by the *Journal* from 57 departments of sociology in the United States and Canada offering graduate instruction, 138 doctoral degrees and 185 Master's degrees in sociology were conferred in the calendar year 1956.

DOCTOR'S DEGREE

- C. Wiley Alford, A.B., M.A. Texas Christian, 1948, 1949. "The Socially Maladjusted Child in School: A Sociological Study in Special Education." *Duke*.
- Wade Andrews, B.S., M.S. Utah, 1947, 1948. "Some Correlates of Rural Leadership and Social Power among Intercommunity Leaders." *Michigan State*.
- Antonio Arce, B.A. Normal School of Costa Rica; M.A. Michigan State, 1952. "Social Implications of Transition from Traditional to Rational Methods of Operation on a Coffee-growing Estate." *Michigan State*.
- Wendell D. Baker, A.B., M.A. Michigan, 1942, 1951. "A Study of Selected Aspects of Japanese Social Stratification: Class Differences in Levels of Aspiration." *Columbia*.
- Harry V. Ball, B.A., M.A. Washington, 1949, 1950. "A Sociological Study of Rent Control and Rent-Control Violations." *Minnesota*.
- Arthur S. Barron, A.B. Tulane, 1950. "The Effects of Three Styles of Interviewing on the Responses of Women from Two Contrasting Socioeconomic Groups." *Columbia*.
- Ernest A. T. Barth, A.B. Rochester, 1950; M.A. North Carolina, 1953. "A Typological Analysis of Ten Air Force Base-Host Community Situations." *North Carolina*.
- Thelma Williams Battan, A.B., A.M. Michigan, 1951, 1953. "Functional Organization in Metropolitan Areas: An Ecological Analysis of Standard Metropolitan Areas' Employed Labor Force Distributions among Various Industrial Groups, 1950." *Michigan*.
- Thomas L. Blair, B.A. Northwestern, 1950; M.A. Boston, 1951. "Patterns of Information Exposure among Workers in a Rural-Town Community in Southern Brazil." *Michigan State*.
- Zena Smith Blau, A.B., M.A. Wayne, 1943, 1946. "Old Age: A Study of Change in Status." *Columbia*.
- Samuel William Bloom, B.A. Pennsylvania, 1943; M.A. New School, 1950. "A Social-psychological Study of Motion-Picture Audience Behavior: A Case Study of the Negro Image in Mass Communication." *Wisconsin*.
- Leonard Z. Breen, B.S. Illinois Institute of Technology, 1949; M.A. Chicago, 1950. "A Study of the Decentralization of Retail Trade Relative to Population in the Chicago Area, 1929 to 1948." *Chicago*.
- John A. Broussard, B.A. Harvard College, 1948; M.A. Washington (Seattle), 1951. "A Comparative Study of the Distribution of Social Power in One Hundred Preliterate Societies." *Washington (Seattle)*.
- Lee Garwood Burchinal, B.A. Otterbein College, 1951; M.A. Bowling Green, 1952. "The Relation of Parental Acceptance to Adjustment of Children." *Ohio State*.
- Irma Butner, B.A. Iowa State Teachers College, 1927; M.A. Iowa, 1947. "Valuations Expressed by College Students Relative to Education, Work, Family Life, and Leisure: A Study Directed to the Normative Base of American Society." *Iowa*.
- Ernest Campbell, A.B. Furman, 1945; M.A. Pennsylvania, 1946. "The Attitude Effects of Educational Desegregation in a Southern Community: A Pathological Study in Scale Analysis." *Vanderbilt*.
- Robert Blair Campbell, B.S. Indiana State Teachers College, 1944; B.A. Illinois, 1944; M.A. Wisconsin, 1951. "Rural Neighborhood Identification." *Wisconsin*.
- Ray Russell Canning, B.S. Utah State Agricultural College (Logan, Utah), 1942; M.S. Brigham Young, 1948. "Changing Patterns and Problems of Family Life in Provo, Utah, 1905 to 1955." *Utah*.
- Sarah T. Curwood, A.B. Cornell, 1937; M.Ed. Boston, 1947. "Role Expectation as a Factor in the Relationship between Mother and Teacher." *Radcliffe College*.
- Yusuf Suliman Dadabhay, B.A. Stanford, 1950; M.A. Yale, 1953. "Sociological Factors in Postaccident Resistance to Work in Industry." *Yale*.

- Edward Zicca Dager, B.A. Kent State, 1950; M.A. Ohio State, 1951. "Social Factors in Personality Change." *Ohio State*.
- Harry Kirk Dansereau, B.S. Maryland, 1943; M.A. West Virginia, 1948. "Orientation toward Unionism: An Attitudinal Study of a Local Union." *Michigan State*.
- Barbara R. Day, B.S., M.A. Washington State College, 1946, 1947. "The Relationship of Need Patterns to Selection in the Formation of Courtship Couples and Same-Sex Friendships." *Washington (Seattle)*.
- Arturo DeHoyos, B.A., M.A. Brigham Young, 1952, 1954. "Ethnic Differences in Social Aspirations of Urban Youth." *Michigan State*.
- Sidney Denman, A.B. Mississippi College, 1947. "The Conflict of Church and Sect Role in the Southern Baptist Ministry." *Duke*.
- Jeanne B. Diana, A.B. Juniata College, 1946; M.Litt. Pittsburgh, 1950. "The Visiting Teacher: A Study in Status Dilemma and Professionalization." *Pittsburgh*.
- William M. Dobriner, A.B. Hofstra College, 1948. "The Impact of Metropolitan Decentralization on a Village Social Structure." *Columbia*.
- John Thomas Doby, B.A. Union College, 1946; M.S. Wisconsin, 1950. "Some Effects of Bias on Learning." *Wisconsin*.
- William Dorfman, A.B., B.S. Pennsylvania, 1943, 1947. "Some Factors in Intergenerational Occupational Mobility." *Pennsylvania*.
- Lan Donia J. Dright, B.S. Illinois Institute of Technology, 1940; A.M. Chicago, 1944. "Therapeutic Influences of Group Membership on Selected Neurotic Personalities." *Southern California*.
- Edwin Douglas Driver, A.B. Temple, 1945; A.M. Pennsylvania, 1947. "Administrative Control of Deviant Conduct of Physicians in New York and Massachusetts." *Pennsylvania*.
- Rev. Hugh E. Dunn, B.A. West Baden College, 1940; M.A. St. Louis, 1942. "The Self-ideal of Selected Married Catholics." *Catholic*.
- John M. Ellis, A.B. Houston State College, 1943; M.A. Texas, 1949. "Mortality in Houston, Texas, 1949-51: A Study of Socio-economic Differentials." *Texas*.
- Robert Arthur Ellis, B.A., M.A. Yale, 1952, 1953. "Social Stratification in a Jamaican Market Town." *Yale*.
- Richard E. Engler, Jr., A.B., A.M. Southern California, 1949, 1953. "A Systematic Approach to the Study of Morale in an Organization." *Southern California*.
- Donald T. Faigle, B.A. Cincinnati, 1951. "Suicide in Relation to Income, Urbanization, and Race." *Indiana*.
- Sylvia Fleis Fava, B.A. Queens College, 1948; M.A. Northwestern, 1950. "Urban-suburban Contrasts in Social Participation: Rural Orientation as a Factor in Suburban Neighboring." *Northwestern*.
- Arnold Sanford Feldman, A.B., M.A. Wayne, 1949, 1951. "Social Structure and Fertility in Puerto Rico." *Northwestern*.
- Robert Allen Feldmesser, B.Litt. Rutgers, 1948; M.A. Harvard, 1950. "Aspects of Social Mobility in the Soviet Union." *Harvard*.
- Raymond Fink, B.B.A. City College of New York, 1947; M.A. Denver, 1949. "Some Social and Psychological Factors in Stability of Response in Attitude Surveys." *Cornell*.
- Nelson N. Foote, B.S.Agr. Cornell, 1939. "The Professionalization of Labor in Detroit." *Cornell*.
- Gerald A. Fortin, B.A., B.S.Sc., M.S.Sc. Laval, 1949, 1952, 1954. "An Analysis of the Ideology of a French-Canadian Nationalist Magazine, 1917-54: A Contribution to the Sociology of Knowledge." *Cornell*.
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- Donald Elon Johnson, B.S. Illinois, 1952. "A Sociological Analysis of Agricultural Extension." *Illinois*.
- Clifford Tingley Jones, A.B. Gordon College of Theology and Missions, 1948. "The Later Adjustment of Seventy-seven Institutionalized Boys." *New Hampshire*.
- Gene G. Kassebaum, A.B. Missouri, 1952. No thesis. *Harvard*.
- Alvin M. Katz, B.S. Columbia, 1954. "A Case Study of Sociological Research in Progress: The Development and Design of Operational Devices." *North Carolina*.
- Fred E. Katz, A.B. Guilford College, 1952. "A Sociological Study of the Professional Relationships of a Medical Specialty." *North Carolina*.
- Vytautas M. Kavolis, A.B. Wisconsin, 1952. No thesis. *Harvard*.
- Barbara Ann Kay, B.A. Ohio State, 1955. "Differential Perceptions and Delinquency Vulnerability." *Ohio State*.
- James William Kelsaw, A.B. Talladega College, 1951. "Social Anxiety and Adjustment to Future Roles: A Study of 216 Alabama Negro Students on Public School Integration." *Fisk*.
- Louise Killen Kent, A.B. Wayne, 1955. "Maude Louise Fiero: Teacher and Sociologist." *Wayne*.
- Mary M. Kent, B.S. Louisiana State, 1951. "A Study of the Relationship of Primary Group Attitudes to the Occupational Satisfaction of Nurses." *Hawaii*.
- Gretchen Ruth Krause, B.A. Ohio State, 1953. "An Analysis of Kame Crania from the Williams Site." *Ohio State*.
- Harold George Kupke, B.A. Concordia Seminary, 1942. "The Doctrines of the Lutheran Church Relating to Marriage and the Beliefs and Practices of Youth." *Wayne*.
- James Thomas Laing, A.B. Kent State, 1955. "The Drain of Talent Out of the Virginias." *Kent State*.
- Theodore J. Litman, B.A. Minnesota, 1954. "The Incidence of Self-hate among the Jews." *Minnesota*.
- Rev. Florian McCarthy, Ph.B. Mount Carmel College, 1948. "Social Correlates of Participation in Activities of the Carmelite Third Order." *Catholic*.
- Marjorie Anita McCormick, A.B. Illinois, 1954. "A Comparison and Critique of Studies of Urban Peripheral Expansion." *Illinois*.
- Edward L. McDill, B.A. Jacksonville State College, 1952. "Correlates of Authoritarianism: A Study of 546 Respondents, Tuscaloosa, Alabama, 1955." *Alabama*.
- Samuel Dale McLemore, B.S. in Ed. Texas, 1952. "Sociology of the 'Stranger' and the Public School Superintendent." *Texas*.
- Thomas J. Maloney, B.S. (Chem. Engineering) Northeastern, 1948; S.T.B. Harvard, 1952. "A Demographic Study of the Religious Institutions of a Middle-sized Midwest City." *Washington (St. Louis)*.
- Sue A. Marx, A.B. Wayne, 1951. "1952 Election Study: Republican Voters in the U.A.W. Membership." *Wayne*.
- Richard Lawrence Meile, B.S. Illinois State Normal, 1954. "Sociometric Rankings as a Function of Conformity of Behavior to Group Expectations: An Analysis of Factors Affecting This Relationship." *Nebraska*.
- J. Jack Melhorn, A.B. Elizabethtown College, 1944; B.A. Yale, 1947. "A Comparative Study of Adjusted and Unadjusted Youthful Marriages." *Southern California*.
- Nicholas A. Merlo, B.B.A. Canisius College, 1940. "An Analysis of Different Sociological Interpretations of the Changing Roles of Members of the Family." *Southern California*.
- Lester Paul Messerschmidt, A.B., B.D. Concordia Seminary, 1951, 1954. "Dating Patterns and Heterosexual Adjustment: A Study

- of Male Students Trained at Coeducational and Non-coeducational Preparatory Schools of the Lutheran Church—Missouri Synod." *Washington (St. Louis)*.
- Merier Jerry J. Milanez, A.B. Xavier, 1954. "A Sociological Analysis of a Biracial Local Union." *Louisiana State*.
- Kenneth E. Miller, B.A. Alabama, 1953. No thesis. *Alabama*.
- Chivambo Eduardo Mondlane, B.A. Oberlin College, 1953. "Ethnocentrism and the Social Definition of Race as In-group Determinants." *Northwestern*.
- Rev. James F. Muldowney, S.J., A.B. Spring Hill College, 1945. "A Study of Catholic Religious Practices of Puerto Rican Migrants in the United States." *Fordham*.
- Elizabeth W. Nall, B.A. Michigan State, 1947. "The Influence of Crisis in the Modification of Social Organization." *Michigan State*.
- Arthur Gale Neal, B.A. Concord College, 1951. "The Interchangeability of Social Movements." *Ohio State*.
- Charles Francis Nelson, B.A. Yale, 1954. "Social Factors in Academic Success, with Particular Reference to Vocational Orientation." *North Carolina*.
- Roy S. Nicholson, Jr., A.B. Marion College, 1950. "Christian Patterns and Cultural Resistance." *Syracuse*.
- Rev. Hugh O'Rourke, St. Columban's Seminary (Navan, Ireland), 1953. "Jewish-Gentile Inter-marriage: A Problem of Assimilation." *Fordham*.
- Marcus Wayne Orr, A.B. Southwestern at Memphis, 1952. "Problems in the Social Adjustment of Paraplegic Veterans: A Study of 109 Persons." *Illinois*.
- Harold W. Osborne, A.B. Ouachita College, 1952. "A Demographic Analysis of the Institutional Population of the United States, 1950." *Louisiana State*.
- Stanley W. Palmquist, B.A. Iowa, 1955. No thesis. *Iowa*.
- Janice Partridge, A.B. Franklin College of Indiana, 1951. "A Descriptive Analysis of the Social Characteristics of Residents of a Prefabricated Housing Subdivision." *Purdue*.
- Patricia Lou Perry, A.B. Texas, 1955. "A Sociological Analysis of the Zoning Process: A Case Study." *Texas*.
- Barbara Marie Peters, A.B. Drury College, 1953. "The Sociological Concept of Human Nature." *Southern California*.
- Boyd Peyton, S.B. Northern Illinois State Teachers College, 1952. "Identification in Husbands of Japanese War Brides." *Chicago*.
- Raymond Pitt, A.B. Oberlin College, 1952. "A Conceptual Analysis of Aspects of the Theories of Socialization Advanced by Cooley, Mead, Freud, and Sullivan, with an Empirical Study Which Tests Related Hypotheses." *Columbia*.
- Rev. Sabin Plechta, B.A. Theological Institute of Markaska, Yugoslavia, 1944. "The Social Thought of Cardinal Hlond (1881-1948)." *Catholic*.
- Belizars J. Radzins, B.S. Springfield College, 1930. "Anti-American Propaganda in Soviet Monthly Magazines." *Washington (Seattle)*.
- Reed Rockwell, A.B. Illinois, 1950. "Land Values and Population Decentralization." *Chicago*.
- Robert C. S. Rommel, B.A. Psychology, Temple, 1951; B.A. Music, Pennsylvania, 1955. "Technics and Taste." *Pennsylvania*.
- Theodore A. Rottman, B.A. Calvin College, 1954. "Selected Relationships of Parents in Incipient Juvenile Delinquency." *Michigan State*.
- John Carey Rowan, A.B. George Washington, 1950. "Migration and Marital and Household Status." *North Carolina*.
- Mustafa Saeed, Baghdad College of Law, 1951. No thesis. *Florida State*.
- Danilo Salcedo, B.A. Chile, 1948. No thesis. *Michigan State*.
- Francisco Sanchez-López, M.A. Salamanca, 1952. "L'Attitude politique de l'Espagne actuelle." *Laval*.
- Samuel Edward Sanderson, A.B. Clark College, 1951. No thesis. *Pennsylvania*.
- Ruben D. H. Santos-Cuyugan, B.Phil. Philippines, 1949. No thesis. *Harvard*.
- William Scalapino, A.B. California (Santa Barbara), 1947. "Incentives, Social and Economic, as Viewed by Managers and Workers." *Southern California*.
- Norma Scavilla, B.A. College of New Rochelle, 1953. "The Concept of Woman in American Women's Magazines: A Content Analysis Study of the Nature and Role of Woman as Presented in Selected Women's Periodicals." *Catholic*.
- Rev. Charles G. Schoderbeck, A.B. Sacred Heart Mission Seminary, 1947. "Formal Social Participation and Religious Practice of Catholic Families in a Rural Township and Borough of Northwestern Pennsylvania." *Catholic*.

- Albert Schwartz, B.A. Florida, 1951. "A Role Analysis of the Insurance Agent in the Community." *North Carolina*.
- Hannah Scoggin, A.B. Louisville, 1948. "Culture and Conflict in Israel." *Louisville*.
- John C. Scott, A.B. Antioch College, 1954. "A Social Profile of Detroit, 1955." *Michigan*.
- Imogen Seger, B.S. Columbia, 1955. "Durkheim and His Critics on the Sociology of Religion." *Columbia*.
- Robert Melvin Seltzer, B.A. Washington, 1955. No thesis. *Yale*.
- Luis Orlando Sepulveda, B.Letters, Chile, 1945. "Factors Associated with the Selection of Teaching as a Career among Wisconsin High-School Seniors with Emphasis on Rural-urban Differences." *Wisconsin*.
- Robert Sévigny, A.B. Montreal, 1952. "Analyse de contenu de manuels d'histoire du Canada." *Laval*.
- Herbert L. Sharp, B.S. Illinois, 1949. No thesis. *Michigan*.
- Miriam Shypper, A.B. Kansas City, 1952. "The 'Career' of Opiate Addiction." *Chicago*.
- Allan A. Silver, A.B. Michigan, 1954. "A Social Profile of Detroit, 1954." *Michigan*.
- Gabriel Silverman, A.B. Los Angeles State College, 1952. "An Ecological Investigation of San Diego and Adjacent Areas." *Southern California*.
- Francis M. Sim, B.A. Michigan State, 1952. "Elements of Legitimacy of a Voluntary Association: A Case Study of the Flint Suburban Forum." *Michigan State*.
- Paul Sites, A.B. Indiana Central College, 1955. "The Social Class Position of a Selected Group of Ministers, Their Parents, and Their Children." *Purdue*.
- Louie O. Sluder, A.B. Baylor, 1943. "The Changing Role of the Protestant Minister in the United States." *Texas*.
- Mariano Solis, A.B. Ateneo de Cagayan, 1953. "The Relationships of Landlords and Tenants in Rural Philippines." *Catholic*.
- Robert H. Somers, A.B. Antioch College, 1953. "A Study of the Latent Content Model of Latent Structure Analysis." *Columbia*.
- Celerino E. Soto, B.S.E. Philippines Women's University, 1934. "The Social Adjustment of One Hundred Filipinos in Washington, D.C." *Catholic*.
- Denny S. Stavros, A.B. Wayne, 1952. "The Assimilation of Southern White Factory Workers in Detroit." *Wayne*.
- James D. Stinchcomb, B.S. Pittsburgh, 1952. "Selected Urban Detective Bureaus: Their Structure, Purpose, and Problems and the Social Situations Which Influence Them." *Pittsburgh*.
- Norman W. Storer, A.B. Kansas, 1952. "Patterns of Change in the Leadership of a Small Community." *Kansas*.
- Mary M. Stycos, M.A. Edinburgh, 1947. "A Consideration of Changes in Attitudes and Behavior of Consumer Panel of Midwestern Farmers." *Columbia*.
- Anthony Y. Suzuki, B.A. Denver, 1944. "The Social Contribution of the Twenty-six Japanese Martyrs of 1597." *Catholic*.
- M. Lee Taylor, A.B. San Jose State College, 1952. "The Migrant Population of California, Oregon, and Washington, 1950." *Louisiana State*.
- Orhan Tece, Law Degree, Istanbul, Turkey. "Legal Aspects of Socially Maladjusted Behavior in the State of Ohio and in Turkey: A Comparative Study of Juvenile Delinquency in Contemporary Society." *Bowling Green State*.
- Kathleen Antonia Tierman, B.S. Lincoln, 1938. "Social Status and Home Responsibilities of Young Girls." *Washington (St. Louis)*.
- Constance Southart Tonat, B.A. Olivet College, 1945. "A Case Study of a Local Union: Participation, Loyalty, and Attitudes of Local Union Members." *Wayne*.
- Stanley Turteltaub, B.A. Pennsylvania, 1955. No thesis. *Connecticut*.
- Fernando Uribe-Restrepo, J.U.D. Universidad Pontificia Bolivariana, 1952. "Social Aspects of Colombian Legislation." *Catholic*.
- Harwin LeRoy Voss, B.A. North Central College, 1954. "The Ecological Patterning of Juvenile Delinquency in Madison, Wisconsin." *Wisconsin*.
- Elizabeth D. Wangenheim, B.A. Toronto, 1950. "The Social Organization in the Japanese Community in Toronto." *Toronto*.
- Mary Rose Warinner, A.B. Park College, 1954. "A Study of Negro-white Interaction in Two Elementary-School Classrooms." *Washington (St. Louis)*.
- Martha A. Warner, A.B. Kansas State Teachers College (Pittsburg), 1954. "Kansas Populism: A Sociological Analysis." *Kansas*.
- Bruce A. Watson, A.B. San Francisco State College, 1951. "The Social Control Aspects of Art, with Reference to Painting, Sculpture, and Architecture." *Southern California*.
- Samuel Stewart West, B.S., M.A., Ph.D.

- (Physics) California Institute of Technology, 1930, 1932, 1934. "Variation of Compliance to Air-borne Leaflet Messages with Age and with Terminal Level of Education." *Washington (Seattle)*.
- Roland B. Westerlund, A.B. Illinois, 1954. "The Rural-urban Acquaintance Study." *Illinois*.
- Stanton Wheeler, B.A. Pomona College, 1952. "Evaluation of Parole Prediction Techniques." *Washington (Seattle)*.
- Ewell R. Williams, A.B. Abilene Christian College, 1945. "A Study of Letters to the Editor of the *Waco Times-Herald* and *News-Tribune* Following the Tornado of May, 1953." *Texas*.
- Morgan J. Williams, B.S. Utah, 1953. "Differences in Socioeconomic Background and Performance of Students at the University of Utah." *Utah*.
- Ernest Moore Willis, A.B. North Carolina, 1949. "A Comparative Study of Negro Segregation in American Cities." *Northwestern*.
- Howard Martin Wolowitz, A.B. City College of New York, 1954. "A Social Profile of Detroit, 1955." *Michigan*.
- Maurice H. Zakhem, A.B. Wayne, 1954. "The Problem of the Arab Refugees in the Middle East." *Wayne*.
- Charlotte B. Zimmerman, A.B. Radcliffe College, 1952. "Sociological Theory of Frederick Le Play." *Louisiana State*.
- Sheila Gordon Zipf, A.B. Michigan, 1954. "A Social Profile of Detroit, 1954." *Michigan*.

DOCTORAL DISSERTATIONS IN PROGRESS, 1956

The following list of doctoral dissertations in progress in universities and colleges in the United States and Canada is compiled from returns sent by 39 departments of sociology. The number of candidates now working for doctoral degrees is 232. This list includes dissertations in social work, divinity, and other related fields whenever the local department of sociology undertakes to direct them.

- Mary Salim Abdul-Haqq, B.S., M.A. Ohio State, 1952, 1955. "Factors Related to Students' Participation in Social Organizations at the Ohio State University." *Ohio State*.
- John Shields Aird, A.B. Oberlin College, 1946; A.M. Michigan, 1951. "Fertility Levels and Differentials in Two Bengali Villages." *Michigan*.
- Donald E. Allen, B.A., M.A. Ohio State, 1939, 1940. "Analysis of Public Conversation." *Missouri*.
- A. M. Almgren, B.A. Bowling Green State, 1943; B.D., S.T.M. Oberlin College, 1944, 1949. "The Social Creed of the Methodist Church." *Ohio State*.
- Salem M. Ansari, B.A., M.A. Osmania, 1947, 1949; M.A. Kansas, 1955. "A Comparative Study of the Sociology of Conflict and the Koranic Theory of Conflict." *Pennsylvania State*.
- Barbara Burt Arnason, A.B. Swarthmore College, 1944; M.A. Chicago, 1945. "The Care and Cure Functions of the Public Mental Hospital." *Radcliffe College*.
- Herbert Aurbach, B.S. Western Reserve, 1948. "A Study in the Application of the Folk-urban Continuum to the Classification of Kentucky Counties." *Kentucky*.
- Robin Francis Badgley, B.A., M.A. McGill, 1952, 1954. "British Immigrants: Acculturation and Assimilation." *Yale*.
- Walter C. Bailey, A.B., A.M. Indiana, 1942, 1947. "The Differential Communication in the Supervision of Paroled Opiate Addicts." *Southern California*.
- Robert Ketcham Bain, A.B. Chicago, 1947. "The Process of Professionalization: Life Insurance Selling." *Chicago*.
- Russell Barta, A.B. St. Mary of the Lake (Mundelein, Ill.), 1942; M.A. Loyola, 1947. "The Concept of Secularization." *Notre Dame*.
- Bernard H. Baum, Ph.B., M.A. Chicago, 1948, 1953. "Decentralization of Authority in a Bureaucracy." *Chicago*.
- Robert C. Bealer, B.S., M.S. Pennsylvania State University, 1953, 1955. "Mass Communications, Social Structure, and Decision-making: A Study of Farmers' Use of Market News." *Michigan State*.
- John Clinton Beresford, A.B. Antioch College, 1952; A.M. Michigan, 1953. "Nature and Origins of Catholic Fertility Patterns in the U.S.A." *Michigan*.
- Bennett M. Berger, A.B. Hunter College, 1950. "The Image of the Sociologist in the Humanistic disciplines." *California (Berkeley)*.
- Joseph Berger, A.B. Brooklyn College, 1949; A.M. Harvard, 1954. "Co-ordination of Facilities, Performances, and Rewards in Small Social Systems." *Harvard*.
- Morris Ira Berkowitz, B.S., M.A. Purdue, 1952, 1954. "Relationship of Group Size to Internal Structure and Problem-solving Efficiency." *Yale*.
- Elaine Berson, A.B. Illinois, 1950; M. Social Work Oklahoma, 1953. "Sociocultural Factors Involved in Aged Mental Patients." *Duke*.
- Norman Birnbaum, A.B. Williams College, 1947; A.M. Harvard, 1952. "Class and Religion in the German Reformation." *Harvard*.
- Margaret Blough, B.S. Lindenwood College, 1934; M.A. Chicago, 1950. "A Study of the Current Role of Organized Labor as a Pressure Group in the Political System of an Urban Community." *Chicago*.
- Richard Marco Blow, A.B. Harvard, 1948; M.A. Fordham, 1950. "The Problem of the Material and Formal Objects of Sociology." *Catholic*.
- John Titus Blue, Jr., B.A., M.A. Michigan, 1945, 1946. "An Empirical Study of Parent-Child Relations." *American*.
- Charles Bolton, A.B. Denver, 1947; A.M. Stanford, 1948. "The Development Process of Friendship and Love Relations." *Chicago*.
- Frank Bonilla, B.B.A. City College of New

- York, 1949; M.A. New York, 1954. "Students in Politics." *Harvard*.
- David J. Bordua, A.B., A.M. Connecticut, 1950, 1952. "Authoritarianism and Intolerance: A Study of High-School Students." *Harvard*.
- Audrey F. Borenstein, B.A., M.A. Illinois, 1953, 1954. "Social Control and Professional Ethics: A Comparative Analysis of the Physician and the Academician." *Louisiana State*.
- Lee Braude, A.M. Chicago, 1954. "The Rabbi: A Study in the Relation of Contingency Situations to Differential Career Structure." *Chicago*.
- Rozanne M. Brooks, B.A. Pennsylvania State, 1944; M.A. Missouri, 1951. "An Empirical Examination of Some Key Hypotheses Derived from Riesman's *The Lonely Crowd*." *Pennsylvania State*.
- Robert Guy Brown, A.B. Rhode Island State College, 1949; M.A. North Carolina, 1951. "Social Organization of a Psychiatric Inpatient Service." *North Carolina*.
- Mary Sheila Burns, A.B. Marian College, 1945; M.A. Catholic, 1953. "A Comparative Study of the Social Factors in Religious Vocations to Three Distinct Types of Women's Communities." *Catholic*.
- Mervyn L. Cadwallader, B.A., M.A. Nebraska, 1948, 1951. "A Cybernetic Theory of Social Change." *Oregon*.
- Donald Campion, A.B., Ph.L., A.M. St. Louis, 1945, 1946, 1949; S.T.L. Woodstock College, 1953. "A Sociological Analysis of Suicides in Philadelphia." *Pennsylvania*.
- Tilman Cantrell, B.A., M.A. Texas, 1947, 1948. "Role Consensus." *Oregon*.
- Wilmoth A. Carter, A.B. Shaw, 1937; A.M. Atlanta, 1943. "Study of the Negro Main Street of a Contemporary Urban Community." *Chicago*.
- Sally Cassidy, B.A. Manhattanville College, 1944; M.A. Fordham, 1946. "A Study of Lay Leadership." *Chicago*.
- Yung Teh Chow, B.A. Feing Hun, 1937; M.A. Chicago, 1951. "Gentry of China: A Study of Social Mobility in a Rural Community." *Chicago*.
- Edwin A. Christ, A.B. Missouri, 1941; M.A. Michigan State, 1942. "The Stamp Collector: A Study in the Sociology of Leisure." *Missouri*.
- Jay Burton Christensen, B.S. Utah State Agricultural College, 1941; M.S. Brigham Young, 1950. "A Study of Religious Attitudes of High-School and College Students." *Utah*.
- Jeanne Elizabeth Clare, A.B. Michigan, 1947; A.M. Columbia, 1951. "Relationship of Non-familial Activities of Women to Size of Family in the United States." *Michigan*.
- Jerry S. Cloyd, S.B. Harvard, 1948; B.S., M.S. Nebraska, 1951, 1953. "A Critique and Analysis of Present Theories of Small Groups." *Nebraska*.
- Bernard Cohen, A.B. Pittsburgh, 1929; A.M. Southern California, 1949. "Social and Cultural Change in Jewish Life as Reflected in Modern Jewish Literature." *Southern California*.
- Bernard P. Cohen, A.B. Harvard, 1951; M.A. Minnesota, 1952. "A Stochastic Model for Conformity." *Harvard*.
- Elizabeth G. Cohen, A.B. Clark, 1953; M.A. Radcliffe College, 1956. "Parental Factors in Educational Mobility." *Radcliffe College*.
- Jane Collier, B.A. Barnard College, 1953; M.A. Yale, 1956. "Social and Economic Factors Affecting Change of Residence within the Metropolitan Area." *Yale*.
- Herbert Collins, A.B. Brooklyn College, 1942; M.A. Duke, 1946. "The Idea of Race." *Duke*.
- Richard Colvard, B.A. Antioch College, 1952. "The Professional Administrator: A Study of the Arkansas Experiment in Teacher Education." *California (Berkeley)*.
- Mary Grace Connolly, B.S.N.E. Catholic, 1948; M.S. Yale, 1951. "Resources Sought for Psychiatric Assistance by the Mentally Ill and Their Families Prior to the Commitment of the Mentally Ill Person to a Mental Hospital." *Catholic*.
- Richard Farnsworth Curtis, A.B. Oberlin College, 1953; A.M. Michigan, 1954. "Consequences of Vertical Occupational Mobility in Detroit." *Michigan*.
- William V. D'Antonio, B.A. Yale, 1949; M.A. Wisconsin, 1953. "Business and Political Leaders in El Paso, Texas, and Juárez Chihuahua: A Comparative Study of Power Structures, National Images, Attitudes, and Ideology." *Michigan State*.
- Bobbie Jean Dedman, A.B. Berea College, 1953; M.A. Vanderbilt, 1956. "Scale Components of Religious Attitudes." *Vanderbilt*.
- William Thomas Delany, B.A., M.A. California, 1948, 1952. "Bureaucrat and Citizen: A Study of Government Bureaucracies in a Metropolitan Setting." *Michigan*.
- Jack DeLora, B.S. Bowling Green State, 1947;

- M.A. Western Reserve, 1948. "Patterns of Locality Involvement of Urban Small Businessmen." *Michigan State*.
- Alfred M. Denton, Jr., B.S. Oklahoma A. and M., 1949; M.A. North Carolina, 1951. "Some Factors in the Migration of Construction Workers." *North Carolina*.
- Irwin Deutscher, A.B., M.A. Missouri, 1949, 1953. "Husband-Wife Relations in Middle Age: An Analysis of the Sequential Roles among the Urban Middle Classes." *Missouri*.
- Harry C. Dillingham, B.A. Texas, 1948; A.M. Michigan, 1951. "A Study of the Relationship between Religious Affiliation and Social Organization." *Michigan*.
- Margaret Donnelly, B.A. Russell Sage College, 1947; M.A. Fordham, 1949. "Industrial Relocation: A Breach in the Pattern of Community Relationships." *Fordham*.
- Louis Dotson, B.A., M.A. Tennessee, 1949, 1951. "A Test of Guttman's Component Theory." *Vanderbilt*.
- Richard E. Edgar, B.A., M.A. Kansas, 1948, 1950. "A Sociological Study of Negro Male Migrant and Non-migrant Characteristics in St. Louis City." *Washington (St. Louis)*.
- Joanne B. Eicher, B.A., M.A. Michigan State, 1952, 1956. "The Social Effects of Out-migration on a County in the Upper Peninsula." *Michigan State*.
- Joseph W. Elder, A.B., M.A. Oberlin College, 1951, 1954. "Industrialization in Hindu Society: A Case Study of an Indian Factory Town." *Harvard*.
- Ray Elling, Diploma, Sorbonne, 1950; M.A. Chicago, 1955. "Family Culture and Patient Participation in Rheumatic Fever Cardiology Clinic." *Yale*.
- T. Q. Evans, A.B. Manchester College, 1945; B.D. Bethany Biblical Seminary, 1948; M.A. Cincinnati, 1950. "An Occupational Analysis of the Work of the Brethren Pastor." *Ohio State*.
- Donald Ross Fagg, A.B. Ohio Wesleyan, 1949; M.A. Harvard, 1952. "Authority Relations in a Javanese Community." *Harvard*.
- William Faunce, A.B. Michigan State, 1950; M.A. Wayne, 1952. "Automation in the Automobile Industry: Some Social Implications." *Wayne*.
- Merle Edison Fish, Jr., A.M. Southern California, 1941. "Sociological Factors Affecting Downtown Churches of Los Angeles." *Southern California*.
- H. Mark Flappen, B.S., B.A. Washington (St. Louis), 1944; A.M. Chicago, 1949. "A Study To Determine Effective and Reproducible Conditions for Increasing Empathy in the Marital Relationship." *Chicago*.
- C. Melvin Foreman, B.A. Seattle Pacific College, 1942. "Levels of Aspiration and Marital Status on the College Campus." *Washington (Seattle)*.
- Martin J. Forman, B.A. Temple, 1950; M.A. Pennsylvania, 1952. "Factors in Differential Social Adjustment and Utilization of Community Resources by the Aged." *Pennsylvania*.
- Audrey W. Forrest, A.B. Omaha, 1947; M.A. Fisk, 1951. "A Study of Negro Illegitimacy." *Nebraska*.
- Jacob Franz, A.B. Southwestern, 1935; M.A. Columbia, 1938. "Class Differences and Sex Roles." *Ohio State*.
- Linton Freeman, B.A. Roosevelt, 1952; M.A. Hawaii, 1953. "An Empirical Test of Folk Urbanism." *Northwestern*.
- David Garfield French, A.B. Michigan, 1935; M.S. Columbia, 1943. "The Utilization of Social Science in the Social Welfare Field." *Michigan*.
- Robert Fulton, A.B. Illinois, 1951; M.A. Toronto, 1953. "Russell Woods: Succession without Violence." *Wayne*.
- Eugene B. Gallagher, A.B. Lehigh, 1949; M.A. Harvard, 1954. "The Patient's Adaptation to the Mental Hospital." *Harvard*.
- Alex Garber, B.A., M.A. Chicago, 1942, 1946. "The Russian Constituent Assembly, January 18, 1918: A Study of the Ideological Dimensions of Historical Explanation." *California (Berkeley)*.
- Stuart Harold Garfinkle, B.A. Pennsylvania State, 1941; M.A. Pennsylvania, 1947. "Tables of Working Life for Women." *American*.
- Jack Porter Gibbs, B.A., M.A. Texas Christian, 1950, 1952. "A Sociological Study of Suicide." *Oregon*.
- John H. Giele, B.A., M.A. Colorado, 1949, 1953. "Recreation Theory: Model and Reality." *Nebraska*.
- Ira Glick, B.H.L. College of Jewish Studies, 1949; B.A. Roosevelt, 1949. "Futures Trading: A Sociological Analysis." *Chicago*.
- David Goldberg, A.B. Wayne, 1952; A.M. Michigan, 1953. "Differential Fertility." *Michigan*.
- Harold Goldsmith, Ph.B. Chicago, 1949. "The Meaning of Migration: A Study of the Migra-

- tion Expectations of High-School Students in an Area of Heavy Out-migration." *Michigan State*.
- Bernard Goldstein, B.A. Sir George Williams College (Montreal), 1946; M.S.Sc. New School, 1948. "Unions among Professional Engineers: A Case Study." *Chicago*.
- Harold Alton Gould, A.B. Rhode Island, 1951; M.A. Ohio State, 1954. "The Social Organization of a North Indian Village." *Washington (St. Louis)*.
- Helen Beem Gouldner, B.A. College of Puget Sound, 1945; M.Ed. Washington, 1950. "An Operational Delineation of Types of Primary Relations." *California (Los Angeles)*.
- Cesar Grana, A.B. Lima (Peru), 1940; M.A. California (Berkeley), 1947. "The Social World of the French Romantic Novelists, 1830-1850." *California (Berkeley)*.
- David J. Gray, B.A., M.A. Pennsylvania State, 1952, 1955. "The Social Community: Its Value as Reflected in Social Thought from the Eighteenth Century to the Present Time." *Pennsylvania State*.
- Robert Greenfield, A.B. Kent State, 1949; M.A. Ohio State, 1950. "Attitudes toward Public School Desegregation in Florida." *Ohio State*.
- J. Eugene Haas, A.B. Upland College, 1950; M.A. California, 1953. "Consensus in Role Conception and Role Performance in Hospital Work Groups." *Minnesota*.
- Bartlett Hague, B.A. Yale, 1950; M.S. Michigan, 1951. "Changing Function and Structure of Family and Work Relations as Related to adoption of Selected Soil Conservation and Woods Practices in Five Sample Areas." *Michigan*.
- Marian Bessent Hamilton, A.B. Georgia State Women's College, 1946; M.A. Duke, 1948. "Charleston, South Carolina, and the Great Tradition: Some Aspects of Race Relations in a City in the Old South." *Duke*.
- Joseph Gilbert Hardee, B.S. Clemson College (South Carolina), 1948; M.S. Agriculture, Kentucky, 1950. "Social Participation in a Rural Community of Victoria, Australia, and Selected Rural Communities in the United States." *Kentucky*.
- Rose Helper, B.A., M.A. Toronto, 1928, 1945. "Racial Practices of Real Estate Institutions in Selected Areas of Chicago." *Chicago*.
- Richard E. Hilbert, B.A. New Mexico, 1951. "Deferred Gratification Pattern among High-School Students of Reading, Pa." *Pennsylvania State*.
- Daniel Grafton Hill, B.A. Howard, 1948; M.A. Toronto, 1951. "The Canadian Negro Community." *Toronto*.
- Walter Hirsch, B.A. Queens College, 1941; M.A. Northwestern, 1954. "Content Analysis of Contemporary Science Fiction." *Northwestern*.
- Lois Wladis Hoffman, A.B. Buffalo, 1951; M.S. Purdue, 1953. "Some Ideological and Situational Factors in Family Power and Role Structures." *Michigan*.
- Ruth E. Hoffman, A.B., B.S., M.A. Nebraska, 1948, 1950, 1953. "The Social Role of the Intellectual." *Nebraska*.
- Wade Hook, B.A. Newberry College, 1943; B.D. Lutheran Theological Seminary, 1949; M.A. South Carolina, 1950. "A Sociological Analysis of Lay Leadership in the United Evangelical Lutheran Church." *Duke*.
- Ida Hoos, A.B. Radcliffe College, 1941. "Comparative Ideologies of Blue- and White-Collar Workers." *California (Berkeley)*.
- William Harry Howell, A.B. Johnson C. Smith University, 1943; M.A. Atlanta, 1947. "The Rank Order of Sensitivity to Discrimination of Negroes in Orangeburg, S.C." *Ohio State*.
- Frank Howton, A.B., M.A. California, 1947, 1950. "The Changing Self-image of the American Businessman." *California (Berkeley)*.
- George H. Hukanir, B.A. Temple, 1939; M.A. Pennsylvania, 1942. "A Study of Interactive Processes between Community and Factory Structure, 1902-52." *Pennsylvania*.
- John E. Hughes, B.A. Temple, 1948; M.A. Pennsylvania, 1949. "A Study of Occupational Prestige." *Pennsylvania*.
- E. Keith Jewitt, B.S. Black Hills Teachers College, 1949; M.A. Wyoming, 1950. "The Preventive Effects of Arrest, Trial, and Institutionalization in Criminality." *Nebraska*.
- Norman Johnston, B.A. Central Michigan College of Education, 1943; M.A. Chicago, 1951. "The Development of Radial Prisons: A Case Study in Cultural Diffusion." *Pennsylvania*.
- Walter E. Juergensen, A.B. Concordia College 1942; M.A. Omaha, 1947. "Ideology and Institutional Change: The Study of Marriage and the Family during the Lutheran Reformation in Germany." *Nebraska*.
- Yoshiko Kasahara, A.M. Texas, 1952; M.S.

- Michigan, 1956. "The Patterns of Migration in Japan." *Michigan*.
- Walter C. Kaufman, A.B., A.M. Rochester, 1950, 1951. "A Factor Analytic Test of Some Revisions in the Shevsky-Bell Typology." *Northwestern*.
- Christine Kayser, A.B. Stanford, 1948; M.A. Radcliffe College, 1951. "The German Student and Political Responsibility." *Radcliffe College*.
- Frank E. Kent, B.A. Central YMCA College, 1937; M.A. Northwestern, 1940. "Family Structure and Labor-Force Participation in Japan, 1930-54." *American*.
- Jerome Kestenbaum, A.B. Yeshiva, 1939; M.A. Illinois, 1943. "A Sociological Study of a Jewish Community of Havana, Cuba." *Florida*.
- Thomas Hoskins Kettig, B.S., M.A. Louisville, 1943, 1946. "Attitudes of Ohio School Teachers toward Racial Integration." *Ohio State*.
- Lucien Blackstone Keys, B.A., M.A. Cincinnati, 1939, 1943. "Development of Scales and Techniques for Analysis of Neighborhoods and Neighboring." *Ohio State*.
- John I. Kitsuse, B.S. Boston, 1946; M.A. California (Los Angeles), 1953. "The Perception of Status Exceptions of Adolescents." *California (Los Angeles)*.
- Frederick Walter Koenig, B.A. Washington, 1951; M.S. Wisconsin, 1954. "The Effect of Role Ambiguity on Group Processes." *Wisconsin*.
- Shigemi Kono, A.B. Yamaguchi National, 1953; M.A. Brown, 1955. "The Japanese Labor Force: A Demographic Analysis." *Brown*.
- Clarence Kraft, B.S., M.S. Illinois, 1937, 1940; M.S. Purdue, 1952. "Mate Adjustment in Protestant-Catholic Marriages." *Purdue*.
- Elizabeth Kramm, A.B., M.P.H. California, 1941, 1948. "The Mongolian Child and His Family." *Pittsburgh*.
- Louise Krause, A.B. South Dakota, 1948; A.M. Chicago, 1950. "A Study of Older Employed Women Living in Chicago." *Chicago*.
- Richard A. Kurtz, A.B. Teachers College of Connecticut, 1951; M.A. Connecticut, 1953. "Marginality in a Fringe Area." *Michigan State*.
- Kian M. Kwan, B.A. Far Eastern University, 1952. "Assimilation of the Chinese in the United States." *California (Berkeley)*.
- Michael Lalli, B.A. Pennsylvania State, 1937; M.A. Pennsylvania, 1949. "A Case Study of Some Sociological Aspects of Relocation." *Pennsylvania*.
- Richard Emmett LeBlond, Jr., B.A., M.A. Cincinnati, 1950, 1950. "Italian Military Elites." *Michigan*.
- Anne S. Lee, B.A., Pennsylvania, 1950. "Differential Fertility in the United States." *Pennsylvania*.
- Hope J. Leichter, A.B. Oberlin College, 1950. "Alternatives and Constraints in the Role of the Chinese Exile." *Radcliffe College*.
- Richard C. Leonard, B.A. College of St. Thomas, 1949; M.A. Catholic, 1950. "Community Values in Educational and Occupational Selection: A Study of Youth in Emmitsburg, Maryland." *Catholic*.
- Judith K. Leventman, A.B. Barnard College, 1953; M.A. Minnesota, 1954. "The Third-Generation Jew." *Minnesota*.
- Seymour Leventman, A.B. Washington State College, 1951; M.A. Indiana, 1953. "Status Situations and Status Aspirations of Minneapolis Jews." *Minnesota*.
- David T. Lewis, B.S. Central Michigan College, 1942; M.A. Ohio State, 1947. "Some Social Correlates of Adjustment to Minority Status." *Ohio State*.
- William Long, A.B. Miles College, 1939; M.A. Atlanta, 1942. "The Relative Status of the Residents of Successive Zones of Settlement in the Negro Community of Chicago." *Chicago*.
- Juárez Lopes, B.A. Escola Sociologia Política (São Paulo, Brazil), 1950; A.M. Chicago, 1953. "Industry and the Local Community." *Chicago*.
- Albert E. Lovejoy, B.A., M.A. North Carolina, 1947, 1949. "The Structure of American Churches: A Study in the Typology of Religious Bodies." *North Carolina*.
- James L. Lowe, B.S., M.A. Missouri, 1939, 1949. "Vocational and Educational Aspirations of High-School Seniors." *Missouri*.
- Amos Lytton, A.B. Akron, 1946; M.A. Chicago, 1949. "Retirement in the Rubber Industry." *Chicago*.
- Rulon McCarrey, B.S., M.S. Utah State Agricultural College, 1937, 1941. "A Study of College Students' Attitudes toward Courtship, Marriage, and the Family." *Utah*.
- Louis J. Maloof, A.B., M.A. Georgia, 1949, 1950. "Arabic-speaking Peoples in Mexico." *Florida*.
- Harry W. Martin, B.A., M.A. Georgia, 1949, 1950. "Participation and Leadership of

- Physicians in Community Life." *North Carolina*.
- Rev. John V. Martin, Belgium. "A Comparison of the Theoretical Systems of Talcott Parsons and Georges Gurvitch." *Harvard*.
- Y. Scott Matsumoto, B.S., M.A. American, 1954, 1955. "Conditions Associated with Transitional Attitudes in Japanese Society." *American*.
- E. Luke Matz, B.S. Ithaca College, 1938; M.S. Pennsylvania, 1948. "The Impact of a Large Industrial Plant upon a Small Agrarian Community." *Ohio State*.
- Robert C. Mendenhall, B.S., M.S. Brigham Young, 1949, 1951. "Differential Strains and Values in Two Mormon Ways of Life." *Harvard*.
- Sheldon R. Messinger, Ph.B. Chicago, 1947; B.A. California (Los Angeles), 1951. "Administration and Decision-making in the California Prison System." *California (Los Angeles)*.
- John T. Mitchell, B.S., M.A. Missouri, 1949, 1950. "Prison Social Structure." *Missouri*.
- Kalliopie Marinou Mohring, Diploma, Superior School of Agriculture, Athens, Greece, 1949. "Change in Family Organization of Greek Immigrants." *Michigan*.
- Chivambo Eduardo Mondlane, B.A. Oberlin College, 1953; M.A. Northwestern, 1956. "Role Conflict, Reference Group, and Race." *Northwestern*.
- William A. Morrison, A.B. Harvard, 1950; M.A. Louisiana State, 1952. "Family Planning in Rural India." *Connecticut*.
- Robert Mugge, A.B., A.M. Florida, 1943, 1944. "Negro Migrants in Atlanta." *Chicago*.
- James F. Muldowney, A.B. Springhill College, 1945; M.A. Fordham, 1955. "Selected Aspects of Social Change in Desegregation." *North Carolina*.
- Myron Krikor Nalbandian, A.B., M.A. Brown, 1952, 1955. "Methods of Population Estimation with Particular Application to Rhode Island." *Brown*.
- Frank C. Nall, B.A., M.A. Michigan State, 1950, 1955. "A Study of Cross-cultural Education on the United States-Mexican Border." *Michigan State*.
- Fredrica K. Neville, B.S. Illinois, 1947; M.S. Louisiana State, 1952. "The Social Situation and Clothing Awareness." *Michigan State*.
- Peter King-Ming New, A.B. Dartmouth College, 1948; M.A. Missouri, 1953. "Perception of Osteopathy by Osteopathic Students." *Missouri*.
- Setsuko Nishi, A.B., M.A. Washington (St. Louis), 1944, 1944. "Nisei Culture in Chicago: Its Manifestations in Social Organization." *Chicago*.
- Dave Okada, A.B. Oberlin College, 1944. "Study of the Ethnic Intolerance of an Ethnic Minority Group: An Assessment of Certain Hypotheses Regarding Ethnic Intolerance." *Chicago*.
- Marcus Wayne Orr, A.B. Southwestern at Memphis, 1952; M.A. Illinois, 1956. "The Renaissance and the Rise of Modern Social Thought." *Illinois*.
- Norman Painter, B.A. Baylor, 1947; M.A. Tulane, 1949. "Delineation and Demographic Comparison of Locality Grouping in a Latin-American Community." *Michigan State*.
- Alexander A. Penelli, A.B., M.A. Dartmouth College, 1946, 1948. "Patterns of Communication in a Small Community." *Michigan*.
- William Phillips, B.A., M.A. Fisk, 1948, 1950. "Some Factors in the Changing Position of the Negro in the Labor Force, 1940-50." *Chicago*.
- Jesse R. Pitts, A.B., M.A. Harvard, 1941, 1950. "The French Family." *Harvard*.
- Belizars J. Radzins, B.S. Springfield College, 1930; M.A. Columbia, 1931. "Testing the Valuanee Theory of Human Behavior in the Washington Public Opinion 1956 Voters Survey." *Washington (Seattle)*.
- Hilda Raj, B.A., M.A. Madras, 1920, 1923; B.A., M.A. Cambridge, 1933, 1939. "Caste, Social Structure, and Changing Religious Beliefs: An Analytical Study of the Nadar Caste of South India." *American*.
- James Andrews Randall, A.B., A.M. Michigan, 1947, 1949. "Comparative Study of Social Participation among Detroit Negroes with Particular Reference to Migratory Status." *Michigan*.
- Robert Rankin, A.B. San Jose State College, 1935; B.D. Union Theological Seminary, 1938. "The Religious Institution and Its Functionaries: A Study of the Methodist Church in California." *California (Berkeley)*.
- Horace D. Rawls, B.S., M.S. North Carolina State, 1943, 1946. "Guidance and Education for the Aged in North Carolina." *Duke*.
- Earle Reeves, Jr., B.A., B.D. Eastern Baptist College, 1949, 1952; M.A. Pennsylvania, 1954. "The Autobiography: A Study of Its

- Values and Limitations in Behavioral Research." *Pennsylvania*.
- Arthur Richardson, B.S., M.S. Purdue, 1952, 1953. "Prefabricated Housing Subdivisions as a Special Case of the General Process of Suburbanization." *Purdue*.
- Marvin P. Riley, B.S. Northwestern, 1942; M.A. Missouri, 1950. "The Hutterites of South Dakota: A Study of Intergroup Relations." *Missouri*.
- Walt Risler, B.A. Upsala College, 1948; M.A. Chicago, 1950. "Personal Documentation and Projective Analysis in Family Research: A Comparative Study of Techniques." *Chicago*.
- Julio C. Rivera, B.A. Popayan Seminary (Colombia), 1942; M.A. Detroit, 1954. "Acculturation of Mexican Immigrants." *Michigan State*.
- Dorris W. Rivers, B.S. Mississippi State, 1937; M.Ed. Duke, 1947. "The Nature and Function of Extension Education: A Sociological Study." *Duke*.
- Mary Ellen Roach, A.B. Kansas, 1942; M.S. Iowa State College, 1946. "The Influence of Status and Other Selected Variables on Clothing Habits and Orientation at Pre-adolescence." *Michigan State*.
- Charles Robbins, B.S., M.S. Iowa State College, 1949, 1951. "The Status and Role of the Citizen-Soldier." *Chicago*.
- Daniel Rosenblatt, A.B. Wayne, 1946; M.A. Harvard, 1954. "The Development of Organizations by Patients in a Mental Hospital." *Harvard*.
- Howard E. Rosenborough, A.B., M.A. Toronto, 1942, 1946. "The Sociology of Consumer Spending." *Harvard*.
- Charles J. Rumage, A.B. Marietta College, 1948; M.A. Ohio State, 1954. "Juvenile Delinquency Proneness: An Analysis of the Kvaraceus Scale." *Ohio State*.
- Louis A. Ryan, A.B. College of St. Thomas, 1937; M.A. Catholic, 1941. "Value Judgments in Selected Introductory Sociology Textbooks." *Ohio State*.
- Maria Cristina Salazar, Universidad Pontificia Javeriana, Bogotá, 1952; M.A. Catholic, 1954. "A Religious Census Study in Menizales, Colombia." *Catholic*.
- Joseph Sardo, B.A., M.A. Florida, 1950, 1953. "The Study of Rural Social Organization in Sicily." *Florida*.
- James Sartain, B.S. Troy State Teachers College, 1947; M.A. George Peabody College for Teachers, 1948. "A Comparison of Parents' with Children's Attitudes in a Community Undergoing Racial Segregation." *Vanderbilt*.
- Delbert Schrag, A.B. Bethel College, 1948; M.A. Chicago, 1952; B.D. Bethany Seminary, 1953. "The Sectarian and Liberal Protestant Minister." *Chicago*.
- Woodrow Wilson Scott, B.S. Utah State Agricultural College, 1940; M.M. Wisconsin, 1953. "Interpersonal Relations as Affected by Ethnic Characteristics of Working Groups." *Southern California*.
- Herbert Seeley, A.B. Chicago, 1942. "The Ecological Distribution of Persons in Selected Psychiatric Categories." *Chicago*.
- Anima Sengupta, B.A. Calcutta, 1936; M.A. Columbia, 1953. "The Role of Women in Indian Public Life in Modern Times." *American*.
- S. Frederick Seymour, A.B. Union College, 1948. "The Labor Force in Kansas City." *Chicago*.
- David M. Shaw, A.B. Redlands, 1953; M.A. Minnesota, 1955. "Experimental Study of Division of Labor in Small Groups." *Minnesota*.
- Ray E. Short, B.A. Willamette, 1944; B.D. Duke, 1947. "Social Control as Applied to International Relations, with Especial Reference to Religious Sanctions." *Duke*.
- Fred B. Silberstein, B.A., M.A. Ohio State, 1949, 1950. "Occupational Mobility and Prejudice." *Ohio State*.
- Leslie Joel Silverman, B.A. Roosevelt, 1950; M.A. New School, 1956. "Economic Insecurity Level and Reference Groups." *Wisconsin*.
- Jonathan Avery Slesinger, A.B. Vassar College, 1949; M.A. American, 1954. "Career Adaptations in a Junior-Management Assistant Program." *Michigan*.
- Jeanette Louise Smalley, B.S.Ed., A.M. Southern Illinois, 1952, 1953. "The Emergent Social Structure of an Interracial Housing Project." *Washington (St. Louis)*.
- Bulkeley Smith, Jr., B.A., M.A. Yale, 1946, 1954. "Factors Related to the Residential Desegregation of Negroes." *Yale*.
- Ted Clifford Smith, B.A., M.A. Utah, 1948, 1949. "Community Structure and Adjustive Processes: Bountiful, Utah." *Utah*.
- Lorenzo Snow, A.B. Brigham Young, 1950; M.A. Northwestern, 1952. "Personality Adjustment of Miami County Youths: A Study of Adjustive Behavior at Two Different Age Levels." *Ohio State*.

- Clinton Snyder, B.S., M.A. Michigan State, 1943, 1954. "Variations in Role and Status Expectations as Related to Personal Interaction." *Michigan State*.
- Richard E. Sommerfeld, B.A., B.D. Concordia Seminary, 1951, 1954; M.A.Ed. Washington (St. Louis), 1953. "The Role Conceptions of Lutheran Ministers in the St. Louis Area." *Washington (St. Louis)*.
- George M. Stabler, B.S. Earlham College, 1950; M.S. Wisconsin, 1953. "*Bejucal*: Social Values and Changes in Agricultural Practices in a Cuban Rurban Community." *Michigan State*.
- Austin J. Staley, A.B., M.A. St. Vincent College, 1936, 1938. "Racial Inter-marriage in Brazil." *Pittsburgh*.
- Howard Stanton, B.A. Chicago, 1947. "Parental Competence and Child Development: An Experiment in Clinical Research." *Chicago*.
- Richard Stern, B.A. Yale, 1951; M.A. Columbia, 1952. "Labor Turnover in a Banking Institution." *Pennsylvania*.
- Gregory Stone, B.A. Hobart College, 1942; M.A. Chicago, 1952. "Clothing and Social Structure: A Study of Symbols in the Context of Community Life." *Chicago*.
- Morris H. Sunshine, B.A., M.A. Missouri, 1949, 1954. "Anomie and Social Mobility." *Northwestern*.
- Samuel J. Surace, A.B., M.A. California (Los Angeles), 1951, 1953. "Occupational Career Patterns and Cultural Differentiation: The Case of the Professional Dance Musician." *California (Berkeley)*.
- Gordon Francis Sutton, A.B., A.M. Wayne, 1952, 1955. "The Organization of a Metropolitan Community as Related to the Nature of Its Potential Growth." *Michigan*.
- Sherman Leonard Syme, B.A., M.A. California (Los Angeles), 1953, 1955. "Social Expectation and Interpersonal Relationships with Reference to an Infertility Clinic." *Yale*.
- Yuzuru Takeshita, A.B. Park College, 1951; A.M. Michigan, 1952. "Socioeconomic Correlates of Urban Fertility in Japan." *Michigan*.
- Richard Robb Taylor, Jr., B.A. Swarthmore College, 1952; M.A. North Carolina, 1955. "The American University as a Behavioral System." *North Carolina*.
- Bernard Alan Thorsell, B.S., M.A. Illinois, 1949, 1955. "Dominant Value Profiles of Some American Subcultures." *Illinois*.
- Charles H. Tilly, A.B. Harvard, 1950. "The Vendean Counter-Revolution." *Harvard*.
- Charles M. Tolbert, B.A. Mississippi College, 1948; M.A. North Carolina, 1954. "A Sociological Study of the Choctaw Indians in Mississippi." *Louisiana State*.
- Stanley Bryce Towner, A.B. California, 1944; M.A. Southern California, 1955. "A Comparison of Aspired Roles of Student Nurses with Achieved Roles of Registered Nurses in Selected Los Angeles Hospitals." *Southern California*.
- Benjamin B. Tregoe, A.B. Whittier College, 1951. "Assimilation and Child Rearing." *Harvard*.
- Paul S. Ullman, B.A., M.A. Southern California, 1951, 1953. "Attitudes Toward and Sex Behavior of Two Groups of Incarcerated Males." *Oregon*.
- Robert C. Vanderham, B.A.; M.A. DePauw, 1947, 1949. "A Study of the Ingham County Hospital as a Social System." *Michigan State*.
- Rev. N. J. J. Van Greunsven, B.A. St. Meinrad's Seminary, 1947; M.A. Catholic, 1955. "A Study in Social Planning: The International Catholic Institute for Social Research." *Catholic*.
- Arthur Vener, A.B. Queens College, 1950; M.A. Michigan State, 1953. "Social-psychological Aspects of Adolescent Clothing Attitudes." *Michigan State*.
- Paule Verdet, Licence de Lettres, École Normal Supérieure des Jeunes Filles, Diplôme d'études supérieures de philosophie, Sorbonne, 1946. "A Study of Preaching in Relation to Discrepancies between Lay and Professional Definitions of the Institution and of the Membership in the Roman Catholic Church." *Chicago*.
- William Wallace Vosburgh III, B.A. Yale, 1950; M.A. California (Los Angeles), 1952. "Relationship between Social Class and Use of Leisure Time." *Yale*.
- Andrew Lester Wade, B.A. Linfield College, 1948; M.A. Oregon, 1950. "Vandalism among Teen-Age Boys: An Example of Adolescent Role Frustration." *Wisconsin*.
- Leonard Wesley Wager, A.B., A.M. Washington (Seattle), 1949, 1952. "Career Patterns: A Study of Airline Pilots in a Major Airline Company." *Chicago*.
- William Ward, B.A. Muhlenberg College, 1941; M.A. Syracuse, 1943; B.D. Lutheran Theological Seminary, 1945. "An Analysis of Relationships between Change of Residence and

- Church Membership in an Area of West Bethlehem, Pa." *Pennsylvania*.
- Leon H. Warshay, A.B. Brooklyn College, 1949; M.A. Chicago, 1951. "The Limitation of Culture Contact as Explanation of Variation in Broadness of Perspective." *Minnesota*.
- Murray Wax, B.S. Chicago, 1942; M.A. Pennsylvania, 1947. "The Temporal Images of Radical Social Movements." *Chicago*.
- Lorell E. Weiss, B.D. Bethany Biblical Seminary, 1934; A.M. Claremont, 1944. "Social Psychology Factors in the Pacifism of the Church of the Brethren in World War I." *Southern California*.
- Benedict Joseph Wengler, B.A. St. Francis College, 1949; M.A. Columbia, 1951. "Sociology of the Apprentice." *Fordham*.
- Charles D. Whatley, Jr., B.A., M.A. Texas, 1949, 1952. "Reference Groups and Recovery from Mental Disorder." *Tulane*.
- Wayne L. Wheeler, A.B. Doane College, 1944; M.A. Nebraska, 1948. "An Analysis of a Liberal Arts College and Its Community Setting: A Study in Social Change." *Missouri*.
- Ernest Moore Willis, A.B. North Carolina, 1949; M.A. Northwestern, 1956. "Urban Neighborhood Attitudes and Reference Group Behavior." *Northwestern*.
- David L. Wolfe, B.S., M.Ed. Illinois, 1950, 1952. "Dilemmas in Human Relations: The Commitments of Academic Professionals." *Oregon*.
- Maurice Wolfe, A.B., M.A. California (Berkeley), 1948, 1952. "The Algerian Intellectual: A Case of the Marginal Man." *California (Berkeley)*.
- George K. Yamamoto, A.B., A.M. Hawaii, 1947, 1949. "The Career of the Japanese-American Lawyer in Honolulu." *Chicago*.
- Simon Yasin, B.A. McGill, 1951; M.A. Michigan State, 1954. "The Utility of Social-psychological Typologies for the Analysis of Decision-making Activities." *Michigan State*.
- James Neale Young, B.S. Clemson College (South Carolina), 1948; M.S. Agriculture Kentucky, 1950. "The Influence of Neighborhood Norms on the Diffusion of Recommended Farm Practices." *Kentucky*.

NEWS AND NOTES

Central Council for Health Education (London).—A summer school in health education will be held August 20–30 on the subject of the promotion of health and the techniques of health education. The fee will be £21 for tuition and full residence.

This is a working conference for doctors, nurses, health inspectors, teachers, and auxiliary health and social welfare workers from various fields and different countries. It will devote attention to new content of health education, community attitudes and motivation, effective methods of group and community education, and the evaluation of results. In addition, the program will offer an intensive practical course in the production and use of teaching materials, with demonstrations of how these can be adapted to local conditions.

For information write to: The Central Council for Health Education, Tavistock House North, Tavistock Square, London, W.C. 1, England.

University of Chicago.—In the Summer Quarter Professors Coleman, Hughes, Shils, and Warner will be in residence.

Ernest Burgess, emeritus professor of sociology, and Robert Havighurst, the past and the current president, respectively, of the American Gerontological Society, William E. Henry, and Bernice Neugarten will attend the International Congress of Gerontology at Merano, Italy, in July.

William F. Ogburn, professor emeritus of sociology, who has been lecturing in New Delhi since last fall, will spend the summer traveling in Indonesia, Australia, New Zealand, and Japan. He will return in the fall to his home in Washington, D.C.

Elihu Katz will spend the second year of his two-year leave of absence at the Eliezer Kaplan School of Economics and Social Sciences, Hebrew University, Jerusalem.

Everett C. Hughes will be on leave of absence for the academic year 1957–58. He will spend the first half at Harvard as visiting professor of education and will carry on research for Radcliffe College. In the second semester he will

conduct a seminar on the professions in postwar Europe at the University of Frankfurt. He will be accompanied by his wife, Helen MacGill Hughes, who will be on leave as managing editor of the *American Journal of Sociology*. They will return to the campus in September, 1958.

Philip M. Hauser and Donald Bogue will attend the meetings of the International Statistical Institute and the International Union for the Scientific Study of Population in Stockholm in August.

Fred Strodbeck, now at the University of Michigan, will join the Department of Sociology in the fall as associate professor of sociology and psychology. He will offer courses and direct research in social psychology.

James A. Davis, now of Yale University, joins the department in the fall as assistant professor of sociology. He will also be associated with the National Opinion Research Center.

Edward Shils, of the Committee on Social Thought, has been appointed professor in the Department of Sociology as well.

In May a work conference was held in Kansas City, jointly of Community Studies, Inc., and members of the Chicago faculty who are connected with research there. Research studies were presented by William E. Henry, Bernice Neugarten, Elaine Cumming, and Dan Lortie on personality and aging; Everett C. Hughes and Anselm Strauss on medical education; Julius Roth on decisions in the treatment of tuberculosis patients; Blanche Geer and Howard S. Becker on medical students and interns; Thomas McPartland on the general practitioner in medicine; Irwin Deutscher and Peter New on the nursing profession; Warren Peterson on leisure and education; Mary New on the rehabilitation of the handicapped; and Richard Wohl on the social history of Kansas City.

Herbert Stember, formerly of the National Opinion Research Center and Pennsylvania State University, has been appointed associate professor of social psychology in the School of Business.

A new project has begun on community living at the National Opinion Research Center, conducted by Peter H. Rossi and James A.

Davis. The project is to cover the community relations of business managers and their participation in various types of local enterprise.

A study by the National Opinion Research Center of factors affecting preventive dental practice is being sponsored by the American College of Dentists and the Zoller Dental Clinics of the University of Chicago and financed by the Institute of Dental Research of the National Institutes of Health.

Duke University.—John C. McKinney, Michigan State College, will become chairman of the Department of Sociology and Anthropology in September, 1957.

Howard E. Jensen, who will retire from the chairmanship in September, will continue as professor of sociology until September 1, 1958, when he will become emeritus.

Foundations' Fund for Research in Psychiatry.—The Foundations' Fund wishes to announce that October 15, 1957, is the next deadline for the submission of applications for research fellowships and research teaching grants in psychiatry, psychology, sociology, neurophysiology, and other sciences relevant to mental health.

Interested persons and departments are invited to write for details to: Foundations' Fund for Research in Psychiatry, 251 Edwards Street, New Haven 11, Connecticut.

Second International Congress of Group Psychotherapy.—The Congress will be held in Zurich, Switzerland, August 28–31, 1957. The papers and discussions are planned to reflect the many applications of group psychotherapy in a variety of settings, in different countries, and with different types of clinical problems, as well as its broader community and interdisciplinary aspects: clinical psychiatry, social psychiatry, psychoanalysis, psychodrama, research, industry, education, social prophylaxis, and training. Special emphasis will be laid on the use of group psychotherapy with children, adolescents, non-psychotic and psychotic adults, etc., and on special problems in such settings as inpatient hospitals and outpatient clinics.

The Congress is under the auspices of the International Committee of Group Psychotherapy, with representatives of seventeen countries. Its officers are: presidents, J. L. Moreno, S. R. Slavson, W. Hulse, and W. J. Warner, United States; J. Bierer, United King-

dom; and S. Lebovici, France; vice-presidents, N. Beckenstein and R. J. Corsini, United States; H. Ezriel, S. H. Foulkes, and T. P. Rees, United Kingdom; Georges Heuyer, France; Hans Hoff, Austria; L. J. Hut, Netherlands; E. E. Krapf, Argentina; K. R. Masani, India; Frisso Potts, Cuba; E. J. Rosen, Canada; C. A. Seguin, Peru; A. Sunier, Netherlands; and Nic Waal, Norway.

For further information address the Second International Congress on Group Psychotherapy, 812 Stuart Avenue, Mamaroneck, New York.

University of Hawaii.—Otomar J. Bartos, a Ph.D. candidate at Yale University, has been appointed instructor in sociology.

Clarence E. Glick has returned after two years' leave. The first year was spent studying race relations in the Federation of the Rhodesias and Nyasaland under a grant from the Ford Foundation. During 1955–56 both Dr. and Mrs. Glick were Fulbright fellows in India.

Andrew W. Lind, who was a Fulbright Research Scholar in the West Indies in 1955, has published two books since his return: *Hawaii's People and Race Relations in World Perspective*. The latter, which Dr. Lind edited, is a collection of papers prepared by persons attending the Conference on Race Relations in World Perspective, which was held at the University of Hawaii in the summer of 1954.

Bernhard L. Hormann, who had an attack of polio in November, 1955, resumed his work in the department last September. He recently completed editorial work and published *Community Forces in Hawaii*, which brings together outstanding articles from the first fourteen issues of *Social Process in Hawaii*.

Hebrew University.—About sixty people gathered in Jerusalem on March 13 and 14, 1957, to participate in a conference on social research, the first of its kind in Israel. The meeting, devoted to lectures and discussion, took place at the new building of the Eliezer Kaplan School of Economics and Social Sciences and was sponsored by the Department of Sociology of the University with the co-operation of the Israel Institute of Applied Social Research and the Henrietta Szold Foundation for Child and Youth Welfare. The nineteen research reports presented dealt with problems of Israeli youth and the continuity of the second generation; economic behavior and occupational

choice; immigrant groups and cultural differences; and other aspects of Israeli life. A concluding session was devoted to the role of applied social research.

Speakers included A. Bonné, dean of the Eliezer Kaplan School of Economics and Social Sciences, and the directors of the three sponsoring organizations—S. N. Eisenstadt, Louis Guttman, and Moshe Smilansky—other members of these organizations and the academic staff of the Hebrew University, research workers of the Ministry of Health and the Settlement Department of the Jewish Agency, and individuals engaged in social study in the country. Organizer of the meeting on behalf of the Department of Sociology was Elihu Katz, assistant professor at the University of Chicago and visiting lecturer in sociology at Hebrew University.

Indiana University.—In the fall a new journal entitled *Victorian Studies* is to be published and edited at Indiana University by Philip Appleman, William Madden, and Michael Wolff. It will be a quarterly devoted to the examination of Victorian culture. Designed to work toward a deliberate co-ordination of the various academic disciplines, it will include articles in the humanities, arts, and sciences, as well as book reviews, the annual "Victorian Bibliography" (sponsored by the Victorian group of the Modern Languages Association), a forum for the discussion of controversial issues, and notes and queries in addition to articles. Subscriptions will be \$5.00 a year; in England 35s.

All communications should be addressed to: The Editors, *Victorian Studies*, Indiana University, Bloomington, Indiana.

University of Kansas.—John and Jean Gullahorn are conducting two studies of cross-cultural education. The first, sponsored by the United States Department of State, is of the American Fulbright and Smith-Mundt grantees who returned to the United States and are now residing in nine midwestern states. The second is of the problems of adjustment of foreign students at the University of Kansas.

William Delaney has resigned to accept a research position with the United States Information Service in Norway.

Henrietta Cox has joined the staff as a part-time instructor,

Raymond P. Cuzzort, of the University of Chicago's Population Research and Training Center, will join the staff in September as assistant professor.

Carroll D. Clark is giving a radio program on the social significance of jazz music for Station KANU.

Michigan State University.—Charles P. Loomis is resigning from the headship, effective upon the appointment of a successor, and is remaining as a staff member in the Department of Sociology and Anthropology. After thirteen years of administrative work, Professor Loomis will devote himself to research and teaching.

Midwest Sociological Society.—The annual meetings of the Midwest Sociological Society were held April 4–6, 1957, in Des Moines, Iowa. The Department of Sociology of Drake University, under the chairmanship of Lester S. Pearl, served as the host. Two hundred and thirty-one members and guests registered for the meetings, which were presided over by the president, C. T. Pihlblad. The guest speaker at the annual dinner was Joseph D. Lohman, sheriff of Cook County (Chicago), Illinois, who spoke on "The Role of the Sociologist in Practical Affairs." Thomas D. Eliot, professor emeritus of Northwestern University and past president of the Society, was elected to life-membership.

Officers for the year 1957–58 are as follows: Paul Meadows, University of Nebraska, president; Manford Kuhn, State University of Iowa, vice-president; Irwin Deutscher, Community Studies, Inc. (Kansas City, Mo.), secretary-treasurer; George B. Vold, University of Minnesota, representative to the American Sociological Society. State representatives are: Otis Dudley Duncan, University of Chicago (Illinois); Thomas E. Lasswell, Grinnell College (Iowa); James A. Anderson, Bethany College (Kansas); Leland Cooper, Hamline University (Minnesota); David B. Carpenter, Washington University (Missouri); Carrol M. Mickey, University of South Dakota (South Dakota); Peter Munch, University of North Dakota (North Dakota); George I. J. Dixon, Wisconsin State College (Wisconsin); and George Wilbur, University of Omaha (Nebraska).

The *Midwest Sociologist* is about to enter its first full publication year under the editorship of Paul Campisi, with a subsidy from Washington University. Subscriptions to the newly expanded journal are available for \$2.00 from the

Secretary-Treasurer of the Society (417 East Thirteenth Street, Kansas City 6, Missouri). Members of the American Sociological Society are encouraged to request their libraries to subscribe.

The Society has accepted the invitation of the University of Minnesota to hold its next annual meeting in April, 1958, in Minneapolis.

Northwestern University.—Melville J. Herskovits, director of the Program of African Studies, has been named president of the newly created African Studies Association by thirty-five leading scholars and specialists on African affairs. Professor Herskovits is currently on a nine-month leave of absence from the University to do research in Africa. The new association was established to stimulate research on Africa by specialists in various scientific fields and the humanities and to facilitate communication among scholars interested in Africa.

University of Pennsylvania.—Thorsten Sellin has been awarded the honorary degree of Doctor of Laws by the University of Upsala, Sweden. He was recently elected president of the International Society of Criminology and has been made chairman of the Mayor's Study Advisory Committee on a new House of Detention in Philadelphia.

Dorothy Swaine Thomas left in March, 1957, for Bombay, India, to assist in the establishment of a research and teaching center in demography which will eventually have students and staff from all countries in the region of ECFA. She is serving as expert for the United Nations. She will return in September.

E. P. Hutchinson has been appointed chairman of a research committee of the American Emigration Conference and consultant to the United States Census Bureau for historical statistics.

Otto Pollak has been engaged in a study for the committee toward an affirmative policy on faculty personnel under the auspices of the University survey being conducted with the aid of a grant from the Samuel S. Fels Fund.

Marvin E. Wolfgang has received a Fulbright Research Grant and a Guggenheim Fellowship for criminological research in Italy, 1957-58.

Norman Johnston, an instructor in the De-

partment of Sociology, is engaged in a research project in collaboration with Stow Symon, the supervising sociologist of the Illinois State Penitentiary, Pontiac. Sponsored by the American Correctional Association, the study is of current procedures of cell assignments and their implication for rehabilitation in adult correctional institutions.

Pennsylvania State University.—Duane Ramsey is on leave of absence for the academic year 1956-57.

Maurice Mook, who was on sabbatical leave during the fall semester continuing his study of Amish communities, has returned.

Arnold Green is on sabbatical leave for the spring and fall, 1957.

George Theodorson, formerly of the University of Buffalo, has been appointed assistant professor of sociology.

Semaines Sociale.—The 1957 meeting of the Semaines Sociale will take place in Bordeaux, France, July 15-20, and will be on the subject of the contemporary family. There will be discussions of the family as related to modern civilization, economic evolution, biology and psychology of today, the advancement of women, the movement of ideas, church and city, fecundity, celibacy, family law, family allowances, and the conjugal situation.

Information as to program and costs may be had from the permanent secretary of Semaines Sociale, 16, rue du Platt, Lyon 2, France.

University of Stockholm (Sweden).—One-year courses are offered by the International Graduate School for English-speaking students in economics, political science, and sociology in combination with efficient instruction in the Swedish language. The courses should be of special value to students who want to see and study the working of economic, political, and social institutions in a Scandinavian country. The twelfth course starts in September, 1957.

Address the School as: The International Graduate School, University of Stockholm, Drottninggatan 120, Stockholm Va., Sweden. All information may also be obtained from the American-Scandinavian Foundation, 127 East 73d Street, New York 21, New York.

BOOK REVIEWS

The Presentation of Self in Everyday Life. By ERVING GOFFMAN. ("Social Science Research Center Monographs," No. 2.) Edinburgh: University of Edinburgh, 1956. Pp. 162. 10s.

At a time when social scientists are calling for "continuity" in social research while at the same time they atomize their research, it is heartening to review the work of one who has, unpretentiously and with enviable tenacity, pursued a single crucial social psychological problem throughout a young and creative career. Goffman first posed the problem in the *British Journal of Sociology* some six years ago: since status is communicated and the appropriate symbolism is amenable to spurious display, how are misrepresentations and their consequences minimized in the social drama? *The Presentation of Self* represents a consolidation of what he has done since then; it holds even greater promise for the future.

Following Kenneth Burke, Goffman has recast W. I. Thomas' concept, "definition of the situation"—a basic item in the conceptual baggage of social psychology—in dramaturgical perspective. Proposed not as a synthesis or alternative, the perspective is merely an addition, capable, incidentally, of informing other points of view. Definitions of the situation are acts, often collective enterprises, controlled by normative standards, which eventuate in recurring organizational patterns and in objective consequences for collective behavior. He implies that definitions of the situation must be sought rather than posited, analyzed rather than described. Such research has been seldom accomplished in the last half-century, although the relevant concept or some equivalent has occupied a central place for at least that long a time.

If the definition of the situation may be regarded as a *human* act, it is also symbolic; the relevant symbolism is called "front." Front includes the perhaps less intimate symbolism of "setting"—furnishings, *décor*, or background items—and the more intimate symbolism of "personal front"—body, clothing, or gesture. An "objective" consistency or grammar provides some guaranty that expectations mobilized by "setting" and vice versa will be vali-

dated in the ensuing interaction, but the guaranties are never foolproof. Thus, all encounters are problematic at the outset, before discourse: the self is not necessarily what it appears to be, nor does it necessarily appear to be what it is. All definitions of situations must overcome this precarious state of affairs if interaction is to persist, and the devices for it are legion and conventional.

People may be brought together as teams in the collective staging of front and are affected at once by the confidential and at times conspiratorial character of the enterprise. The building of the essential loyalties, intimacies, discipline, and a division of labor provides additional hazards. Moreover, those for whom the situation is defined—the audience—find themselves involved in a common circumstance with its ensuing perils. For one thing, any member of whatever audience can always be "taken." Goffman documents the sundry conventions available to minimize the risks of the encounter and get on with it. He carefully scrutinizes the effects of the fact that areas within which definitions are contrived are frequently separated in space from areas in which definitions are communicated (e.g., the dressing room and the stage or the toilet and the parlor) and gives careful attention, too, to the "discrepant roles," such as "informer," "shill," or "mediator," which avert the effects of error and deceit upon team and audience alike.

However, Goffman's work is not merely the provision of a conceptual scheme; from an amazing variety of sociological and anthropological data, he has documented, if not tested, the validity and utility of his argument. This reviewer would hesitate to emphasize the studied personal appraisals and displays in the daily presentations of self, to the neglect of candor, and would like to see situations defined according to the mood of the participants distinguished from those defined by their relative "value." Nevertheless, the monograph under review impresses one as one of the most trenchant contributions to social psychology in this generation.

GREGORY P. STONE

University of Minnesota

Modern Public Opinion. By WILLIAM ALBIG.
New York: McGraw-Hill Book Co., 1956.
Pp. xii+518. \$6.50.

This work is new but related to Albig's earlier *Public Opinion* (1939), about a fourth of the content being common to both books. The years in between have provided a great deal of additional materials, however. In the Preface Albig states: "When I review what I have learned of meaningful, theoretical significance about communication and about the theory of public opinion, I am not so encouraged. Gross generalization and theory have made little advance during these [past] years."

The treatment covers the nature of public opinion and communication, psychological process legends and myths, attitude and opinion measurement, propaganda, mass media of communication, and related matters and shows evidence of the author's long interest in the subject. There are traces of a dramatic unifying plot. Among the characters are the common man, who is "unintellectual . . . unaware of intellectual traditions and the history of thought"; the intellectual, who is just that; the liberal, who has faith in the common man's essential wisdom (with reservations?); the conservative, who resists change, reveres authority, and distrusts the common man, preferring to "elevate elites and experts"; and the "authoritarian propagandist," who seeks to control the mind-life (by means of *Time-Life*?) of publics in the interests of crass materialism and anti-intellectualism. These "good guys" and "bad guys" have been fighting it out, with the bad guys seeming to gain in recent times; but the author hopes for a victory of the good guys and thinks that it would be better if the enlightened managers of the mass media would pitch in on the right side.

This looks old-fashioned and almost quaint in its black-and-white tones, with inadequate evidence for the generalizations; but perhaps we should not judge a relatively new interdisciplinary field by the strict standards we desire for sociology. In any case, it seems certain that those desiring general orientation will find the book entirely satisfactory.

ROBERT E. L. FARIS

University of Washington

The Rape of the Mind. By JOOST A. M. MEERLOO. Cleveland and New York: World Publishing Co., 1956. Pp. 320. \$5.00.

The reviewing of social science books is particularly difficult because the best works typically deal with several related topics at different levels of abstraction and from a variety of viewpoints. The book at hand, subtitled "The Psychology of Thought Control, Menticide, and Brainwashing," is an excellent example. At a minimum, it is a valuable, if shocking, description of recent cases; at another level, a theoretical framework, largely psychological, for the analysis of conditioning, persuasion, coercion, psychotherapy, and, in fact, all methods of influencing the mind. The author offers a wide range of examples, current and historical. The framework, though not well documented, should interest others studying the same topics.

On this generally sound base Meerloo builds a superstructure concerned with human rights and the integrity of the mind. He questions the effects of new industrial techniques and mass communication on modern life. In the chapter entitled "Technology Invades Our Minds" he speaks of the "creeping coercion of technology." He offers few solutions but adds his voice to the chorus raised in warning against the growth of "mass society."

Meerloo is most effective when he discusses the nature and the dangers of the "thought-control" which, he believes, constantly besieges us all. In speaking as a psychiatrist on such problems as Pavlov and the conditioning of the mind, the use of hypnosis, drugs, and even psychotherapy as instruments of coercion, and "the turncoat in each of us," he is concise and enlightening. Recent times offer countless examples of the battle over the minds of masses of people, and Meerloo uses them well wherever he finds them, as in the forced confession of Hungary's Cardinal Mindszenty or the investigations of the American Congress and court system.

The description of man's seemingly unlimited bestiality arouses anger, fear, and sorrow. The author points the finger of blame at all governments and all men, and we must agree. After being alternately warned and frightened, we look for his solutions in vain. The section called "In Search of Defenses" is the weakest part of the book. The 1955 Eisenhower order on the behavior of prisoners-of-war is passed over too lightly. The chapter on "Education for Discipline or Higher Morale" is inadequate.

Perhaps the book suffers most from the failure to relate psychological problems to political and economic reality. When we ask which economic and political systems, which religions and ideologies, advance or retard the struggle for freedom, however defined, the questions go unanswered. The author, though well grounded in psychology, fails to use the best work of economics, political science, and sociology, and this must be counted the major shortcoming of an otherwise excellent work. It is, of course, a failure not confined to Meerloo but one which appears often in social science where the point of view is not interdisciplinary.

ARTHUR JORDAN FIELD

University of Rhode Island

Analytische Gesellschaftslehre. By MAX GRAF SOLMS. Tübingen: J. C. B. Mohr (Paul Siebeck), 1956. Pp. 588. DM. 34.

Gesellschaftslehre might be translated as "theory of social processes" or simply as "sociology," although the author intends the term to convey his own particular orientation. This orientation goes beyond the usual scope of sociology and is considered rather as a part of general anthropology; it deals with the social processes which characterize man as different from even the highest animals, in all places and at all times. The book is the first part of an attempt to build a theoretical master scheme in the grand manner, and, like similar efforts in the past, it falls short of success. This is inherent in the task which the author set himself, the social sciences not having reached as yet the knowledge prerequisite for such a task.

Briefly, the theory here set forth sets up three viewpoints for the analysis of social processes, each of the three having its own laws and subsuming reality under different classifications. But completely outside all these theories are a lengthy set of conditioning factors: the natural environment, the biological-somatic equipment of the human body, the material conditions of the economy, and a long series of attributes and characteristics of individuals, of which feeling, wanting, willing, thinking, and habit are considered especially important. A third of the volume is devoted to the conditioning factors which are remarkable not only for the eclecticism with which they are enumerated but also for the atomistic image of man on which they are based.

Of the three aspects of the theoretical scheme, only two are dealt with here; the third one is briefly indicated, with fuller treatment reserved for another volume. The first one, *Gefüglelehre*, is a social-psychological system of generalizations relating social processes to previously elaborated stages of personality development and to attributes of individuals. The posited ahistorical immutability of human nature is contrasted with its consequences for social relations. The second system of generalizations for the analysis of social processes, *Gerüstlehre*, is formal sociology in Simmel's sense and deals with social structure. The third, *Geltungslehre*, deals with values, norms, signs, and symbols.

The attempt at analysis and synthesis made here is imposing. The author brings to it the wide reading and erudition which one has come to expect from European scholars. It is impressive, however, more in the wide range of historical and literary examples cited than in the theoretical accomplishment. The latter tends to slide over into social philosophy, and, in spite of the emphasis on the scientific nature of the work, there are few propositions or generalizations but many classificatory schemes. The philosopher will find here an attempt to integrate some divergent trends in modern German social thought. The American sociologist will likely be frustrated: intrigued by the convergence with Parsons' trichotomy of personality system, social system, and cultural system; confused by the redefining of familiar terms even of those that have become current in English, like *Gemeinschaft*; if he is research-oriented, he will question the many references to literature and poetry in the absence of references to empirically verified knowledge; and, if he counts himself as an interactionist in the G. H. Mead tradition, he will be impatient with the image of human nature which is far more psychological than social.

In sum, this is a book which will leave few readers indifferent but is likely to satisfy hardly any.

KURT JONASSOHN

Chicago, Illinois

Political Behavior: A Reader in Theory and Research. Edited by HEINZ EULAU, SAMUEL J. ELDERSVELD, and MORRIS JANOWITZ. Glencoe, Ill.: Free Press, 1956. Pp. ix+421. \$7.50.

This new *Reader* meets the high standards set by the Free Press in its previous sociology readers. It will prove exceedingly useful in courses on the scope and method of the social sciences. But the value of the work goes far beyond such convenience. For the editors felt that the compendium is "admittedly self-conscious and in many respects venturesome." Perhaps the temerity exaggerates the degree, for political behaviorism is still regarded askance. But the careful marshaling of able studies in this *Reader* should help to overcome such prejudices.

Included are over forty articles and excerpts written by more than fifty social scientists. Very few items date further back than 1950 (the Founding Fathers Graham Wallas, Arthur Bentley, and Charles Merriam are exhibited briefly). Most specialists in the behavioral viewpoint are likely to respect the discretion which the editors have generally exercised and will probably even find some new items. Non-specialists will find the range of materials an adequate survey. Most key men are represented and by selections long enough to give the reader some real feeling for their work.

The *Reader* incorporates mercifully few articles on methodology as such, being content to exhibit methods in actual use illuminating concrete political subject matter. Opinion structures, political participation and apathy, leadership and communication, and behavior in interest groups, parties, legislatures, bureaucracies, and at the polls—these are the categories within which most of the specific empirical studies are grouped (though the editors' interest is to exemplify methods of research rather than a substantive field).

The editors are careful to avoid fighting with defenders of the legal, historical, and institutional orientations in political science, but some skeptics in those more traditional enterprises may yet doubt if the editors succeed in demonstrating that recent research in political behavior contributes significantly to the main body of political knowledge. In this respect the quality of performance is most uneven. Yet a certain amount of unsophisticated promiscuity (the editors call it "catholicity and eclecticism") may well be pardoned.

The professional sociologist may be pleased when Eulau, Eldersveld, and Janowitz frankly declare that the behavioral approach "seeks to place political theory and research in a frame of reference common to that of social psychology,

sociology, and cultural anthropology." But he may dispute their evident view that this can more fruitfully be done by men who have been sensitized to political phenomena through training in the main tradition of political science than by those whose roots lie in the cognate disciplines. Close study of the *Reader* would give all those interested a broader basis for weighing the contributions of the various social sciences to the study of politics.

H. BRADFORD WESTERFIELD

University of Chicago

Custom and Conflict in Africa. By MAX GLUCKMAN. Glencoe, Ill.: Free Press, 1955. Pp. ix+175. \$3.50.

Originally delivered as a set of Third Programme lectures over the British Broadcasting Corporation, this work can be of considerable interest to the sociologist and anthropologist, for in it Gluckman attacks an important and challenging problem: why and how do social systems hold together despite ever present internal conflict? He answers that conflict itself produces cohesion, and he explains at least one way in which it does so, relying heavily on the well-known monographs on Africa by British social anthropologists and also on his own field work. He does not intend his conclusions to be limited to Africa, however, and makes occasional references to his own and other societies. The cohesive effects of conflict are examined in the feud, authority, the family, witchcraft, ritual, and race discrimination in South Africa.

Gluckman analyzes and demonstrates the cohesive effects of conflict most convincingly in his opening chapter on "Peace in the Feud." Referring to the Nuer, he suggests that "certain customary ties [such as those of a matrilineal lineage] link a number of men into a group. But other ties [such as local allegiances] divide them by linking them with different people who may be enemies to the first group" (p. 10). Then, "if there are sufficient conflicts of loyalties at work, settlement will be achieved and law and social order maintained" (p. 17). The argument centers on Simmel's distinction between *diad* and *triad*: A two-party social system can be held together only by the common interests of Parties A and B, for all their conflicting interests tear at the bond. But, with

the admission of additional parties, their very conflict of interest can serve to bind them. The only requirement for cohesion is that A and B not be allied against C on *all* issues but that, to use Gluckman's words, allegiance and conflict be cross-linked. Moreover, the greater the number of parties and interests, the stronger are conflicting interests' cohesive effects—an argument he again uses in reference to authority, the family, and discrimination.

Like others who have discussed the socially cohesive effects of conflict, Gluckman fails to distinguish conflict in interests or values from discord or strife but discusses them sometimes separately, sometimes together, and claims to demonstrate with one argument how both produce cohesion. The distinction turns out to be critical for Gluckman's work, however, because he makes no case at all for the cohesive effect of strife. His arguments may be summarized. An expression of conflict which leads to cohesion may be ritual, such as ritual role reversal between males and females, which may afford psychological release of tension. At the same time, Gluckman suggests, the very display and indeed exaggeration of conflict affirm the rightness and acceptability of the social order which gives rise to both conflict and harmony. In a challenging aside Gluckman suggests that this is a luxury of strongly cohesive societies in which personal relations are functionally diffuse. Societies such as our own, in which most personal regulations are functionally specific, must be satisfied with direct ritual affirmation of the social order. Again, in his chapter on authority, he illustrates how rebellion against incumbents as distinct from revolution against the social order maintains that social order through channels other than cross-linked allegiances. If the foregoing discussion is intended to refer to the earlier argument about the cohesive effects of conflicting interests, it adds little. If not, it provides evidence only for the much less interesting proposition that custom defines both the establishment of conflict and its resolution and that giving expression to conflict can serve to resolve conflict through various ill-defined tension-releasing psychological means.

The idea that conflicting interests may cement a social system enhances our understanding of social organization. Gluckman handles this idea well, but it is not original with him; the idea has been developed by Simmel and his followers, by E. R. Leach and the Wilsons among anthropologists, and also by some po-

litical scientists. Unhappily, Gluckman does not seek to relate his work to this growing body of thought.

The remainder of Gluckman's discussion contains numerous challenging insights and ideas which may bear rewarding fruit in contexts other than that of his present theme, but it does not support that theme.

ANDREW GUNDER FRANK

Iowa State College

Indonesian Society in Transition: A Study of Social Change. By W. F. WERTHEIM. The Hague and Bandung: W. van Hoeve, Ltd., 1956. Pp. xi+360. \$5.00.

In this work, issued under the auspices of the Institute of Pacific Relations, Professor Wertheim has expanded upon and revised a report originally prepared for the Institute on the effect of Western civilization on Indonesian society. This is the culmination of a distinguished social history which does in fact deal with the dynamics of change rather than in descriptive reconstructions. Not only a regional significance, it is an important contribution to the sociology of social change, acculturation, assimilation, and nationalism.

Wertheim is a historical sociologist, not a cultural ethnologist; his sources are primarily those of Dutch scholars and reporters, from which he builds a coherent analysis of the making of modern Indonesia. Emphasis is upon the broad patterning of change and the types of forces at work rather than upon the changing mode of life in given communities. This is a product of historic research leavened with evaluations rooted in personal experience rather than a contemporary cross-sectional work in which currents of change are assessed.

First giving a geographical and cultural background, Wertheim summarizes the political history of Indonesia from times before contact with Europe to the present day. There follow seven chapters dealing with the development of as many institutions or spheres of life. Thus the major lines and forces of change are shown of economic and religious institutions, the status system, labor relations, urban structure, and cultural and political nationalism. At no point does one flounder in detail for the sake of historicity. Each line of change is treated as patterned sequences in which various forces

(e.g., Dutch policy, Islam, feudal authorities, plantation economy, etc.) are shown at work. If at some points the analysis seems to stress economic forces, there is no hint of special pleading for the Dutch or, for that matter, any Indonesian group.

Sociologists not explicitly concerned with processes of Asian transformation will find particular significance in chapters dealing with the status system, urban structure, and religious reform. Although the author does not fully reconcile the conflicting status systems, he effectively displays the growth of an intricate composite involving status by race, by estate, by ethnic group, and by various achievement patterns. Material on the social and ecological development of city life should help us in overcoming our general provincialism in urban sociology.

Wertheim views the revolutionary movement in Indonesia as more revolutionary politically than socially or culturally. He foresees change but not toward the individualism characteristic of Western society. Specific prognostications are wisely avoided, but, nonetheless, internal and external forces shaping Indonesia are forcefully outlined and assessed.

BRYCE RYAN

University of Miami

The State of the Social Sciences: Papers Presented at the 25th Anniversary of the Social Science Research Building, the University of Chicago, November 10-12, 1955. Edited by LEONARD D. WHITE. Chicago: University of Chicago Press, 1956. Pp. xiv+504. \$6.00.

Introduced by a measured and informative address of welcome by Dean Chauncey D. Harris, this collection of papers commemorates the twenty-fifth anniversary of the University of Chicago's Social Science Research Building. At various points through the volume participants in the occasion remind us of the high hopes that attended the dedication of the building; one is led to surmise that the thirty papers here may indicate the extent to which they have been realized after the brief but eventful quarter-century.

The contributors present views of the basic evaluations of contemporary social sciences which very nearly box the compass. They present a many-faceted examination; a sampling of

the varied perspectives, values, and conceptions of legitimacy and validity that now support or afflict the disciplines that create a scientific understanding of social man. If there be unity in diversity here, it must be supplied by the reflective reader, yet there seems to be at least an agreement among the contributors to take seriously the new place of the social sciences in modern society.

Although gracious in tone, the most severe judgments are advanced by Chancellor Kimpton, who links twentieth-century social scientists with Paracelsus. Mr. Kimpton is well informed on the nature of present research in the social sciences both at the University of Chicago and elsewhere, and one can only assume that his remarks are meant to be taken literally. The image of social science held by this responsible university official is sobering. Those taken aback by it perhaps may find solace in the fact that apparently only those fields which use "the rigid techniques of the laboratory" share in the essence of science; hence, many sociologists may join hands in mutual misery with a large and important section of the physical scientists. From a quite different perspective, Frank Knight repeats and develops his view of man as a "romantic and opinionated animal" whose conduct is only to a very limited extent subject to prediction by the use of scientific methods. Knight's discourse is, as always, subtle and provocative.

At quite another point of the compass we find James G. Miller's "Toward a General Theory for the Behavioral Sciences." Here it is taken for granted that studies of man can be scientific and that a general theory of behavior systems can formulate commonalities among all kinds of living, "open" systems. The formulation is couched in terms such as system, boundary, input-output, coding, information, and equifinality; nineteen general propositions are presented in illustration. The whole conception is an ambitious, even daring, venture, but its abstractness is of a kind not too well suited to the present state of empirical study. The final verdict rests with empirical testing which may be articulated with the present schema.

In similar vein challenges to new modes of thinking are put forward in "Models: Their Uses and Limitations" by Herbert Simon and Allen Newell, where the properties of the newer electronic computers are related to the thesis that there is no fundamental difference, in a certain specific sense, between theories and analogies.

The impact of psychoanalytical thinking on the social sciences is discussed by Harold Lasswell, who himself has had a great deal to do with it. Although Lasswell underplays the reverse, the impact of the social sciences upon psychoanalysis, his discussion is clear, insightful, and judicious within the chosen framework.

From anthropology, Clyde Kluckhohn explores the possibility of comparative study of value emphases in different cultures by means of a fairly complex classification of "clusters" of priorities. George P. Murdock from comparative materials formulates hypotheses of the operation of political moieties and possible lessons for the politics of modern nations.

There is a concise account of the recent work of Bales on decision-making groups, which well illustrates the power of persistent work on carefully delimited problems. This is followed by a stimulating article by Murray Horwitz on psychological needs, which contributes to thinking on "needs" and "valences" in social contexts. Of the separate contributions, space permits only a few comments. Willard Olson discusses biosocial theory of human development; Allison Davis provides an essay on status anxiety; Philip Hauser documents the course of urban ecological studies; and Everett Hughes focuses on some relatively neglected cultural aspects of urban research. The economist, Stigler, says wise things about economic development under high capitalism; Jacob Viner incisively discusses international stabilization; and Theodore Schultz argues for the crucial importance of "cultural" factors in economic development. There is a worthwhile discussion by Berelson on public opinion research; Riesman deftly dissects both the "older" and the "newer" social sciences. The historians and political scientists are ably represented, as, for example, by Louis Gottschalk and Robert Cushman. Practitioners of the civic art say things worth remembering. The humanists, albeit somewhat defensively if not unnecessarily, call upon us to view man as a total being in a world of values.

The fact that so many scholars, scientists, and men of affairs have recorded their thoughts in this volume is a guaranty of the variety of the fare. There is here the inevitable unevenness of nearly all symposiums; on the other side, there are the exhilarating contrasts and shadings of multiple perspectives on complex problems, generated by the work of many strong and subtle minds. Able and distinguished people do not always prepare able and distinguished

papers; in this instance, however, the contributors have given us an unusually competent and edifying presentation of a wide range of interests, standards, and styles of thought in the social sciences of today.

ROBIN M. WILLIAMS, JR.

Cornell University

Adult Education in Transition: A Study of Institutional Insecurity. By BURTON R. CLARK. Berkeley: University of California Press, 1956. Pp. vi+202. \$3.00.

Clark finds that adult education in Los Angeles and in California at large has increasingly become a "service" organization characterized by a "cafeteria-style program with considerable turnover of courses." In contrast with traditional educational practice, the adult schools almost completely adapt their classes to the specific interests of students and the community; here, if not elsewhere, Max Weber's picture of education in America has come true with a vengeance: "The American's conception of the teacher who faces him is: he sells me his knowledge and methods for my father's money just as the greengrocer sells my mother cabbage."

The study clearly specifies the pressures on adult education which have given it the character of a service. Most important is the "enrolment economy." Financing depends directly on the number of students in a class, and those without a minimum enrolment are eliminated. When to the enrolment economy is joined the marginal position of adult education in the public school system and its "wide-open" goals ("education is learning"), a service organization seems almost inevitable. Weber's observation above, of course, raises the question of the extent to which adult education would develop a "service" orientation in a culture different from our own—a question which Clark does not discuss.

By almost any criteria, this is a first-rate case study. Guided by a relatively well-articulated orientation, derived largely from the work of Selznick, it focuses on the organization as a whole, its adaptation to pressures from within and without. This perspective highlights organizational changes resulting from day-to-day adaptations as well as from planning. Clark's work has a theoretical orientation, and he

makes it explicit. His case study is also rich in data, including interviews, documents, observations, financial reports, and questionnaire material. Indeed, Clark's judicious use of these data makes the organization come alive.

Furthermore, Clark's work fulfils that primary, but often overlooked, function of a case study: the providing of general hypotheses. In fact, he might have placed even more emphasis on fully explicit and clearer hypotheses, for some are ambiguous (e.g., "The more diffuse the goals of action agencies, the more an overall institutional evolution will tend to be governed by emergent phenomena").

The gap between concept and data is often very large. The writer seldom specifies what will and what will not be called "pressures," "organizational adaptations," or "unanticipated consequences." Curiously, this gap between theory and data occurs largely in the introductory theoretical statement and in the concluding chapter on implications of the findings for theory and policy. What goes on inside this theoretical sandwich is more substantial. The stated theoretical framework is focused on organizational tensions and adaptations; when deep in his data, however, Clark gives his attention largely to role tension and adaptation. He discusses, for example, the adult school administrator, how his activities and decisions are shaped by various pressures, and how his position is related formally and informally to public school administrators, other adult school administrators, teachers, students, and other clientele. In short, relations of roles are given emphasis in the analyses, if not in the theory. Yet, if they are valuable for the empirical analysis of organizations, should they not be incorporated systematically into the theoretical framework?

The study is intended for educators as well as for social scientists. As Clark himself has noted, he has told administrators little that they did not know before. However, it is likely that school personnel will gain a fresh conception of adult education from this monograph. In addition to his specific recommendations, his interpretation may show the administrators the consequences of alternative policies in what looks like a struggle to the death with junior colleges and the new "community" colleges.

HELEN P. GOULDNER

Urbana, Illinois

The Population Problem of Southern Italy: An Essay in Social Geography. By ROBERT E. DICKINSON. Syracuse, N.Y.: Syracuse University Press, 1955. Pp. xi+116. \$3.00.

This monograph presents the agricultural situation in Italy south of Rome. The problem of overpopulation is viewed in terms of local agricultural resources. There is little attention to fertility and mortality, the crucial variables being identified in the realm of agricultural economics. Students of this region will find the data on agricultural production and physical geography useful. The author has chosen to emphasize the detailed data he has collected, with little attempt to relate his research to studies in other areas or broad theoretical issues.

From the standpoint of "action" research, Dickinson's attempt to compare the success of various agricultural programs to improve the lot of the peasants is interesting. The work concludes with a sobering evaluation of prospects for future reform.

SANFORD M. DORNBUSCH

Harvard University

Crime and Society. By GRESHAM H. SYKES. New York: Random House, Inc., 1956. Pp. vii+125. \$0.95.

Social Movements in the United States. By C. WENDELL KING. New York: Random House, Inc., 1956. Pp. vii+127. \$0.95.

These two works are the most recent additions to Random House's growing series, "Studies in Sociology." Assuming that the *raison d'être* of this unique publishing venture is to introduce the undergraduate and the intelligent layman to sociology and its perspectives in various fields of interest, series editor Page might be scored as follows: Sykes, one hit; King, one error.

In 116 succinct pages Sykes manages not only to cover but also to interpret cogently in sociological terms the heterogeneous materials that naturally comprise the field of "crime." After a brief introduction placing crime in a sociological as well as a legal context, succeeding chapters treat the judicial process, types of crime, criminals, the social control of crime, and the prison community. The author succeeds in making clear to the reader the essential facts

and theories as well as the sociologist's view of crime and criminals as basically social phenomena. Moreover, despite the polemical character of theories of crime and punishment, Sykes manages at all times to be realistic and fair. The Epilogue expresses the belief, increasingly common among sociologists, that the study of crime sheds light on fundamental sociological in contrast to narrow "social" problems.

In contrast, King's rambling treatment of social movements seems to fail on almost every count: coverage, perspective, succinct communication. Apparently none too familiar with the "collective behavior" tradition in sociology, the author approaches his topic in essentially cultural terms and is thereby forced to deal at some length with such generic problems as innovation, selection, and integration as they bear on the larger topic of social change. As a result, despite his laudable concern to see social movements as the agents of social change inherent in mass society, his attention is often subverted from the essential movements themselves. What finally emerges is neither a clear picture of the nature and function of social movements in sociological terms nor, as one might expect from the title, of their incidence in the United States. Socialism is mentioned but once, and communism and native Fascist developments are not referred to at all.

Finally, King has not even correctly interpreted some of the sources which he cites. His conception of the mass society is practically synonymous with that of the urban society; social unrest to him is merely an additive concept; and he hardly appreciates Tönnies' formulation of social movements as social collectives.

HAROLD W. PFAUTZ

Brown University

On the History and Method of Economics: Selected Essays. By FRANK H. KNIGHT. Chicago: University of Chicago Press, 1956. Pp. vii+309. \$6.00.

This is the third collection of essays by Frank Knight to appear in book form. The first, *The Ethics of Competition*, contains some of his essays in technical economics; the second, *Freedom and Reform*, includes chiefly his essays in social philosophy and social action. The present book stands roughly between the two

previous ones, ranging all the way from economics to social philosophy, with a substantial sprinkling of papers on methods in the social sciences. The twelve essays, which were published originally between 1928 and 1951, may be grouped into three classes. The first four deal with economics and the history of economic theory. The last two deal with sociopolitical philosophy and in particular with the problem of freedom. Six essays in the middle are concerned with the purpose and method not only of economics but of social science in general.

These methodological essays are likely to evoke the greatest interest among those social scientists not especially interested in economic analysis in the narrow sense. Here Knight exhibits clearly his basically rationalist approach to social problems, a philosophical position regarded as antiquated by some, especially those who look with some favor upon cultural relativism. Knight's exposition of this approach to social problems is of a high order, even if he is unable to explain fully those phenomena of social life which appear completely nonrational. Most of the essays are concerned with economic behavior in which rational, means-ends-oriented action has considerably greater significance than in other areas of social behavior, such as, for example, primary face-to-face relations or the behavior of crowds.

A second basic social-philosophical position which is clear in these papers is Knight's opposition to scientism, and especially positivism, as applicable to the study of social problems. Knight clearly recognizes that, up to a certain point, human behavior is subject to physical and biological "laws." But these aspects of human behavior are the least important for social analysis. Instead of selecting some specific psychological theory to build a bridge between the natural science aspects of human behavior and the specifically social facets, he creates his own psychology of rationalism. Perhaps his position is most clearly stated in a summary of an article containing a devastating criticism of George A. Lundberg's *Can Science Save Us?* Knight says there:

For a genuine understanding of social phenomena and problems we "obviously" have to use the methods and concepts of various sciences, physical, biological, and "psychological" in a broad interpretation; but, in addition, we have to enter other universes of discourse. Whether "adaptive response" can be interpreted mechanistically is a disputed question; but human beings engage in *rational*

manipulation, which certainly involves more than either or both. And, apart from various forms of quasi-mechanical interaction and one-sided "control" among themselves, they to some extent reach a consensus through discussion, which cannot be conceived in terms either of mechanics or of the use of given means to achieve given ends. Ends are also in part problematic; and discourse which recognizes them as such cannot make sense without reference to norms, "values," which are more than individual wants, as the latter are more than empirical facts.

To be sure, an adequate discussion of the problems posed implicitly in this passage is a most difficult task; probably, Knight himself could not argue that he has been able to solve them fully. But his answer, which includes the courageous postulate that social scientists, as persons and as scientists, *must* choose a value position and that it is most promising and "best" to choose human freedom as the ultimate social value, is a viewpoint which leads to most valuable—even if not generally agreed upon—insight into the social process and the process of inquiry into social problems.

BERT F. HOSELITZ

University of Chicago

Race Issues on the World Scene: A Report on the Conference on Race Relations in World Perspective, Honolulu, 1954. By MELVIN CONANT. Honolulu, T.H.: University of Hawaii Press, 1955. Pp. xiv+145. \$4.00.

Race Issues on the World Scene is of particular interest to those who recognize the necessity of a global view of the complexities of race situations. This book, along with a companion volume, *Race Relations in World Perspective* by Andrew W. Lind (Honolulu, 1955), is a perceptible advance toward "the truth" about how peoples with various racial identification and cultural orientation feel about themselves and live and have their being on "frontiers" around the world. Both volumes are welcome additions to race literature, as each strips off the pseudo-scientific strait jackets that have impeded thought, policies, and practices in race relations during the nineteenth and the first half of the twentieth centuries, when inapplicable concepts taken from theology, cultural anthropology, physical geography, biology, and psychology have become dominant. The latter have crippled theorists and administrators in bringing forth relevant and fruitful propositions for resolving conflicts and reducing tensions wherever people meet.

This volume places emphasis on peoples in contact, the interconnectedness of all mankind; on the processes through which, through their experiences of living on "cultural frontiers," racial groups come to acquire new sensitivities, new perceptions and images of themselves and their relations to other groups in the world. These, as Robert E. Park has said, are the bases upon which moral society rests.

The thirty-five participants of the conference, as the editor implies, avoided the pitfalls of searching for "the facts" of race. Accordingly, if one is looking for facts—"causes of race problems"—he need not bother examining either of the volumes, especially Conant's report; one must be thankful for this—we already have too many "facts." It is a refereshing trend, indicating, as it does, that social scientists are abandoning the superficial assumption that there can be a science of race relations, as it were—through the study of "facts."

The conference, as reported in this volume, may well mark the turning point in the assessment of race issues. The stress is upon the unity of mankind: "The common denominator of all race situations . . . [is] the human component" (p. 143). Moreover, "race is not a subject for scholarly inquiry alone; it is a political issue of extraordinary vitality; . . . racial problems arise when ideas of racial differences become interwoven with the struggles of groups" (p. 4). "No narrow concept of race would meet the problem of analyzing the complex phenomena present in race situations" (p. xii).

The book is written in a popular non-technical style without jargon. The editor of the papers is addressing his report to the lay public, purposively avoiding the temptation to over-conceptualize race issues, yet, nevertheless, capturing the flavor and "meat" of the conferences and producing a masterful compendium of the views of conference participants. Moreover, the editor does not attempt to introduce new concepts, theories, and methodologies; rather, he places race into a broader and more meaningful perspective, adds fresher dimensions to problems, raises sharper and more relevant questions, and suggests fruitful future studies. For these reasons the volume is recommended to sociologists and others interested in understanding the dynamic character of race situations and assessing actions and peoples on racial frontiers.

MOZELL C. HILL

Atlanta University

Social Psychology. By ALFRED R. LINDESMITH and ANSELM L. STRAUSS. Rev. ed. New York: Dryden Press, 1956. Pp. xvi+703. \$5.50.

This thorough revision of *Social Psychology* is a great improvement on what was originally a good textbook. It is, in my opinion, the best of the textbooks in this field. Major revisions are: the devoting of more space to theories that are alternatives to symbolic interaction, namely, psychoanalysis and behaviorism; the stressing of conversation as the prototype of linguistic behavior (p. 200); a greatly enlarged and improved discussion of personality; much more detailed presentation of the relationship between personality and culture and social structure; and the inclusion of topics omitted or merely referred to in passing in the first edition, namely, the development of personality in childhood, changes in personality, and motivation. The book is well written, interesting, and readable.

Perhaps its greatest value, both as a contribution to social psychology and as a textbook, is that Lindesmith and Strauss explicitly state their theoretical position to be that of symbolic interaction. Their exposition is cogent and shows the logical, empirical, and practical superiority of symbolic interaction over competing theories. Two examples will be sufficient here. First, it is demonstrated that the various types of deviant behavior can be accounted for within the same conceptual framework that is used to explain normal and conforming behavior. It is therefore unnecessary to invent a different pathology for each form of deviation. Second, it is shown that a social-psychological theory of personality carries one into adulthood and accounts for the fact that the individual both becomes and remains an adaptive member of human society. Psychoanalytic theory, in contrast, stops one's psychosexual development at about six years and has nothing to offer as a theory of knowledge, language, or self-consciousness. H. S. Sullivan's theory halts "at adulthood." Psychological theories claim that intellectual development begins to decline about when one reaches his majority. Since the existence of elderly people has been discovered by some psychometricians, however, the latter have begun to realize what has for long been common knowledge to the Murngin, and even to anthropologists and sociologists, namely, that adults tend to have greater knowledge than children and that older people are superior to adolescents in coping with situations that

require decisions. Chapters vi-viii in Lindesmith and Strauss could well have constituted a section with the title "The Social Psychology of Knowledge."

The major criticism is that the book fails at several more or less crucial junctures to hold to symbolic interaction or to develop its behavioral and theoretical implications. For example, in arguing that not all thinking involves "the use of words or even of symbols," they write: "Obviously, some sort of thinking process goes on, probably fairly continuously, in lower forms of life. Similarly, the infant is capable of simple thought processes without the aid of language, and adults engage in thinking processes which are not dependent on language or other conventional symbols" (p. 203). Lindesmith and Strauss, through these propositions, abdicate the throne of symbolic interaction. Since animals possess no symbols, one may ask what the means is by which animals conduct their "sort" of thinking. And what is the agency that allows infants to think without language and adults to think without symbols? They claim that the artist—sculptor, painter, musician—works primarily with other than linguistic symbols (p. 204). In their discussion they confuse the finished product (say, a painting), which the artist may not be articulate enough to "explain" to an observer, with the symbolic processes (self-conversation) that enable the artist to create. Hence they seem to err in concluding that language is not necessary in musical composition, in painting, or in sculpturing. But this point is technical. Lindesmith and Strauss have written the best book in the field that has been published to date—a book from which it is a pleasure to teach, and one intellectually stimulating to the student.

FRANK E. HARTUNG

Wayne State University

Sociological Theory: Present-Day Sociology from the Past. Edited by EDGAR F. BORGATTA and HENRY J. MEYER. New York: Alfred A. Knopf, 1956. Pp. xvii+546+iv. \$6.75.

This new book of readings began in a welcome and civilizing impulse: the editors' idea that the dismissal of early theorists is too quick for today's students. They offer this collection to demonstrate the dependence of sociology on its history and the great qualities of observation and analysis of the best "armchair" sociologists.

The selections are drawn from the latter part of the nineteenth century to the 1920's—from Durkheim, Cooley, Simmel, Piaget, Maine, Veblen, Park, Ross, McDougall, Thomas, Mannheim, MacIver, and Linton, in all sixty selections and twenty-six authors. That Cooley is represented by ten selections suggests the general tone.

From a practical standpoint the editors are probably reasonably justified in the title *Sociological Theory*. There is a certain long-haired quality about the essays, for they are clearly more conceptual and more analytical than most sociological writing. It may well be that in most departments of sociology they would be read only in a course on sociological theory. Yet it is also true that most of them deal with specific topics, such as custom, social class, personality, and leadership.

One gets the impression that the editors believe that "theory" refers to whatever is conceptual or partly speculative. However, one may question relegating all conceptual and analytical writing to the limbo of "theory." Preferably, these materials should be taught as part of substantive courses. Whatever conceptual material is helpful should be brought in. If so, though, this book may help perpetuate an anachronistic and ultimately self-defeating educational policy. Insofar as this is a risk, it might be well to recognize its "interim" character in serving a worthwhile purpose until sociological teaching and research has made itself really comfortable with conceptual materials.

A book on theory properly addresses itself to the basic framework of sociological inquiry, whatever the specific subject matter, and to the correlative search for new and more rewarding units of analysis. This may be done historically, by tracing the emergence of a distinctively sociological perspective and by way of such efforts as Parsons' quest for the "social system." The historical study has an intrinsic educational value and therefore should have a secure place in the undergraduate curriculum; but theory as a contemporary study is properly a corner of the field, the preoccupation of advanced students, a curious and precarious area that continually loses its most powerful ideas as they become part of the standard apparatus of sociological analysis.

PHILIP SELZNICK

University of California, Berkeley

How the Soviet System Works: Cultural, Psychological, and Social Themes. By RAYMOND A. BAUER, ALEX INKELES, and CLYDE KLUCK-HOHN. Cambridge, Mass.: Harvard University Press, 1956. Pp. xiv+274. \$4.75.

Unlike much of the vast literature on the Soviet Union, this book deals neither with the government or economy of the U.S.S.R. nor with its policies or ideology. Rather, it is primarily concerned with the impact of all of these on the Soviet citizen. It draws on data assembled by the Harvard Project on the Soviet Social System from hundreds of interviews and thousands of questionnaires administered in 1950-51 to refugees from the Soviet Union. *How the Soviet System Works* is not a summary of these findings but a series of reflections and insights derived from them as well as from more traditional studies of the Soviet Union. Impressive in scope and depth, the reports of the Harvard Project, published and unpublished, are listed in an appendix.

This work may confirm the theory that totalitarianism results when an autocratic regime seeks to maintain itself in an industrial society or in one it seeks to industrialize. Dangers to autocracy arise from industrialization in the form of new groups, notably independent intellectuals, the urban middle classes, and industrial labor, which, unlike the peasantry in pre-industrial areas, are capable of organization and thus of effective political and even revolutionary action. To overcome these dangers, the regime atomizes such a society by spreading its terror and propaganda to the entire population and in this process transforms its own character from old-fashioned authoritarianism into modern totalitarianism. As the book makes very clear, the Soviet regime refuses to permit concentrations of power in any independent group. The all-pervasive terror is almost universally feared. While only 5 per cent of the population are estimated to be completely loyal, almost all feel helpless in the face of its power, and their disloyalty thus remains confined to the realm of thought.

One of the supremely important questions to which the authors address themselves, notably in their final chapters of evaluations and forecasts, is whether an abatement of the terror would make the regime more popular or would, on the contrary, produce ever growing demands for fundamental changes. They present arguments on both sides of this question and appear unable to resolve it conclusively. On the whole,

they are inclined to believe that the regime can, in return for relatively small concessions, obtain significant improvements in morale, efficiency, and productivity and that it is, in fact, moving in this direction.

Conclusions drawn from the reactions, in 1953 and since then, of Central European workers—Poles and Hungarians—with their tradition of organized opposition through trade unions and socialist parties, may not be valid for Soviet workers. As this book reminds us, the latter have a recent background of peasant life and have frequently maintained their ties with the peasantry. And while the peasants are, according to the authors' data, clearly the most dissatisfied people in the U.S.S.R., they—like landless peasants in most countries and periods—are also the least likely to offer active and effective political resistance to the regime.

Other areas on which this thoughtful and stimulating book throws revealing light are the widespread inefficiency and the role of informal and illegal practices as "adjustive mechanisms" essential to the operation of an "overcontrolled, overcommitted and overcentralized" system; the type of society widely desired by Soviet citizens; the regime's basically cautious attitude in foreign affairs; the role of ideology in the process of Soviet policy formation; and the problems of nationalities and religion.

In view of their solid contribution, no one can argue with the authors' choice of emphasis on social and psychological factors in Soviet life. Yet one might doubt whether their stated purpose of providing "a basis for the assessment and, hopefully, the prediction of change" can adequately be fulfilled by means of an analysis of "the attitudes, life experiences, and patterns of adjustment" of Soviet citizens in the various major social groups at one given time. For purposes of prediction, even a fascinating chapter on generational differences can be no substitute for a systematic consideration of the changing role and power relationship of the various groups. This the book cannot provide, since it does not focus on economics and politics. It does, however, contain data to support the hypothesis that industrialization must be accompanied by the rise of a new managerial elite. The authors explicitly, and no doubt correctly, reject any likelihood of "a revolutionary restructuring" leading to "democratization" and "surrender" by the present leaders of their decision-making power. Yet the probability of growing influence within the existing regime of

the industrial managers and of the officer corps, whose tasks of managing a complex apparatus of manpower and machines and whose problems under the Soviet regime, therefore, have many similarities, is not denied. Such a change, no less fundamental because it is gradual, may well be proceeding at the present time.

JOHN H. KAUTSKY

Washington University

Town and Country in Brazil. By MARVIN HARRIS. New York: Columbia University Press, 1956. Pp. x+302. \$4.50.

The first problem in making a good community study is to find the right community. The author has chosen beautifully. Minas Velhas is a town of about fifteen hundred inhabitants in the mountains of eastern Brazil. The nearest railroad is fifty miles away, and until very recently the only approach was by trail. There is no electricity or mechanical power. Despite its smallness and extreme isolation, Harris characterizes the community as urban, and this is the central thesis of the study.

Like hundreds of other mountain settlements in Brazil, Minas Velhas was founded in the eighteenth century as a mining camp. It soon became the administrative center of a large region. When the gold mines were exhausted, the town was not abandoned and scarcely lost population; a variety of small artisanal enterprises took the place of mining. The administrative functions remained and, although much curtailed, still help to support the place today. There is no cultivation close to the compact inclosure of the town. The six rural villages in the vicinity are spatially, culturally, and even racially separate.

The author's insistence that Minas Velhas is urban rests upon a good deal of evidence. Occupational specialization is marked. The 904 persons in the local labor force are grouped into 69 distinct occupations. There are sharp differences in status, and stratification is based upon the compound criteria of wealth, occupation, race, and education. Political interest runs high, and local affairs are strongly influenced by national party politics. On the other hand, religious controls seem weak. Economic life is characterized by a good deal of individualism, and the cash nexus regulates many transactions. The town has such urban features as ballroom

dancing, newspapers, picnics, a public garden, a pool hall, and more retail stores than are needed.

This work takes its place in a long series of community studies directly or indirectly stimulated by Redfield's *Folk Culture of Yucatan*. Redfield's study may be regarded as an attempt to base the Great Dichotomy which dominates so much of sociological thinking (sacred-secular, folk-urban, status-contract, mechanical solidarity-organic solidarity, not to mention *Gemeinschaft-Gesellschaft*) on empirical evidence. The exploration continues to be exciting, marked by such real turning points as Tax's discovery of an impersonal market economy among the Highland Indians of Guatemala and Lewis' documentation of the importance of social change in Tepoztlán. The urbanism of Minas Velhas may turn out to be another fissure in the magnificent but flimsy structure of the Great Dichotomy.

Its effect, however, is somewhat diminished by the enthusiasm with which the author pursues his theme. In several places one cannot see the data for the hypotheses. Harris is not satisfied to show that Minas Velhas has many urban characteristics. He insists in one place (p. 244) that there is no inconsistency between urbanism and folk beliefs and in another place (p. 282) that there is no connection between urbanism and industrialization. Some of the generalizations about the sharpness of the division of labor and the weakness of kinship ties in the community are contradicted by details given elsewhere. These are minor flaws, however. The main outlines of the case are clearly set forth and appear to be factually supported.

THEODORE CAPLOW

University of Minnesota

The Great Plains in Transition. By CARL FREDERICK KRAENZEL. Norman: University of Oklahoma Press, 1955. Pp. xiv+428. \$5.00.

Kraenzel's book is to be valued for reminding us that even Western man cannot successfully disregard the forces of his physical and biological environment, although he may forget them. The author is at his best when dealing with climate, soils, flora and fauna, and farming, with historical reactions to the peculiar conditions of the region, and with the practical

technical problems of surviving the Plains' unbelievable vagaries in day-to-day and year-to-year weather.

Kraenzel is a self-designated "patriot," as well as a student, of the Plains, and it is pleasant to pick up a volume occupied with ecology that, in spite of inexcusable repetitiveness, is not stuffy. But there is more editorializing here than the author's admirably sincere convictions require and a serious weakness for theoretical oversimplification.

For Kraenzel the basic problem of the Plains has been "the nature of the fundamental harmony between climate and civilization" (p. 72). Humid-area civilization, brought into the Plains by most of its settlers and perpetuated in its economic, political, and intellectual life, has institutions and a philosophy basically unadaptable to life under semiarid conditions. Adaptability requires flexibility, mobility, and reserve. The Indians and the open-range cattlemen had it, but they were "done in" by invaders, just as modern Plains children are done in intellectually by educational institutions committed to humid-area doctrines. The problem can be solved only by a dynamic regionalism: regional government, expansion of the co-operative movement, the spread of dry-land farming, regionally sound irrigation projects, and the modification of schools, medical services, and other essential institutions to give them flexibility, mobility, and reserve.

Reversing the gross functionalists, who never found anything dysfunctional, Kraenzel sees no functions for what he labels humid-area life-patterns and so can account for their persistence only by *ad hoc* explanations—historical chance or the shenanigans, consciously or unconsciously motivated, of people with humid-area doctrines. He disregards the warnings of sophisticated ecologists against reification in regional analysis—the region defined by climate and soils is to him "realist." He regards as a "minority" just about everyone living there—religious and ethnic groups, different kinds of farmers, managers and organized labor, professionals—and his account of the social psychology of "minority conduct"—aggressiveness, withdrawal, personality fragmentation, brawling and feuding, preoccupation with special interests, scapegoating, and the like—is confused.

Yet the whole is greater than the sum of its parts, and the book is a useful pioneer effort. His analysis, which though mainly socio-

logical, takes due account of relevant geographical, psychological, economic, historical, technological, and social factors (p. viii), falls short of his goal, but he commands attention to problems highly significant for both social science theory and the national welfare.

WARNER BLOOMBERG, JR.

Chicago, Illinois

Race and Nationality in American Life. By OSCAR HANDLIN. Boston: Little, Brown & Co., 1957. Pp. xiv+300. \$4.00.

Into this volume are gathered, in original or somewhat modified form, thirteen articles published in various general or learned periodicals or prepared for public or learned bodies. Immigrants, slave and free, Negro and other races, are the characters. The drama is the course of ideas and practices concerning these diverse people, all now become Americans, and their fate in this country. Touched upon are the main problems which have been discussed popularly or by serious students of these matters: divided loyalties, prejudice, ideas concerning race.

Handlin has become more than the historian of immigration and the immigrant in the United States; he is the popularizer of this aspect of American history, if "popularizer" be the word for one who writes for the upper-middle-brow public. It is a useful function. There is perhaps the danger that in performing it he will not continue to show the originality and brilliance of some of his earlier work. Sometimes after setting up the straw man of a too simple conception of history (e.g., of the origins of American Negro slavery), he comes very close to giving an answer that is likewise too simple. Every form of discourse has its own temptations; we should not complain too much if a man who uses a particular form occasionally yields to them.

EVERETT C. HUGHES

University of Chicago

Immigrants and Their Children, 1850-1950. By E. P. HUTCHINSON. New York: John Wiley & Sons (for the Science Research Council in cooperation with the Bureau of the Census), 1956. Pp. xiv+391. \$6.50.

This volume in the "Census Monograph Series" is an extended analysis of the geographical and occupational distribution of the foreign-born and native-born of foreign parentage in the United States from 1850 to 1950. The year 1850 is the cut-off point, because that was the first year in which the population was classified in the Census according to native or foreign birth; internally, the study is divided into two periods: the epoch of relatively free immigration before 1920 and the era of restricted immigration between 1920 and 1950.

Significantly, too, 1920 was the year in which Niles Carpenter published his Census monograph with its elaborate inquiry into the composition of the immigrant population up to and including the enumeration of 1920. Since Carpenter gave only passing attention to occupational analysis, Professor Hutchinson's present volume supplements and completes the unfinished work of collecting occupational data for the early period, especially after 1870, when this information was first made available in Census reports. This attempt to supply auxiliary data from 1870 to 1900 or so results in a disproportionate amount of space being given to occupational distribution of the foreign-born in 1870 (one chapter), 1880 (one chapter), 1890 (one chapter), and 1900 (one chapter), while the entire period from 1910 to 1950 receives an amount of space roughly equivalent to the analysis of the single year 1870 or 1880 (one chapter). This will disappoint those who desire a more comprehensive account of the recent period. Part of the difficulty, however, can be accounted for by the more uniform occupational categories in the Census enumeration of recent years and the consequent possibility of including them together as more comparable data (see discussion on pp. 198-99).

Information on country of origin is not available after the Census of 1890, and this led to the gathering of a 3½ per cent sampling of the 1950 Census to determine this factor for first- and second-generation populations. Occupational changes over time thus become measurable within the limits of possible sampling errors (treated in Appendix E).

This is a reference work of inestimable value for the social scientist, supplying, as it does, the means for its own self-correction and interpretation. For example, Appendix A furnishes a complete list of instructions to Census enumerators on items of nativity and parentage from 1850 to 1950. It provides the most complete summary

now available of the following characteristics of the immigrant population and their children over time: age, sex, country of birth, urban and rural residence, distribution by states, and both major and detailed occupational groupings. Hutchinson also furnishes indexes of concentration, both geographically and occupationally, that make rapid comparisons possible on a more sophisticated basis. Although the evidence is too complex for easy summarization, the following quotations will serve as important examples:

There appears to have been more change in the national origins composition of the foreign-born population of the United States since 1900 than in the major occupational characteristics of the separate foreign stocks [p. 265].

Each foreign white stock has its own, and in some cases, a quite distinctive, pattern of occupational distribution [p. 266].

The children of immigrants do not generally follow but rather avoid the occupational specializations of the immigrant generation, are more widely distributed occupationally, and tend to conform more closely to the occupational distribution of the white labor force as a whole [p. 266].

In centering the main attention of his work on the two main periods 1850-1920 and 1920-50, Hutchinson misses the opportunity to focus on smaller units such as the period from 1930-50, where a separate analysis might have revealed more about the specific refugee wave. Furthermore, in presenting the figures on the growth of the labor force and the proportion of immigrants in it, the writer might have clarified the picture by comparisons with total population growth, so that changing ratios could be observed.

These are minor blemishes in what promises to be an important analysis of an entire century of immigration for the specialist as well as the student.

R. A. SCHERMERHORN

Western Reserve University

Leadership and Perceptions of Organization. By ELLIS L. SCOTT. ("Ohio Studies in Personnel, Leadership Series," No. 82.) Columbus: Bureau of Business Research, College of Commerce and Administration, Ohio State University, 1956. Pp. xvii+122. \$2.00.

This brief and detailed research report is concerned mainly with the accuracy of percep-

tion of the positions of superiors, peers, and subordinates by 630 enlisted crew members of ten submarines and with relating it to leadership nomination, morale, and operational effectiveness. A second focus is reciprocated perceptions of status relationships in relation to the same measures of effectiveness.

The primary source of data is a "segmental organization chart" in which each enlisted submariner was asked to show his own position and to name and indicate the position of "those working close to you." The enlisted man's statement was compared with the relations indicated in the formal organization chart and a number of types of error scores computed, including omissions, nomination of persons outside respondent's unit, and misidentification of another's organizational level. From the same data mutual nominations, or reciprocations, were tallied, and a number of scores were derived. Both kinds of scores—accuracy and reciprocity—were finally correlated with ratings and paper-and-pencil measures of morale and effectiveness. These "payoff" correlations tend to be low and statistically not significant, though an occasional relationship appears substantial enough to be ground for speculation.

The author presents his work as an exploratory study and quite properly suggests that its chief value is to suggest hypotheses for further research. The author has explored, with painstaking thoroughness, all the combinations of scores derivable from his major tables, but he has done so almost without the aid and comfort of theoretical ideas. The failure to find appreciable numbers of significant relationships tends to lessen the suggestive value of the research, for the reader is left in doubt as to whether there is any important relationship between the perception of organization and effectiveness, whether the measurement techniques are perhaps inadequate, or whether the relationships are more complicated and devious than can be detected by linear correlation techniques.

Finally, the usefulness of the data is reduced by the somewhat unsophisticated analytic techniques. For instance, in calculating error scores, the author has not adequately taken into account individual differences in the number and variety of a respondent's charted relationships, in consequence of which, for example, respondents who have the greatest number of subordinates appear, at first glance, to be least accurate in perceiving their organizational relationships. Since this sort of analytic technique makes one distrust the results as ground for

speculation, it might have been more useful for future research if the simple, raw data had been presented intact, so that artifactual findings did not appear.

HENRY W. RIECKEN

University of Minnesota

A Preface to Democratic Theory. By ROBERT A. DAHL. Chicago: University of Chicago Press, 1956. Pp. xi+155. \$3.00.

Dahl's *Preface* develops classical political theory into forms which allow it to be enriched by contemporary social theory and modern methodology. First, with admirable rigor the author formalizes the basic goals, definitions, and means-end statements of Madisonian and Populistic theories of democracy. His analysis of these two formalized systems lays bare logical inconsistencies within them and emphasizes the extent to which they tend to be axiomatic rather than real-world theories which explain American political behavior. Then he describes the "American hybrid" by constructing an ideal type, "Polyarchal Democracy." In this theory he hypothesizes that achievement of the Madisonian goal of non-tyranny by either majority or minority, with popular sovereignty and political equality, as a function of an array of both formal (constitutional) and informal (practice) conditions which exist inside the governmental processes and within the social system itself. Dahl's analysis of the conditions of consensus on political procedures and alternatives within the social organizations constituting the polyarchy makes important use of the concept of social norms.

Students in the behavioral sciences will enjoy Dahl's penetrating analysis of the problem of intensity of preference in chapter iv. He relates the subjective "sensate intensities of want or preference" to their operationalized form in observable "intensity ratings" and then sketches the ways in which these expressed intensities are useful in predicting choice.

Dahl is quite clear as to whether the various issues raised by his formalization of the three theories of democracy are ethical (value-oriented), logical (consisting of interrelations among definitions and derivations), or empirical (being testable propositions about operationalized variables). His use of more formal, symbolic devices in the footnotes and Appendix allow him to make elegant presentations of his

analyses. His use of empirical data from time to time hints at the potential which exists for empirical work in democratic theory. The reader, for example, will be intrigued with his empirically supported conclusion that neither Supreme Court juridical review nor equal representation of the states in the Senate provide relief from tyranny in the "situation where a relatively intense minority prefers an alternative opposed by a relatively apathetic majority" in the American hybrid (pp. 109-19). Dahl works masterfully as a social behaviorist, while focusing the essential features of our political heritage.

Although the author, as he himself clearly understands, has only prefaced the development of democratic theory into more testable forms, his work can be built upon by behaviorists interested in political processes. It is an important step forward. A tremendous gap remains to be closed, however, between the formalized theory and the formulation of practical ways of testing it empirically. Dahl recognizes this, too, and at times seems discouraged. Little wonder that he, as political scientist, invites the "aid of our colleagues in the other social sciences."

HAROLD GUETZKOW

*Center for Advanced Study in the
Behavioral Sciences*

Les Parties communistes d'Europe, 1919-1955.
By BRANKO LAZITCH. Paris: Les Isles d'Or, 1956. Pp. 255.

This is a manual of data on election returns of all European Communist parties since 1920, biographical data of major Communist leaders, and short histories of particular Communist parties and congresses. In addition to this useful information, the thirty-page Introduction offers some generalizations on the nature of communism. The author observes that there is no clear relationship between economic depressions and growth of communism. The latter is due to the disintegration of the state. If the democratic state is strong, communism cannot advance. The second cause of the growth of communism is its successful exploitation of popular fronts with the socialists, according to Lazitch. But the relationship into which the Communists enter with other groups is not necessarily to the advantage only of the Communists; it is a two-way relation.

The book covers communism up to Stalin.

Polish Communist leader Gomulka is not listed among the Communist elite. The role of Titoism is not analyzed.

JIRI KOLAJA

Talladega College

The Nature and Function of Priesthood: A Comparative and Anthropological Study. By E. O. JAMES. New York: Vanguard Press, 1955. Pp. 336.

This book compares the role of the priest in the rituals of a number of ancient and primitive religions as well as in Christianity. The author is professor of history and philosophy of religion at the University of London.

The priest is viewed as the official organ for conciliating and supplicating sacred powers. The priesthood as a profession has developed through increasing specialization of ritual actions. The primitive unspecialized officiant merged functions of magician, shaman, and priest. The seer, diviner, and oracle, like the prophet, appeared as agents of revelation. Unlike these practitioners, the priest acts as mediator between the sacred and the secular through his management of institutionalized ritual procedures, and this distinguishes priests from other religious or ritual practitioners.

While the book describes primitive, Greek, and Far Eastern ritual, its primary focus is on the ancient religions of the Near East and early Christianity. In the analysis of the Egyptian system of divine kingship and in the Hebrew rituals of sacrifice, James finds the clearest expression of the function of the priesthood. The basic myth of fertility and renewal appears in the rituals of divine kingship through which the faithful were assured that the gods would maintain the universe in a manner consonant with man's well-being. The Hebrew rituals were similar in function. In both cases, the priest, or the divine king acting as priest, is the manager of the ritual whose consecrated position is essential to effectuate the link between man and god.

As performer of the sacrificial ritual, through the Mass, the Christian priesthood achieved its greatest influence. James also traces other roles of priesthood, such as the granter of absolution, the custodian of sacred and pseudo-scientific lore, and the source of legal judgment. In all these the priest appears as the duly constituted mediator between the sacred and secular

worlds, leading the author to his major conclusion: "The priestly tradition has been a cohesive force enabling its members to live together in an orderly system of social and religious relations consolidated on a transcendental basis" (p. 298). James implies that weakening of this tradition has led to contemporary social disorganization and the re-appearance of pseudo-priestly functions through totalitarianism.

The book presents a rich fund of materials on world religious rituals and myths and their history and appearance in different religions. The materials on primitive religion suffer from a failure to devote much attention to the North American Indians.

The least convincing part of the book is its attribution of a stabilizing function to the office of priesthood. In making this assertion, James has confused duties with functions. His description is of the role of priests in ritual. His assertions sweepingly cover the office of priest. "Function" is a conceptual knife with more than one blade. As he himself recognizes, the power of the priesthood, resting on its ritual management, may be a conservative force preventing change and hence leading to conflict, as in the late Middle Ages. It may be a source of discontent, as in the conflict which emerged between the Pharaoh and the priesthood in ancient Egypt. For the sociologist it may be useful to ignore the function of priesthood. It may be more fruitful to inquire into the conditions under which the priesthood has functioned in different ways.

Despite this criticism, the book is a valuable study of myth, ritual, and comparative religion.

JOSEPH R. GUSFIELD

University of Illinois

Patterns of Mothering: Maternal Influence during Infancy. By SYLVIA BRODY. With an Introduction by RENÉ A. SPITZ, M.D. New York: International Universities Press, Inc., 1956. Pp. 446. \$7.50.

In 1945 Hartmann and Kris published a vitally important paper, "The Genetic Approach in Psychoanalysis," in the first volume of the *Psychoanalytic Study of the Child*. The authors pointed out that psychoanalytic propositions had been primarily concerned with intrapsychic dynamics, that these had been

more fully elaborated and accepted, and that there had been a striking lack of interest in explanations of dynamic behavior through the investigation of antecedent events. Though the *Psychoanalytic Study of the Child* is currently in its eleventh year of publication, the criticism still applies. The series has been characterized by an inability to relate psychoanalytic data to observations in experiments, particularly when dealing with the mother-child interaction.

Miss Brody's book is a wholesome attempt to bridge this gap. The body of the book is concerned with an analysis of the detailed behavior of thirty-two mothers of infants in the first half-year of life. The material for her study is from the Infancy Research Project conducted under the joint auspices of the United States Public Health Service and the Menninger Foundation of Topeka from 1948 to 1951. The author's frame of reference is essentially psychoanalytic. Her thesis is that the mother's interaction with her infant is best mirrored in the activity of feeding and that this experience is the most useful paradigm for her total behavior toward the infant.

The book also contains an extensive review of literature dealing with maternal behavior, infant growth, and the feeding behavior of infants. Though experimental clinical and cultural findings are presented, the material dealing with behavior which results from psychic conflict is most widely represented. The final section deals with current theoretical concepts regarding ego development as a product of maternal behavior and the vicissitudes of early drives.

Miss Brody's contribution to the understanding of maternal behavior and the construction of a typology for the description of such behavior is fascinating in combining direct observation and psychoanalytic methods. She wisely separates her observational data from her speculations and treats these as separate units of the book.

The content analysis of mothering behavior is described with a perceptiveness and empathy which should serve as a model for other researchers. Miss Brody has a remarkable facility for avoiding jargon and is able to describe clinical phenomena in rich and imaginative ways so that the reader can easily grasp the unique significance in mother-child interaction. It is perhaps too early to categorize mothering behavior in specific typologies, a difficulty of which the author is completely aware.

The book is a most welcome contribution to

the systematic observation of children by psychoanalytically trained observers. It is hoped that it heralds the beginnings of many such studies, so that eventually systematic observations of life-histories from birth to adulthood become integrated into personality research.

HARRY TROSMAN, M.D.

University of Chicago Clinics

The Core of the City: A Pilot Study of Changing Land Uses in Central Business Districts. By JOHN RANNELLS. New York: Columbia University Press, 1956. Pp. xxix+233. \$5.50.

The contribution of this monograph is "a scheme of relationships among activities and some methods for measuring patterns of land use" (p. 188). Despite the subtitle, the data pertain only to the city of Philadelphia, comprising land-use surveys on a block basis of the number of establishments and floor space utilized by major kinds of business in the city's central business district in 1934 and 1949.

The taxonomic portion of the volume develops fairly conventional concepts like "activity," "establishment," "systems of activities," "linkage," "kind of business," "accommodations," and "facilities." Such classifications are, of course, an indispensable starting point for empirical research; but the value of any particular classification can be judged only by its ability to order research findings, a criterion on which Rannells' categories remain to be tested.

Another major part of the book is devoted to the development and illustration of techniques for measuring locational distributions and their relationships. Reliance is placed mainly on centographic techniques and elaborations of Lorenz curve analysis, together with cartographic representation. Various abstract or schematic diagrams also are introduced as devices for summarizing relationships. Much of this material is ingenious—though perhaps less original than might be assumed from the lack of reference to the substantial body of previous work with similar techniques. However, the proposed techniques are unnecessarily complex. No doubt, their extensive application in systematic research would result in considerable simplification and the elimination of redundancy.

Fortunately, the data of the study—em-

played largely for illustrative purposes rather than for rigorous tests of hypotheses—are presented rather fully in raw as well as summary form. Therefore, other specialists in the field may compare alternative techniques of analysis with those of the author on a common body of information.

The author recognizes that his volume falls short of “a substantial contribution to urban location theory” in that it comprises only “two half-books: a beginning at theory and a somewhat fuller treatment of methods” (p. vi). But the reader is not so likely as the author to be sanguine about the prospect that the synthesis of these two contributions will produce a full-blown theory of spatial structure and change.

The volume can be recommended for study by specialists in urban land-use analysis and, indeed, by all research workers attempting to cope with problems of method in the study of urban structure. However, it has little to contribute to those with only a general interest in the urban community or to writers of textbooks in urban sociology.

OTIS DUDLEY DUNCAN

University of Chicago

Risk and Gambling: The Study of Subjective Probability. By JOHN COHEN AND MARK HANSEL. New York: Philosophical Library, Inc., 1956. Pp. x+153.

This little book presents in popularized form a number of investigations into the subjective sets of children and adults as to chance events and the uncertainties of everyday risk-taking. The popularized style makes for uneven quality in the reporting of the relevant research and for difficulty in evaluating it. However, as an amusing account of the foibles of children and adults in risk-taking, this book ought to interest teachers of statistics as well as students of child development. This research documents many of the beliefs about chance which inhibit learning of mathematical probability theory as taught in statistics courses and, as such, would constitute valuable outside reading for both student and teacher.

By “subjective probability” the authors mean “the expression of our states of uncertainty.” They have studied the changes in ideas which influence risk-taking from the seven-year-

old’s belief in luck, fairness, and the dependence of events in a series, through the first recognition of the concept of statistical independence at the age of ten to fifteen, to the final adult manifestations of risk-taking in, for example, gambling and crossing the street in front of an oncoming automobile. Cohen and Hansel have shown the significance of subjective probabilities as action-guiding beliefs in numerous decision situations and have uncovered a host of interesting areas for research.

The chapter entitled “The Language of Uncertainty” describes a study in which subjects were asked to give numerical values for adjectives denoting quantity, frequency, and duration. This chapter is relevant for the design of questionnaire and attitude-scale studies. For example, an adjective, “many,” takes on the median numerical value 15.5 with reference to “friends” and the median numerical value 175.5 with reference to “stones.” Further development of this type of study could be of great use.

As a first exploration of the attitudes upon which people act in taking risks of the development of these attitudes during childhood, and of the differences between subjective and mathematical probability models, this is a stimulating book. Its relevance for social psychology is evident, but the mixture of research reporting and popularization in it is disappointing.

ROBERT J. POTTER

Chicago, Illinois

Physique and Delinquency. By SHELDON and ELEANOR GLUECK. New York: Harper & Bros., 1956. Pp. xviii+339. \$6.00.

In *Physique and Delinquency* the Gluecks have employed a part of the massive data on five hundred adolescent delinquents and five hundred controls that was drawn together in their study *Unraveling Juvenile Delinquency*, published in 1950. In the present work they provide a deeper analysis of the interrelations between body types and selected traits and sociocultural factors.

It was their purpose to determine what relationships might exist between four major somatotypes, some of which had previously been found to differentiate delinquents and non-delinquents generally, and forty-two sociocultural factors. (For summary of these see pp.

27-31.) More particularly they sought to discover *inter alia* what traits and factors exert a statistically significant differential influence on the delinquency of different body types and which, though perhaps associated with delinquency itself, do not. While following in general the somatotype criteria and the broad classifications established by W. H. Sheldon, the Gluecks have found it necessary in this research, in order to develop an intercorrelational analysis of relatively small samples, to employ only four physique-pattern categories; Sheldon has identified some eighty-eight.

As in *Unraveling Juvenile Delinquency*, it is pointed out here that delinquents are differentiated from non-delinquents by their mesomorphic physique and that ectomorphy is relatively uncommon in the former. These findings conform to the observations of W. H. Sheldon. However, the Gluecks probe more deeply into the relationships with other variables to reveal what appear to be traits distinguishing non-delinquent mesomorphs from other physique types (e.g., physical strength), traits contributing selectively to the delinquency of mesomorphs (e.g., destructive-sadistic drives), and sociocultural factors contributing selectively to the delinquency of mesomorphs (e.g., lack of family recreation). Similarly, they analyze the traits and factors associated with endomorphic and with ectomorphic delinquents and non-delinquents. From the data they arrive at three major conclusions:

1. The basic morphologic differentiation of the physique types is accompanied by differences in the incidence among them of certain traits, some of which are actually associated with delinquency, others potentially so.

2. Differences in the physical and temperamental structure of body types bring about some variation in their response to environmental pressures.

3. Differences in the incidence of certain traits among the physique types, as well as divergences in their reactions to the environment, are reflected in certain differences in the etiology of delinquency among the body types [p. 249].

Again in this work, as in *Unraveling*, they propose that their materials be used to distinguish at school entrance those children who are headed for delinquency. They suggest (chap. xv) that the present refinement of their material relating to physique may add new dimensions to prediction and treatment, particularly, as to the latter, through establishing programs in the family, school, recreational re-

sources, and clinic that will take into account the relationships between body types and those traits and factors through which delinquency may be facilitated. This produces a serious question, aside from probable skepticism about the prediction instruments and the efficacy of our treatment tools: How can the future body type of the child be determined at the time of his entry into public school so as to permit an application of special measures designed to meet his constitutional needs? In *Unraveling* the Gluecks said:

As regards physique, we are dealing with a discipline as yet highly controversial because physical anthropologists have not yet answered a major question, namely, whether or not the somatotype remains constant and, if it does, whether, in the formative years of growth around the age of six or seven, when children normally enter school, the physique type is as yet reliably distinguishable. For this reason we did not utilize the differential findings in the comparison of the physique of the delinquents and non-delinquents for predictive purposes [p. 258].

This limitation on the practical applicability of their data is not discussed in the current work.

The Gluecks suggest at several points the desirability of further research to follow up the leads that are set out here. We may hope that more studies will be made into the relationship of constitutional factors to delinquency. However, companion studies to *Unraveling* or *Physique* would be extremely expensive and time-consuming, and something more than superficial follow-up projects are needed to test their suggestive hypotheses.

PAUL W. TAPPAN

New York University

The American Experience of Swedish Students: Retrospect and Aftermath. By FRANKLIN D. SCOTT. Minneapolis: University of Minnesota Press, 1956. Pp. xiii+129. \$3.00.

Indian Students on an American Campus. By RICHARD D. LAMBERT and MARVIN BRESSLER. Minneapolis: University of Minnesota Press, 1956. Pp. xi+122. \$3.00.

Foreign Students and Higher Education in the United States. By CORA DU BOIS. Washington, D.C.: American Council on Education, 1956. Pp. xv+221. \$3.50.

The books by Scott and by Lambert and Bressler are reports upon projects sponsored by

the Social Science Research Council as part of its program of research on "cross-cultural education" (see prefaces to both volumes by Ralph Beals). The book by Cora du Bois is a general work designed primarily as a guide for people engaged in exchange-of-persons projects.

Scott's book is a concise, well-written, and generalized account of the experiences of the Swedish student who has studied in the United States and returned to his homeland. Scott is a historian, and his book has the flavor of skilful historical journalism. The Lambert-Bressler document, while equally well written, utilizes familiar social science concepts and modes of presentation. In spite of these considerable differences in style and approach, the findings are on the whole comparable and complementary.

Scott's image of the Swedish America-educated student may be summarized as follows: The Swede approaches America as one of the several great cultures in the Western tradition: a large and powerful country, but one not essentially different from his own. He finds some things—notably sexual relations and the paternalism of bureaucracy of the American university—somewhat strange, but beyond this he has little difficulty. On the whole, he takes America in his stride; he tends not to blame it for his failures, if any, and he views his experience in the United States more as a phase of his education and less as a profound personal adventure. Back home, he generally puts his American training to good use, although he must "prove himself" in Swedish, not American, terms. In addition to his special professional training, the Swedish student educated in America acquires a broader outlook, is more tolerant of cultural differences and is able to get along with a wider variety of people than his stay-at-home colleagues.

Lambert and Bressler offer a portrait of the Indian student in America which differs from the Swedish picture in almost every detail. Since the book strives for an objective analysis of process, the dynamics of the Indian response to America may be summed up in the authors' own words:

The Indian student in his unofficial ambassador role finds his country and by extension himself accorded a low status in the eyes of Americans. Moreover, in his interaction with individual Americans he is constantly affronted by statements and questions which touch upon historically sensitive areas. As a result of these twin processes, he develops a set of defense mechanisms to protect him from the ego

assaults which would result from an acceptance of the low status ascription. On the one hand, he devalues his perceptions of the American culture. On the other, in his ambassadorial role, he exalts the value of his own culture [p. 80].

Thus, there emerges a picture of the rather assertive, argumentative, "touchy" Indian student sometimes seen on American campuses, eager to learn but constantly aware that he must defend and exalt his own country, and, at the same time, uncertain whether his country is Western or Oriental. While as a student the Indian student seems to do as well as the Swede, from the ideological standpoint he has much greater difficulty—in fact, there is really no likeness between the two. Moreover, upon return home, the Indian encounters considerably more difficulty in adjustment and more ambiguity of role than does the Swede. Top educational prestige in India still tends to be assigned to the graduate of a European, and particularly English, university, and the grading system of the American university is so alien that returnees have difficulty fitting into the normal channels.

It is apparent from these two interesting books that highly educated members of modern societies may carry with them a powerful sense of national identity, which operates so as to determine or modify their responses to environments. More important, this sense of national identity has much to do with the feelings of security and confidence ("self-esteem"—see the Du Bois book for a general discussion) in the ability to manage the environment. And, further, it tends to determine which aspects of the environment are perceived as gratifying or threatening.

Another way to view the difference between Swedish and Indian behavior emerges in the suggestion of Lambert and Bressler that when Indian students simply fit into the convenient slots of American society, accepting their role as students, they behave in a variety of individual and unexceptionable ways. But when they attempt to *manipulate* (pp. 16–17) the American environment—for example, in their assumed "ambassador role" of "selling" Americans on India and reacting to America from the standpoint of Indian notions of cultural difference—they not only experience difficulties in adjustment but behave in a highly uniform manner. Swedes, on the other hand, never seem to attempt to "manipulate" the American environment (p. 53) but continually accept it as

student or guest. As-yet-unpublished Japanese data show that many Japanese students neither manipulate nor accept but simply expect America to live up to their ideal image and feel frustrated when it does not. Clearly, intercultural experience and learning are complex problems and cannot be understood with the relatively simple and uniform concepts of "marginality" and the like.

Another research lead which is suggested by these materials concerns the problem of consistency or continuity of the individual personality. Since to some extent—especially in Scott—we obtain a picture of an individual changing through time as he moves through a series of different experiences and differing environments, one can approach the question of the individual's relationship to society in a fresh manner. It is possible to obtain some notion of precisely how large a contribution the situation and role, and the individual's awareness of history, play in determining behavior. But it is clear that the meaning to the individual of an education abroad changes continually as international relations change and as his perceptions of these relations change. He continually relives his experiences, changing his views of them, and thereby changing the effect of his experiences upon his own society.

The book by Cora du Bois brings to focus the propositions underlying much of the above. Her book is a preliminary formulation, based upon tentatively analyzed data. Yet it hits all the high spots and provides a workable manual or outline for the foreign-student adviser and the exchange-of-persons program planner and operator. In addition, it provides detailed historical and statistical materials on the changing patterns of foreign study in America and on the

role of various private and governmental agencies in the field. The adequacy of the American university's management of the foreign student is analyzed in detail.

Of greatest interest to the social scientist is Part II, a preliminary theoretical analysis of accommodation to American life and culture and the typologies of adjustment developed in the Social Science Research Council and other studies which may enable the scientist to predict certain dimensions and qualities of intercultural experience. One of the most suggestive, but as yet very vague, formulations is of the "stages of sojourn adjustment" (spectator—adaptation—coming-to-terms—predeparture) which offers possibilities for generalization. Particularly interesting is the finding that sojourn adjustment differs from returnee adjustment: in the former the individual is in a new environment, in a sense released from responsibilities; in the return phase, he once more must come to terms with old role expectations—but with changed outlook and behavior.

The final assessment of the accomplishments of this research must await the publication of all the reports and of a proposed, but not yet announced, over-all synthesis to be published as a separate contribution, probably under the authorship of Brewster Smith. At present the chief value of the publications is their explicit information upon national groups of students. The phrase "cross-cultural education" presumably connotes a whole series of processes centering around learning and acculturation, only a few of which have been developed in the three volumes reviewed here.

JOHN W. BENNETT

Ohio State University

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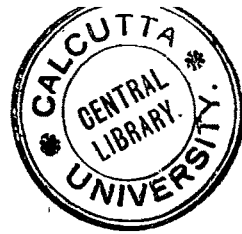
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THE AMERICAN JOURNAL OF SOCIOLOGY

Volume LXIII

SEPTEMBER 1957

Number 2

TWELVE YEARS LATER: AN ANALYSIS OF FIELD EXPERIENCE

ROSALIE HANKEY WAX

ABSTRACT

This paper is an analysis of that crucial period of field work during which the student changes from a novice to a professional. The data are provided by a detailed description of the personal experiences of a field worker attempting to begin work in a relatively difficult situation. The account suggests that one of the most salient tasks faced by the novice in a difficult field situation is the definition of his role. In achieving this definition he passes through three stages: (1) the stage of insecurity of role, (2) the stage of gradual definition of role, and (3) the stage of validation of role. In the course of this development the student usually believes that he is achieving his ends by *learning* from his informants. In point of fact, however, he is *teaching* them to assume the role behavior which will enable him to learn from them.

From July, 1943, until May, 1945, I lived as a participant observer first in the Japanese Relocation Center at Gila, Arizona, and later in the Tule Lake Center for "disloyal" Japanese in northern California. Immediately on leaving the field, I wrote a long autobiographical saga describing my experiences in full detail.

In recent years, colleagues, distressed by the scarcity of detailed accounts of how an investigator works, have repeatedly urged me to publish this material. I pointed out that it was crude, unstructured, and that I could not possibly rewrite it without robbing it of its one literary virtue—its conscientious and engaging naïveté. Finally, an astute colleague suggested that I present it in the form of excerpts with appended analyses. Here, then, is a shortened version of the first part of the document, which describes what I did and what was done to me during the first five months I spent in the Gila center. The analysis which follows was written in 1957.

I arrived in the Gila center in southern Arizona in July, 1943. The community at this time numbered approximately 12,000

people. The administration assigned me to a bedroom in the women staff workers' barracks. Since fraternization with evacuees was not encouraged by the administration, receiving Japanese visitors in this room was not good policy, and almost all interviews had, perforce, to take place in evacuees' living quarters or the administrative offices in which the evacuees worked.

I began work with one Issei informant who had been employed for the study¹ of the center and I hoped to acquire additional informants through half-a-dozen letters of introduction to Japanese friends of the anthropologist who had previously worked in Gila.

In establishing an acquaintance with these and other potential informants, no role was possible but that of a student of sociological phenomena. The assumption of an occupation such as teacher or nurse, under which covert studies could have been car-

¹ For a detailed description of the relocation and of the centers see D. S. Thomas and R. Nishimoto *The Spoilage* (Berkeley, Calif: University of California Press, 1946).

ried out, would not have been compatible with the policies of the War Relocation Authority. Accordingly, I introduced myself as a student of anthropology, hired by a group of professors who had no connection with the administration. The object of my employers was to obtain and publish the true picture of the evacuation and life within the centers.² I emphasized the confidential nature of my data and added a description of some phase of the study which was likely to meet with the approval of informants. Most evacuees, for instance, were pleased to hear that a study of Japanese financial losses due to evacuation was under consideration. Since informants almost invariably asked me about my occupation and "why I lived in camp if I didn't belong to the WRA," this self-introduction was usually made at the first interview. (I never used the term "informant." It carried the connotation of "informer." Instead, informants were called "the people who talk to me.")

In my inexperience I had high expectations of the effectiveness of this initial role. I knew that the status of student carried considerable prestige with the Japanese. I anticipated that the academic objective would meet with approval, reasoning that the evacuees would be pleased with a correction of the inaccuracies previously published by ill-informed and prejudiced persons. Above all, I expected that the fact that the study had no connection with the WRA administration would facilitate gaining rapport.

Optimism, however, was rapidly succeeded by bewilderment. Calls upon evacuees to whom an introduction had been provided yielded little but a polite exchange of amenities. When any specific question on attitudes to the center was asked, the informant usually professed ignorance or else took refuge in verbiage phrased to conform with what he thought a Caucasian would like to be told or ought to be told. The Issei

informant also proved to be a cautious individual who confined his reports largely to innocuous topics.

Perhaps the most important handicap during this early period was my notion that there were two distinct varieties of Japanese, a "pro-American" and a "pro-Japanese." This incorrect idea sprang from my emotional reaction to the current anti-Japanese propaganda, a reaction which took the form of a stubborn faith that the great majority of Japanese residing in America did not look with any favor on Japan, did not seriously consider expatriation or repatriation, believed that the United States was going to win the war, and, in short, held many of the attitudes which I imagined I would hold in their place. The other variety of Japanese, I thought, was "pro-Japanese" and comprised a small group, inclined to violence and responsible for all the disturbances in the centers. With this group, I had been told, it would be almost impossible to make contact.

This incorrect preconception greatly increased the difficulties of gaining a proper perspective of the facts, which were that only a few evacuees belonged in either category and that the great majority vacillated between the two extremes as they were moved by events inside and outside. It also increased the difficulties of interpreting data, since significant statements betraying the vacillation of persons classed as "pro-American" were ignored or relegated to the status of incomprehensible phenomena. Finally, it unnecessarily limited contacts, since I later discovered that the so-called agitators and those residents who were more sympathetic to Japan were approachable, once the proper techniques were applied.

At the conclusion of the first month of work I had obtained very little data, and I was discouraged, bewildered, and obsessed by a sense of failure. Nevertheless, one fact at least was now apparent: attempting to get specific and accurate information on the matters in which the study was interested was, at this period of the investigation, poor technique. A radical change of approach was

² No specific problems were proposed at this time or during the 18 months I spent in the field: I was expected to serve more as a reporter than as an anthropologist.

in order. Thereupon, I decided to concentrate on an attempt to become saturated with center attitudes and behavior and gain the confidence of many individuals of varied background, opinion, and age.

The first step in the new program consisted of undertaking simultaneously a number of sociological and anthropological studies which had no connection with the delicate center attitudes and very little connection with the kind of data I was employed to get. Women were interviewed on how they thought evacuation had altered their way of life. Parents were interviewed on what they thought evacuation had done to their children. Anyone at all was interviewed for information and attitudes on social stratification in Japan and the United States. The secretary of the co-operative was interviewed on the proposed co-op educational program. Officers of the community council were interviewed on what they thought about juvenile delinquency. A serious study of Japanese was undertaken with a Kibei instructor. Almost all these interviews were arranged by recommendation from one informant to another, except for officers of organizations who could be contacted directly.

These red-herring studies had many advantages besides the primary one of facilitating contacts. They provided an excellent opportunity to become familiar with the pattern of center life and made an entree which none but the most timorous of evacuees could refuse without discourtesy, presenting the field worker in the role of a conscientious scholar.³ They provided the opportunity for a return visit to discuss specific problems which appeared as the investigations were pursued. They gave the informant a reasonable story to tell too curious neighbors.

Such a device would not have been nearly

³ Industry proved to be an excellent policy in the centers, for, to some extent, it alleviated suspicion, since one of the traits commonly associated with Caucasian spies was that they were believed to dawdle about with no legitimate occupation. Industry, per se, also commanded considerable respect among the Japanese.

so effective in a normal modern community as it was in the centers. The extreme monotony of life there, the scarcity of recreation, and the fact that few evacuees were ever too busy to begrudge the time for a chat provided an ideal background. Even employed evacuees rarely had anything to do in the evening but sit and talk. Consequently, though some informants were probably not deceived by this ruse, once it became apparent that I was not going to ask embarrassing questions, my visits were usually not resented.

Nevertheless, a great many of the first interviews were quite fruitless. Some evacuees did not attempt to hide their opinion that I was a dissembling spy. Others were more courteous but confined their comments to the questions pertinent to the interview or to long stories of the painful experiences of themselves, their friends, or the Japanese in general. I persevered, however, and, in time, some of the persons who had served me as informants invited me to call as a friend.

When some of these friendly individuals began to be of marked assistance, giving a considerable amount of their time, I offered to pay them for their trouble. These offers were politely refused. (Part of this refusal to accept payment sprang from the fact that it would have stigmatized the informant as an *inu*, that is, a stool-pigeon.) Even the language teacher refused payment in a manner which implied that the suggestion was in poor taste. One did not accept payment for talking with a friend. This produced a situation—created by informants—which comprised an extremely important step in the eventual development of rapport. Informants had indicated that they were willing to accept me as a friend or friendly acquaintance but not as an employer. The value of the role hinged upon the fact that it put me under an un verbalized obligation. My informants and I knew that I was getting information on the strength of a personal relationship. I had no means of recompensing them except by returning their friendship and accepting the obligations it implied, the

most important of which was observing the complicated taboos of the in-group. I had much to learn, however, before I was able to convince a sufficient number of informants that I was worthy of this quasi-membership in the in-group.

This information was slowly gained through assuming the role of a willing learner. Instead of giving information to a stranger who dominated a somewhat formal interview, friendly informants now began to instruct me in some of the less confidential aspects of center life and attitudes and in the rudiments of Japanese etiquette. Issei men were most inclined to do this. My inferior age and sex status made them less hesitant to give instruction and offer advice. At first this instruction consisted chiefly of dissertations on Japanese customs and other matters on which the younger generation was ill-informed. The respectful attention with which this instruction was received often put informants in a mellow mood, and they might proceed to informative, though still cautious, discussions of current center attitudes or even to advice on gaining rapport. This advice was often given by implication. An informant might remark that the Japanese were "very suspicious people." One quoted a Japanese proverb: "Look upon every stranger as if he were a thief," thereby implying that if I expected to get information I must exercise great caution and patience.

One of the most valuable pieces of advice given by informants was the repeated statement, "Japanese seldom lie; but they will seldom tell you anything to your face." More sophisticated informants expressed the same idea by remarking that Japanese prefer to make significant statements briefly and by implication. They added that if the hearer has the intelligence to understand the suggestion, well and good; if not, it is his misfortune. I took this advice to heart but was not able to apply it capably until I had learned far more about the center attitudes, simply because I often could not recognize the hints when they were given to me.

The inferior role of learner did not cause

me to lose status as a scholar, for, as I learned from informants, the ideal Japanese scholar is a modest and self-effacing individual. Moreover, the task of "understanding the Japanese" was one which every Japanese believed ought to be approached with modesty by an ignorant Caucasian, even if he had a university degree.

In any case, skill at small talk and gossip and at making the proper responses to a recital of grievances was acquired. Some familiarity with the tabooed subjects was gained and some skill at showing no curiosity about them. For instance, it was discovered that an academic interest in Japanese culture was flattering, but interest in what anyone thought of the Japanese emperor or the American government was taboo.

When I began to make modest progress in the Japanese language, informants drilled me in simple phrases and were delighted when I remembered their instructions.⁴ While my exuberant and energetic personality prohibited any attempt to conform to the ideal standards of Japanese female behavior, my forceful remarks were not resented if they were covered with a veneer of self-depreciation. It was good manners to apologize for anything which conceivably merited an apology. Gradually, familiarity was gained with the cynical and ironic humor which permeated the camp and concerned itself chiefly with the hardships of center life and sarcasms aimed at staff members or Japanese who were disliked. With practice it was possible to take an active part in such byplay.

It should be stressed that the salient advantage of the role of learner did not lie so much in the semiformal instruction given but rather in the increased intimacy of the

⁴ It was not imperative that I learn Japanese, since most evacuees could carry on a fluent conversation in English if they chose to do so. On rare occasions I used an interpreter. On the whole, however, I found it more practical to use Issei informants who spoke some English. If they were ashamed of their English, I demonstrated my difficulties in attempting to learn Japanese, which almost invariably put them at ease.

relationship and the informants' largely unconscious imparting of attitudes. Both the red-herring studies and the role of learner gave an additional advantage: they provided for many evacuees the only opportunity for conversing with a Caucasian on equal terms or with the Caucasian in the quasi-subordinate position of learner. Many of their contacts with the WRA staff were on the basis of employer to employee or of the Japanese as petitioner and the Caucasian as the potential donor or withholder. It was, therefore, a rare treat for an evacuee to be approached as an authority, capable of instructing a "stuck-up Caucasian."

As I proceeded with the three-faceted role of conscientious investigator of innocuous scholarly subjects, willing learner, and half-accepted friend, I was able to gain skill at the most fundamental technique of all—alleviating their suspicion. The primary importance of this task had, of course, been appreciated from the beginning of the study. It was obvious, for instance, that part of the technique consisted of avoiding tabooed topics, at least until relations were well established. However, it was extremely difficult to acquire a thorough knowledge of these taboos without an intimate knowledge of center life. Moreover, there were subtle gradations in taboos, depending on closeness of intimacy. In an early interview, hinting that one understood the significance of certain taboos was poor policy: it might be interpreted as an effort to extract information on a delicate topic. As the acquaintanceship deepened, informants began to take for granted that I had some familiarity with touchy matters. In time, I could discuss some of them openly and give no offense. Certain subjects, however, could never be introduced. Attempting to identify any Japanese whom an informant was discussing was always taboo and was associated with a governmental investigator.

I was just beginning to conclude that I had developed considerable skill in alleviating suspicion when I committed what was probably the crudest error in almost two years of field work. A friendly informant re-

marked during an interview that one could always tell when Japanese were under emotional strain "because they get so quiet."

A few weeks later I visited a family with whom extremely good rapport had been developed. A group of intimate friends of the family also called. The conversation turned to the possible fate of those evacuees who had recently been sent to Tule Lake. One of the visitors remarked that he had just received a letter which was very informative. I was extremely curious as to what might be going on in the newly established center for "disloyal" Japanese. Relying on my rapport with the hosts, I remarked that I would like to see the letter. The silence that fell on the chatting group was almost palpable, and the embarrassment of the hosts was painful to see. The *faux pas* was not that of asking to see a letter, for letters were passed about rather freely. It rested on the fact that one did not give a Caucasian a letter in which the "disloyal" statement of a friend might be expressed. The enormity of the error was increased by the fact that the hosts were my closest Japanese friends, had treated me with candor and affection, and had assisted me greatly. Now I repaid these favors by embarrassing them before Japanese friends and giving the impression that they were consorting with a spying Caucasian. I immediately apologized for attempting to pry into private correspondence and the tension eased. Fortunately, the hosts trusted me sufficiently to interpret the incident correctly.

After this experience I redoubled my efforts to control my enthusiasm and to approach delicate matters obliquely, if at all. The error, in fact, was eventually turned to good account. It was found that a delicate topic might be approached by degrees. If a hint of the silent tension manifested itself, the subject could be dropped. The overwhelming importance of patience was re-emphasized. Until *all* informants present trusted me, I could show no curiosity on those matters about which data were often most urgently required. Indeed, while rapport was being built up, it was good policy

to show no special interest if an informant began to skirt a delicate topic. Weeks later, he might bring it up again and give more details. It took months, for instance, to learn who some of the alleged *inu* were. Information was eventually gained from a woman who had been expressing unusual hostility toward the administration. Instead of exhibiting shock or curiosity, I reminded the informant of the "ears in the walls." The informant, encouraged by this atypical reaction from a Caucasian, continued with fury: "That Omura, Miura, and Mohri! They are the worst ones. Before I'm going out [of the Center] I'm going to see Mohri. I'm going to tell him, 'You just wait till the war's over. Then we'll see that you get yours.'"

In the Gila center at least half-a-dozen visits extending over several months were usually required to impress a potential informant with the fact that I was dissociated from the administration and could be trusted not to report anti-administration or pro-Japanese statements. In fact, the alleviation of suspicion was a never finished task calling for constant alertness, no matter how intimate rapport might be. No informant ever forgot that he was talking to a Caucasian—a person whose country was at war with Japan.

The program of becoming familiar with center attitudes was continued for almost five months, during which much information was collected but little of the kind of material desired by the head of the study. My employer wanted long and detailed verbatim statements of evacuee attitudes—especially their attitudes toward current events. I was doggedly submitting my red-herring studies and describing the attempts I was making to reach the point where I could get the kind of information I was supposed to get. As the letters from my superior grew increasingly critical, I grew increasingly stubborn. I knew I was not doing a capable job, but in my more optimistic moments I hoped I was making progress.

Then, in early December, 1943, the camp was shaken by an event of the type on which

the study particularly desired data. An adolescent Nisei boy, who was later diagnosed as schizophrenic by his physician, had for many months been attempting to obtain permission to leave camp and continue his education. He had answered the Military Questionnaire in the negative and, like many other Nisei, had later decided to change his answer to affirmative. He was given a hearing before the Project Attorney, a man with a singular lack of insight into the evacuee's behavior and its motivations. The boy explained that he had become angry over the evacuation and the abrogation of his rights as a citizen and, moved by illogical emotion, had answered in the negative. The attorney insisted that if the boy had had the sentiments of a loyal American, he would have taken legal means to right the injustice done him. The boy attempted to explain that his emotional state, his ignorance of law, and his loss of faith in the intentions of America precluded such calm behavior. The attorney dismissed this explanation as irrelevant, held that the boy was disloyal to the United States and a liar, and recommended that he continue to be classed as disloyal. The boy did not know of this negative recommendation, although he probably suspected it. Months passed. His parents prepared to relocate, although their son's status was still undetermined. The young man brooded on his predicament and became more and more irritable and depressed. One evening, he went to the Army-patrolled camp entrance, paid no attention to the sentry's challenge, walked out of the gate, and was shot.⁵ In a few hours the news spread, and the camp buzzed with excitement.

I immediately made a round of visits. To my surprise little effort was needed to obtain data on the rapid development of attitudes. Acquaintances or friends, individually or in family groups, did not censor their excited comments. With new informants such as the

⁵ The wound was not serious. The young man made a rapid recovery and was allowed to relocate, the attorney's recommendation being overruled in Washington.

injured boy's physician, his pastor, and his neighbors, no introduction was needed. Detailed opinions and explanations were freely offered; some informants did not even conceal the fact that a certain group of hot-heads were counseling a meeting "to make something of the shooting." The officers of the community council appeared glad to explain their plans for controlling the situation and for presenting the administration with certain requests which, if acceded to, would quiet the excited residents. In short, it was possible for the first time to prepare a detailed, accurate, and well-balanced report, which presented a comprehensive picture of the dynamics of an event and the attitudes it produced.

Just as I was finishing this report I received a letter curtly ordering me to abandon my time-wasting interest in the Japanese language and in quaint Japanese customs and to report what was going on. Under the circumstances, I could do little but laugh. I wrote in reply that I would not defend my strange field techniques in detail but would allow the inclosed report to justify them. Needless to say, my techniques were not criticized again.

The success in reporting this incident served as convincing evidence that the techniques pursued in the five preceding months had been effective and revived my dwindling self-confidence. In fact, the crisis appeared to crystallize my attitudes toward the field situation and those of the residents toward me. I found that my hesitant techniques and precautions had fallen into a coherent pattern. Consequently, I now could proceed boldly where previously I had crept step by step. As for my informants, it is possible that up to this time many of them had not been conscious of the fact that they trusted me. Previously they had given me casual assistance. Now, for the first time, my need for data was specific and obvious. My informants were forced to make a choice. Either they must cut me off coldly, or they must accept me in a closer intimacy. Almost all chose the latter course. Some began to regard me as a friend who looked to them for

clarification and interpretation of current issues and came to expect that I would call on them for enlightenment. This was an ideal situation, for this was exactly the type of data the study required.

From this point forward it was relatively easy to keep informed on the salient political and social developments. All that was required was to maintain the existing rapport, pick up the issues as they came along, ask questions about them if attitudes permitted or, if they did not, merely keep my ears open. Rapport was so well established in some families that an important issue was bound to be brought up and discussed before me almost as if I myself were an evacuee.

ANALYSIS

This document provides an unusually detailed description of the initial and perhaps the most important period of field work—the period during which the student changes from a novice to a professional. It is, in fact, a naïve and forthright depiction of the complex process of redefinition of the self, a blow-by-blow account of how a student changed from a baffled creature who did not know what she was to a competent and "self-confident" individual whose role had been validated and reinforced by "significant others." In addition, the student quite unconsciously betrays the fact that she changed her informants' conception of their role, that she taught unresponsive or hostile individuals how to be good informants.

The document indicates that this process may be divided into three chronological stages: insecurity, gradual definition, and validation of role.

The student attempted to define her role in general terms before she entered the community. "No role was possible but that of observer of sociological phenomena." Much of her "discouragement, bewilderment, and sense of failure" sprang from the fact that she was trying to operate in a relatively unstructured situation. She literally did not know what her role entailed. Even the phrase she coined to define her role, "observer of

sociological phenomena," was confused jargon. She had never seen anyone play the role and had never heard it adequately described. The term was equally meaningless to her potential informants. They did not know what she was, and, even if they had, they would still have been obliged to learn how to behave in the presence of this strange human specimen.

Her discomfort was increased by the fact that her potential informants had a series of crippling roles ready and waiting for her. To them she was at best a well-meaning but ignorant pest in whose presence one had to keep a strict watch on one's tongue. At worst she was a clever and hypocritical spy for the administration, trying, by kindness, to lead them to betray themselves or their fellows.

If her own image of herself had been clear and well defined, if she had been reasonably sure that she was capable of playing the role of a competent investigator, she might have found this situation irritating, but not demoralizing. Indeed, many months later, when she took up residence in the "disloyal" Tule Lake Center, she was not at all disturbed when informants regarded her as a spy. But during this period of initiation she found herself trying to defend what to her was an ideal rather than a real self-image against the community's already well-defined derogatory appraisals. Desperately she tried to see herself as "a person to whom one gave information," while everyone about her regarded her as "a person to whom one gave no information."

Harry Stack Sullivan remarks that "when anything spectacular happens that is not welcome to the self, not sympathetic to the self dynamism, anxiety appears, almost as if anxiety finally became the instrument by which the self maintained its isolation within the personality."⁶ The anxiety which this student suffered in trying to defend the self of which she approved was so agonizing that she was unable to describe it

adequately in her document. She spent days alternately crying or writing letters to relatives and academic friends. Then she refused to associate with the Caucasian staff members, hoping that this irrational rudeness would make her more acceptable to the Japanese. She repulsed the overtures of a few kindly and insightful staff members who tried to draw her out of her self-imposed solitary confinement, interpreting their offers of friendship as "attempts to steal her data." (Paradoxically, she had no data worth stealing.) Finally, she succumbed to an urge to eat enormously and in three months gained thirty pounds.

The acute nature of these symptoms suggests that she was struggling to cling not only to the role of a competent field worker but to the central and most essential parts of her personality. If a trusted person had asked her whether she were doing good work, she would have admitted that she was not. But she dared not admit it to herself.

Curiously, the student consciously defined the process of role definition as a one-sided phenomenon. She saw herself as initiating red-herring studies so that she might gradually slide into the role of learner and accept instruction in the complicated art of "understanding the Japanese."

But her document clearly indicates that she taught her informants as energetically as she learned from them. With dogged persistence, she showed them how a sociological investigator behaved, and she demonstrated again and again that she was sincere, reliable, and trustworthy. Simultaneously, she trained them in their role of informants, let them know what kind of information she wanted, and, with their help, developed devices which enabled them to communicate it.

It was through this complex reciprocal interaction that the roles of "sociological investigator" and of "informant" became defined. The student learned to adopt the refinements of behavior which enabled her to perceive what she was becoming and to communicate this image more efficiently to informants. And, thanks to their assistance,

⁶ *Conceptions of Modern Psychiatry* (Washington, D.C.: William Alanson White Psychiatric Foundation, 1947), p. 10.

she was no longer the inept, bumbling self she had been. The amorphous ideal role was taking on a concrete social substance.

Conversely, the informants learned that she was "catching on," that she often knew more about juicy items of gossip than they did, and that, above all, she did not "blab to the administration." Thereupon, many of them took the trouble to learn what kind of information she wanted. Indeed, some eventually became so well trained that they attended secret meetings and made voluminous mental notes of rumors and expressions of public opinion so that they might give her long accounts which they expected her to take down verbatim.

Perhaps the most interesting phenomenon revealed by this autobiographical document is the fact that the evacuee community accepted the student's definition of her role before she accepted it herself. If she had been thoroughly convinced that she was a competent investigator, she would not have been surprised when, after the shooting of the evacuee boy, informants, both old and new, poured out more information than she could take down. For the first time she was presented with irrefutable evidence that she was what she wanted to be. Her role was being validated and reinforced by the society in which she lived. Whether she liked it or not, she now was "the sociological investigator."

It is significant that from this point the tone of the document undergoes a radical change. The field worker's activities seem to take place in another dimension. As she herself remarks: "I now could proceed boldly where previously I had crept step by step." In the most salient meaning of the popular phrase, she was now sure of herself.

This analysis suggests that the fundamental process involved in the initiation into full-scale field experience does not differ significantly from the process of learning any new role. The neophyte passes through an initial period of anxiety and distress, during which he and the persons with whom he interacts contribute to the definition of both his and their roles. If both parties are able

to agree on these definitions and are able to communicate the fact that they agree, much of the anxiety disappears and is replaced by the complex phenomena which we call "self-confidence."

In this case the first stage in the process is depicted with unusual clarity, for few mitigating or blurring conditions were present. The field worker was cut off from many aspects of her former life and self. She literally did not know what she was and had only the vaguest notion of what she ought to do. If she and her informants had not been able to create a structure in which they were both able to operate, the result, at least for the field worker, might have been catastrophic.

The practical implications are obvious. The experienced professional, bulwarked by the approving appraisals of his colleagues, may undertake a long period of solitary field work in a hostile and un-co-operative community and suffer no extraordinary psychological discomfort. But the neophyte, whose status is on trial, ought to avoid so extreme an ordeal. He should be accompanied or visited frequently by someone who can reinforce the approved image of his developing self.

In this more fortunate situation, he will still suffer the anxiety which, if Sullivan and others are correct, accompanies all situations in which the self is ill-defined. But he will probably suffer less if he realizes that his symptoms are an instrument of self-defense rather than an indication that he is bound for a mental hospital. Moreover, he may take comfort in the fact that a large part of his "cure" lies in work, or, if he prefers, in play. He must learn to play his own role and the role of his informants, and he may be obliged to teach them both to his informants.

Explicitly or implicitly, most discussions of field methodology have pictured the field worker in the role of learner. The role of teacher has received relatively little emphasis. Similarly, many social scientists, particularly anthropologists, state that they were significantly changed by their first field experience. Only rarely will they admit that

they changed the subjects of their investigation.

Perhaps these two neglected aspects of field work are related. The social scientist who attempts to maintain the fiction that a human being can interact with or communicate with another human being without changing him is understandably reluctant to admit that he transformed his informants into teachers.

Obviously, one cannot play the role of learner if the informant does not know how to play the role of instructor. And if the informant does not know how to play this role, no one but the field worker will teach him.

It should not be overlooked that this process involved an emotional as well as an intellectual communication. Understandably, the evacuees did not like Caucasian Americans, and, like mistreated and unloved children, they were not willing to adopt a new role or learn new skills to please a person whom they regarded with hostility and distrust. Most of them had to become aware that the student liked and respected them before they would take the trouble to become competent and co-operative informants.

On the other hand, while emphasizing the importance of friendship, the student should have noted that many people who liked her did not become particularly skilful informants and that some people who disliked and perhaps even despised her became excellent sources. Among the latter were the relatively intelligent but arrogant pro-Japanese agitators in the Tule Lake Center, who believed that they could carry on long conversations with a Caucasian female during which she would learn nothing and they would learn a great deal.

Here, however, the field worker was able to step into a situation which had been structured for centuries. Because of long and arduous training in proper Japanese behavior, she was able to participate with a fair amount of skill in a favorite form of Japanese gamesmanship—the art of communicating by means of cryptic and oblique implication. When these would-be experts in intrigue discovered that she could play this game, they could not resist the challenge.

The unstated, yet fundamental, rule of this game characterizes many highly formalized competitive situations such as the interaction between skilled diplomats or between two opposing lawyers. It is, perhaps, even more clearly manifested in such games as chess or bridge: that is to say, one need not respect a person's general role in order to respect his segmental role. So long as a player is playing, his status depends on his skill. Moreover, this type of conversational gymnastics is one of the most admired attributes of a geisha. And in the dismal and tension-ridden Tule Lake Center there were no geishas. The astute reader will perceive that the unfortunate agitators were beaten before they started.⁷

Finally, an extended and repeated contest between a skilful professional (the agitator) and an apt and observant amateur (the field worker) can have only one outcome. The amateur improves by reason of the fact that he has placed the professional in the role of teacher.

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⁷ There were a number of other reasons why the agitators proved to be excellent informants; for a fuller discussion see R. H. Wax, "Reciprocity as a Field Technique," *Human Organization*, XI, No. 3 (1952), 36-37.

THE HYPOTHESIS OF SLOW CYCLICAL VARIATION OF CREATIVITY

S. S. WEST

ABSTRACT

The hypothesis of the existence of cycles of creativity is scrutinized in the light of data available in the literature. Dates of the rise of sculpture yield a cycle length of 319 years; the numbers of known active scientists in Arabic and classical Greek cultures yield a cycle length of 164 years. The rate of growth of the American Physical Society parallels the current cycle, extrapolated, of the latter periodicity, a maximum being predicted at about A.D. 1980-90.

The vague concept of cyclic change in history—the rise, flowering, decay, and fall of local civilizations—has long been felt to be a useful means of integrating historical knowledge. It first began to take quantitative form with the publication in 1912 of Sir Flinders Petrie's essay,¹ and has more recently been given support by the work of A. L. Kroeber² and others. In 1935 Sorokin and Merton³ examined the varying intensity of Arabic science from A.D. 700 to A.D. 1300 with the aid of George Sarton's compilation⁴ of known scientists, but this exploratory effort has not been extended subsequently.

Admittedly, the available data are fragmentary, but this seems a poor reason for neglecting them altogether. It is also true that inaccessible value-judgments have influenced the recording and transmission of information about creative individuals and their works, but the uncertainty thus introduced is probably, at worst, not more serious than that which is tolerated in studies of relations between attitudes and their resultant social expression. Moreover, creativity which is not recognized and utilized by the culture in which it occurs is not of

great importance from the viewpoint of sociological theory. If it is recognized, it will tend to be recorded and hence transmitted. This paper will present some inferences drawn from available data and the manner in which they might be used for long-term sociological prediction.

THE RISE OF SCULPTURE

Petrie estimated the approximate date at which sculpture passed from a crude or "archaic" to a reasonably finished stage for each creative era of the Egyptian, Greek, Roman, and medieval phases of Western civilization, and C. E. K. Mees⁵ has rectified these dates with the assistance of more recent archeological evidence. Mees places a "rise of sculpture" at approximately 3200, 2800, 2100, 1500, and 500 B.C. and A.D. 70 and 1300.

The shortest interval between successive epochs in this series is 400 years. If we assume that each interval approximates an integral multiple of some period of time which is the same for all the intervals (that is, a cycle), then deviations of estimates of the length of this period for the several intervals are minimized by assigning to the respective intervals 1, 2, 2, 3, 2, and 4 cycles. The six intervals then yield an average period of cyclic fluctuation of about 330 years. (It is supposed that epochs not recorded lie below the threshold of historical perception.)

¹ W. M. Flinders Petrie, *The Revolutions of Civilization* (New York: Harper & Bros., 1912; New York: Peter Smith, 1941).

² A. L. Kroeber, *Configurations of Culture Growth* (Berkeley: University of California Press, 1944).

³ P. A. Sorokin and R. K. Merton, "The Course of Arabian Intellectual Development, 700-1300 A.D.—Study in Method," *Isis*, XXII (1935), 516-24.

⁴ George Sarton, *An Introduction to the History of Science* (Baltimore: Williams & Wilkins Co., 1927-48).

⁵ C. E. K. Mees and J. R. Baker, *The Path of Science* (New York: John Wiley & Sons, 1946), pp. 186-88.

Let us now designate the date in years A.D. by y and the number of cycles (counting from an arbitrary zero at 3200 B.C.) by x , and fit by least squares a relation $y = a + bx$, in which a and b are constants. We find $a = -3,117$ years and $b = 319.2$ years, with a correlation coefficient $r = 0.9995$. The standard deviation of observed from predicted dates is $\sigma = 47.2$ years.

By itself this result does not tell a great deal, but its significance is enhanced considerably when it is compared with certain independent information to be examined in the next section. At this point it may be noted, however, that epochs of artistic creativity are predicted at about A.D. 1670 and 1990. There is no reason to demand that these represent sculpture rather than the graphic arts, and it may be that the earlier unrecorded epochs were represented by works that could not survive the effects of time.

GREEK AND ARABIC CREATIVITY IN SCIENCE

Enumerable events of scientific activity include (a) the existence of a man engaged in producing new knowledge of a specified kind and (b) the discovery of a novel relationship between phenomena or concepts. Although the latter type of event is useful when reasonably complete historical records are available (as for the last two or three centuries),⁶ the former can be defined more simply and identified more easily. Sorokin and Merton studied the development of Arabic science by counting men known to have contributed significantly to each field and concluded that their data "reflect the actual movements in so far as these are accurately described by the qualitative judgments of the historian."

Their data consisted of numbers of scientists listed by George Sarton as having contributed during a given half-century to mathematics, physics, chemistry, natural

history, and humanistics. Number of men per half-century was plotted as a function of time (a) unweighted, (b) with weighting according to the importance of the individual on a three-point scale, and (c) with weighting on a fifteen-point scale, and smooth curves were drawn by eye through the points. The curves given by Sorokin and Merton show an apparent cyclic variation which is especially clear for mathematics and humanistics and differ little between weighted and unweighted cases.

Sorokin and Merton's tabulation indicates that mathematics was the most active field in Arabic science (with 114 men known), followed by medicine (89 men), and then by physics and chemistry (25 and 21 men) at about half the intensity of medicine. The unweighted curve for mathematics shows regular oscillations (with a period of about 160 years) about a declining trend. The first two of these cycles cover roughly the duration of the Abassid caliphate (A.D. 750-1058), when intellectual development was focused in the eastern part of Islam. During the following two cycles the focus was in the west, predominantly in Spain.

To obtain comparable material from another period, the method of Sorokin and Merton has been applied to Greek scientists, with Sarton as source. Three categories were used: mathematics, medicine, and a category designated "physical science" which included physicists, all engineers who produced significant innovations in mechanics, all geographers who contributed to geology or geophysics, and the few chemists who could be identified. Polymaths who contributed to two or (rarely) to all three fields were tabulated in each, following Sorokin and Merton. Mathematics was found to be represented by 46 men, medicine by 38, and physical science by 71. The number of men who contributed during each half-century to the chief scientific field of each culture is given in Table 1.

In the case of Arabic mathematics, the mean number of men is 10.36 per 50-year interval. Chi-square for deviations from the mean is 21.6, which yields (for 10 degrees of

⁶ See, for example, T. J. Rainoff, "Wavelike Fluctuations of Creative Productivity in the Development of West-European Physics in the 18th and 19th Centuries," *Isis*, XII (1929), 287-307.

freedom) a probability of 0.019 that deviations of the same or greater magnitude could occur by chance. The null hypothesis of constant level of creative activity can therefore be rejected at a 2 per cent level of confidence. In the case of Greek physical science, the mean is 6.46 men per 50 years, chi-square is 7.24, and $P(\chi^2) = 0.70$, so that the null hypothesis cannot be rejected on the

TABLE 1
DISTRIBUTION OF KNOWN GREEK AND ARABIC SCIENTISTS

Years (B.C.)	No. of Men Active in Greek Physical Science	Years (A.D.)	No. of Men Active in Arabic Mathematics
600-550.....	5	750-800...	6
550-500.....	7	800-850...	22
500-450.....	8	850-900...	13
450-400.....	4	900-950...	7
400-350.....	6	950-1000...	15
350-300.....	11	1000-1050...	11
300-250.....	6	1050-1100...	6
250-200.....	3	1100-1150...	10
200-150.....	8	1150-1200...	10
150-100.....	7	1200-1250...	5
100-50.....	6	1250-1300...	9
Total.....	71	Total.....	114

TABLE 2
REGULARITY OF VARIATION IN FIGURE 1 AS
DEMONSTRATED BY FITTING REGRESSION
LINE TO DATES OF MAXIMA AND MINIMA

ORDER OF MAXI- MUM	DATE OF MAXIMUM OR MINIMUM Observed	Predicted	DEVI- ATION (YEARS)
0...	487 B.C.	488 B.C.	+1
0.5...	405 B.C.	406 B.C.	+1
1.0...	325 B.C.	324 B.C.	-1
1.5...	240 B.C.	242 B.C.	+2
2.0...	167 B.C.	160 B.C.	-7
8.0... A.D.	830	A.D. 825	+5
8.5... A.D.	912	A.D. 907	+5
9.0... A.D.	985	A.D. 989	-4
9.5... A.D.	1075	A.D. 1071	+4
10.0... A.D.	1150	A.D. 1153	-3
10.5... A.D.	1230	A.D. 1235	-5

basis of this test. However, the coefficient of variation is 0.43 in the former case and 0.32 in the latter, which are sufficiently similar values to suggest a possibility that the lack of significance in the Greek case results merely from its lower mean rather than from

the absence of systematic variation. Let us proceed as though this were true.

With the assumption that the observed number of men is roughly proportional to the corresponding number of active scientists of the given kind in the given 50-year interval, number of men was plotted as a

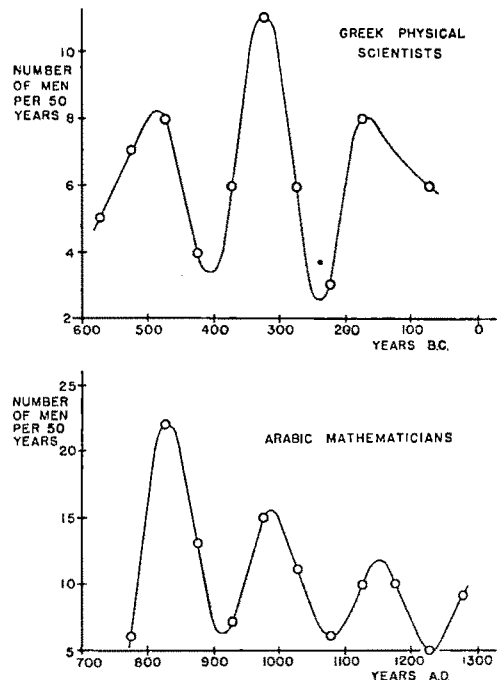


FIG. 1.—Variation of frequency of known active scientists in Greek and Arabic cultures. Observed values are represented by circles. The curves were drawn through these points in such a way as to make each maximum or minimum as symmetrical as possible.

function of date at the mid-point of each interval, and a smooth curve was drawn by eye through each set of points, in such a way as to make each maximum or minimum as symmetrical as possible. From these curves, dates of maxima and minima were estimated to have the values given in Table 2. The curves are shown in Figure 1.

The slope of a regression line of estimated year of maximum or minimum on order of maximum was found to be 161.0 years for the Greeks and 160.2 for the Arabs. These

figures suggest that variation in scientific activity in the Greek and Arabic cultures, which occupied overlapping geographic areas, may have been a continuous periodic process. Accordingly, the lapse of time between the last Greek and the first Arabic maximum was assigned the nearest integral number (6) of 160-year cycles, and a regression line was fitted to all eleven dates of estimated maxima and minima of both cultures. The slope of this line, which is equal to the length of the cycle, was found to be 164.1 years, and its predicted dates of maximum or minimum are given in Table 2. Standard deviation of observed from predicted date was 4.0 years.

GREEK AND ARABIC DATA

It is known that Roman and Hellenistic science of the period between the two cultures from which our data are derived drew information from the one culture and passed it on to the other. Moreover, the European and European-American scientific development which followed was based to a high degree upon the work of the previous two thousand years. One may therefore postulate a continuous periodicity with cycles of the same length (164.1 years) as found above. Between and beyond the two groups of observations, the regression line predicts maxima at 4, 168, 332, 496, 661, 1317, 1481, 1645, 1809, and 1973 A.D., with minima at 570, 78, and 86 B.C. and A.D. 250, 414, 578, 743, 1399, 1565, 1727, 1891, and 2055.

Thorough discussion of these epochs requires an intimate knowledge of classical and medieval history, but some interesting coincidences may be mentioned. Minima appear to be associated with some degree of general social disorganization: The predicted minimum at 78 B.C. coincides with the death of Sulla, thus marking roughly the beginning of the period of administrative revisions and civil wars by which the Roman republic was terminated *de facto*. The minimum at A.D. 250 falls in the period of military anarchy,⁷ which was the most sharply defined crisis of the Roman Empire. Other

coincidences can be found in the history of science itself: The maximum at 1645 is well recognized as a time of high creativity, the foundations of modern physics having been laid in the generation just preceding and the generation just following it. (Galileo died and Newton was born in 1642.) The minimum at 1727 falls in a period when previous advances were being consolidated and little new work was being done. The minimum at 1891 lies near the beginning of the series of experimental and theoretical discoveries which form the basis of present-day physics.

The over-all lengths of the Greek and Arabic periods of scientific development suggest a tendency for every fourth minimum to be lower than intervening minima, yielding a great cycle of 656 years, which would place such major minima at 570 B.C. and A.D. 86, 743, 1399, and 2055. If this tenuous inference is valid, it indicates that the final decline of the current phase of creativity in science lies only a generation or so in the future. This is reasonable, for the year 1980 approximates the end of the creative life of persons whose scientific training was completed before 1940, when the replacement of individual research by group research for engineering purposes was sharply accelerated. It may also be noted that the major minimum predicted near A.D. 2055 is analogous to those which were contemporaneous with the change in content of Roman and Hellenistic thought with the influx of oriental religions in the late first century and with the breakdown of Mohammedan civilization in Spain under the impact of resurgent Spanish military power.

GROWTH OF THE AMERICAN PHYSICAL SOCIETY

Since physical science is the field of investigation most prominent in the American

⁷ S. A. Cook, F. E. Adcock, M. P. Charlesworth, and N. H. Baynes (eds.), "The Imperial Crisis and Recovery," *Cambridge Ancient History*, XII (New York: Macmillan Co., 1939), chaps. vi and vii, pp. 165-281.

culture, one may ask whether its recent growth provides any support for the extrapolation from antiquity. Membership in the American Physical Society was chosen as an index of activity, the available tabulation⁸ extending from 1899 to 1951, by single years except in the earlier portion of the range. Since the first ten years of the society's life showed a type of growth different from that occurring subsequently (perhaps because of the time required for its existence to become generally known), only data for 1909 and following years were used, forty points being thus available to determine a relationship of number of members to time. The absolute number of members was used, although there may be reason to consider their fraction of the total adult population of the United States as being perhaps more meaningful.

Since the present accelerating growth of the society obviously cannot continue indefinitely, being limited by a maximum membership which is some unknown fraction of the number of adults in the country, the rate of growth must ultimately diminish toward zero. A simple representation of such a process is given by the logistic curve $N = B/(C + e^{-At})$, where N = number of members and t = time in years, which has been extensively applied to the growth of total populations. After making a rough preliminary estimate of the parameters, the method of Schultz⁹ was applied twice, and a final adjustment was made of C alone. At this stage of approximation, the parameters were found to have the following values: $A = 0.0664$; $B = 272$; $C = 0.0020$. The sum of the squares of *fractional deviations* was minimized rather than the sum of the squares of absolute deviations, in order to avoid weighting the values of N corresponding to more recent years.

At this point the variance of fractional

deviation was $0.5110/40 = 0.01278$, and an oscillation having a period of 28 years was evident in the residuals. When this was removed, the second residuals were found to vary with a period of 9.3 years at half the amplitude of the longer period. When each of these harmonics was introduced as a cosine, their phases were zero and π , respectively, relative to the point of initiation of the society. The final approximation of the growth of the American Physical Society was thus found to be

$$N = \left[1 + 0.1280 \right. \\ \times \left(\cos 2\pi \frac{t}{28} - \frac{1}{2} \cos 2\pi \frac{3t}{28} \right) \Bigg] \\ \times \frac{272}{0.0020 + e^{-0.0664t}},$$

where $t = 0$ at A.D. 1898. The final residual variance of fractional deviation was 0.001002, corresponding to $\sigma = 3.17$ per cent of the value predicted by the foregoing formula. The maximum deviation was 6.3 per cent.

The upper asymptote of N is given by $B/C = 136,000$, a quite reasonable figure. The inflection point occurs at $t_0 = (-\ln C)/A = 93$ years, corresponding to A.D. 1991. The point symmetrical to the initiation of the society is at A.D. 2084; hence organization of the society began about 7 years after the predicted minimum of scientific activity, and its rate of growth should be greatest (on the assumption of logistic growth) about 18 years after the predicted maximum. Insofar as one can trust a fit to so small a portion of the logistic, these results verify the prediction of the current cycle of scientific activity. Discussion of the superposed harmonic variation of N is beyond the scope of this paper.

DISCUSSION

The inadequacy of available data appears to prevent a more satisfactory direct test of a simple intuitive hypothesis stating merely

⁸ *Bulletin of the American Physical Society*, XXVI, No. 9 (1951), 15.

⁹ Henry Schultz, "The Standard Error of a Forecast from a Curve," *Journal of the American Statistical Association*, XXV (1930), 139-85.

that there exists a periodic fluctuation of creative activity. Critical test requires a more sophisticated hypothesis, based upon postulated causal factors. One such factor commonly invoked by social scientists is climate.

Huntington¹⁰ argued for the influence of climate upon social interaction, but his exhibits of quantitative data are not as convincing as one might wish. D'Arcy Thompson¹¹ presented a graph showing width of tree-rings, smoothed by a 100-year running average, as a function of time. His graph shows a period of about 150 years, with maxima of tree-ring growth at A.D. 1570 and 1720 and minima at A.D. 1480, 1620, and 1775. These maxima occur at +5 and -7 years, respectively, from predicted minima of scientific activity based on the Greek and Arabic data and the minima at -1, -25, and -34 years from predicted maxima, thus suggesting that a cooler climate may be conducive to creativity. Closer agreement would not be very likely, the trees being in North America and the scientists in Europe.

Since climate depends upon output of energy from the sun, one might hope for information from studies of sunspot number, which is used as an index of this output in predicting radio reception. Only the 11-year period in sunspot variation has a large amplitude, although a 78-year period has been recognized¹² for a long time, its maxima having occurred in 1778, 1860, and about 1947. If alternate peaks of this variation were to have opposite effects, it would constitute a variation of 156-year period, leading the predicted variation in scientific activity by about 30 years and synchronous with Thompson's last two minima of tree-ring growth. C. N. Anderson¹³ claims to

¹⁰ Ellsworth Huntington, *Mainsprings of Civilization* (New York: John Wiley & Sons, 1945).

¹¹ D'Arcy Thompson, *On Growth and Form* (New York: Macmillan Co., 1945), p. 240, Fig. 74.

¹² G. P. Kuiper (ed.), *The Sun* (Chicago: University of Chicago Press, 1953), pp. 333-35.

¹³ C. N. Anderson, "A Representation of the Sunspot Cycle," *Bell System Technical Journal*, XVIII (1939), 292-99.

have identified a 312-year period in sunspot data extending back to 1610 but does not state the phrase of this harmonic. On the whole, data concerning climate and associated phenomena, although suggestive, are of doubtful utility in interpreting variation of creativity.

There is also a possibility that varying kinds and degrees of social interaction during growth of a population from one relatively stable size to another can produce during certain periods of the growth process more personalities of the kind required for scientific creativity. If one considers twenty readily available population logistics¹⁴ (for Austria, Belgium, Denmark, England and Wales, Formosa, France, Germany before 1855, Germany after 1855, Hungary, India, Italy, Japan, Java, Norway, Philippine Islands, Scotland, Servia, Sweden, the United States and the world), one finds the following values:

Median time from 1 to 99 per cent of growth = 320 years
 Median date of 1 per cent of growth = A.D. 1740
 Median date of inflection point = A.D. 1905
 Median date of 99 per cent of growth = A.D. 2065

These values are very close to the length and dates of predicted minima of the extrapolated cyclical variation of scientific creativity. It may be, therefore, that we are dealing, after all, with a world-wide process of which creativity and population growth are merely more obvious aspects.

The type of research procedure which seems most likely to prove fruitful is that which has been used in connection with the "macrodynamic" theory of business cycles proposed by M. Kalecki.¹⁵ In Kalecki's theory the economic system is regarded as a

¹⁴ Raymond Pearl, *Studies in Human Biology* (Baltimore: Williams & Wilkins Co., 1924); Kingsley Davis, *The Population of India and Pakistan* (Princeton: Princeton University Press, 1951); Sung-Ken Quo, "The Population Growth of Formosa," *Human Biology*, XXII (1950), 293-301.

¹⁵ M. Kalecki, "A Macrodynamic Theory of Business Cycles," *Econometrica*, III (1935), 327-44.

resonant dynamic structure, the energy for whose oscillations is drawn from erratic shocks of impressed force but whose period of oscillation is internally determined. Smith and Erdley¹⁶ studied the rate of flow of investment capital by means of an electrical analogue of Kalecki's model of an economic system, finding that the system was unstable and oscillated with a 10-year period, as had been predicted from analysis of economic time-series.

What appears to be needed, therefore, is a set of sociological postulates which are precise enough individually and comprehensive enough as a set to form the basis for an electrical analogue of the Smith-Erdley type. If these can be found in such a form as to reproduce a period approximating 164 years, then hypotheses might be deduced from them which could be tested in ways more congenial to sociologists. In an economy which depends upon expanding technology, the possibility of predicting variations of scientific creativity several generations in advance of occurrence makes such an effort well worth while.

UNIVERSITY OF MICHIGAN

COMMENT

A. L. KROEBER

West's paper rests on the Flinders Petrie assumption of 1911 that higher cultural productivity changes in cycles—he calls them "revolutions" and pictures them as such in an ascending spiral in his final diagram. West, in operating with equally spaced maxima and minima, accepts at least wave-like recurrent oscillations. Both also are ready to shift over considerable geographical areas to follow their cycles through—from Egypt to Greece to Rome to the West. This is a second assumption which almost all historians and most sociologists will hesitate to make.

Toynbee, the only professional historian

who has seriously and systematically compared civilizations, has chosen a different problem in the field, namely, the internal comparability of phenomenal events and their causes—the structural history—of civilizations regarded as individuated units. He does not assume either cycles recurrent in time or notable shifts in space. Neither have I assumed or dealt with recurrent cycles. On the other hand, Ligeti (*Weg aus dem Chaos* [1931]) and Curt Sachs (*Commonwealth of Art* [1946]) do find and document them in European culture history of the last thousand years and, interestingly, of wave lengths of about 140 and 35 years, respectively—a ratio of 4 to 1. West's cycle lengths of 164 years for science and 319 for sculpture exceed by little, being simple multiples of Ligeti's and Sachs's durations.

What is important is that each investigator be conscious of his assumptions, so that they may be treated as working hypotheses. Others can then test them out if the original investigator is prevented by enthusiasm, bias, dogmatism, or ignorance from testing them fully. With all the widespread interest in the nature of civilization, it is remarkable how little follow-up testing there has been. The prime value of West's paper is that he does extend and test the assumptions of Flinders Petrie and of Sorokin and Merton. Petrie was sometimes incredibly highhanded, but he was also a man of unusual insight, as in recognizing the value of emergence from archaism as a critical point of comparability in the arts. To dismiss Petrie summarily would be equally highhanded.

While I am personally rather skeptical about cultural phenomena occurring cyclically to any considerable degree, I agree that more specific work should be done on this basis—if only to lay the ghost of the theory if it is wholly imaginary or, on the other hand, to determine when, where, and how far it applies, so far as it may prove to be real. Shrugging dubious problems off may be convenient but does not help the progress of understanding.

Some interesting special situations eventuate from West's work. Thus technology

¹⁶ O. J. M. Smith and H. F. Erdley, "An Electronic Analogue for an Economic System," *Electrical Engineering*, LXXI (1952), 362-66.

obviously is one of the components of civilization that tends to be strongly accumulative. Given sufficient time, it even accumulates in disregard of spatial limitation, in contrast to the arts and religions. As a creative endeavor, the history of pure science behaves much like the history of the arts, in rising intermittently and dying away. But creative science is also closely related to technology, whose higher levels are applied science. So we are apparently compelled to infer that the creative bursts of science—be they rhythmic and cyclic or not—occur on the surface of a cumulatively growing body of permanently retained scientific-technological knowledge. In art and religion, on the contrary, there is little evidence of any underlying accumulating mass.

West's assumption that Greek, Roman, Arabic, and Western science may be a continuous historic movement—as most historians of science largely take for granted—leads him to compute extrapolated maxima and minima of creativity on the basis of his 164-year cycle. Most of these extrapolations cannot be validated or challenged because of falling in eras in which scientific creativity was subliminal. But three of the recent ones are interesting. The minimum of 1565 falls in the Copernican-Galilean interval; 1727, in the middle of a remission which I put at 1700–1750 (*Configurations* [1944], p. 169); and 1891 pretty well represents the point where classical physical science (including knowledge but not explanation of the periodic law) had crystallized and the beginnings of nuclear physics were impending, also when the initial Darwinian impulse had definitely lost impetus but Mendel and genetics had not been rediscovered. These three seem to be periods when the current approaches were yielding diminishing returns and a "reconstitution" was necessary before new creative approaches could be developed.

My own method has been to try to determine substantively where such points of reconstitution and florescence, respectively, fall, without being too much influenced by considerations of possible repetitive regu-

larity. Yet when West by projective computation fixes times of trough and culmination just about where my laborious empirical analysis set them, it may be only coincidence, but I am impressed.

When it comes to explanation of given dates, however, there is likely to be more confidence in descriptive analyses of the situation of science itself at a given moment than in coincidences of earlier minima with the death of Sulla or the military breakdown in the Roman Empire around 250. The latter might be used to explain the subliminal status of scientific creativity in Europe for many centuries following; it seems too massive a cause to adduce for a 164-year-recurrent minimum falling in A.D. 250.

In regard to the growth of the American Physical Society, I cannot follow the mathematical demonstration, but it seems a gratuitous elaboration to point out that the society was founded 7 years after the 1891 trough of creative productivity and will reach its culmination in 1991, 18 years after the peak of our present cycle in 1973. The turn of the century just was a period in which it was fashionable to found national societies for the sciences—the sociologists organized in 1905, the anthropologists in 1902. This may have been mainly a function of enough professorships having been established to provide a sufficient body of members for a series of societies. Or any of a dozen other near-at-hand, specifically related, temporary, and non-recurring factors may have been at work. It seems sound method to exhaust these smaller, pertinent factors before falling back on a rhythm that has been pulsating for millennia. True, the rhythm may *also* have been at work. But if a rhythm that was operative in Greece in 599 B.C. contributed to the foundation of a society in America in 1899, it must have produced hundreds of other effects in the intervening centuries and countries, and a presentation of these would be more significant than the particular belated effect on the American Physical Society.

Most problems of sociology and anthropology may fairly be described as limited

and diurnal. In contrast, the problem of civilization and cycle is millennial and immensely broad and ramified, with few scholars working it, and those casually. There is no integrated profession devoted to the study of civilizations, few avenues of publication open for papers and still fewer for books, unless they emphasize emotional aspects like crisis, dissolution, or doom. Every sincere contribution that is more than half-baked ought accordingly to be welcome. But it ought also to be subjected to critical analysis.

Civilizations are among the major phenomena confronting our intellects as civilized beings. They come and go fitfully, they arise and flourish and dissolve, they are discontinuous in place and duration, but there are also immense continuities among them. The precise nature of both the continuity and the discontinuity is far from clear. Apparently, the combination of these two qualities projected the idea of cycle into the concept of civilization. I incline to think that

the notion of rhythmic, repetitive regularity is mainly, if not wholly, erroneous and unfortunate. If so, it should be got rid of; but, to eliminate it, it must be met, analyzed, and adjudged. Experience to date is that it cannot be disposed of by ostracism, avoidance, or hostility. Until we control more knowledge, we cannot assert that cyclic quality in civilizations is wholly illusory. There may be an element of it that is deep-seated and well-buried. More probable are rhythms, relatively brief, of generational or multiple length, and internally characteristic of particular civilizations rather than fundamentally pervasive of all civilization—surface modulations on their non-rhythmic or crudely rhythmic mass. Our total intellectual experience to date renders it unlikely that there is an all-or-none answer to the problem of cycle in civilization. But how shall we attain an answer at all except by resolutely facing the facts?

UNIVERSITY OF CALIFORNIA
BERKELEY

THE LEISURE ACTIVITIES OF THE MIDDLE-AGED

ROBERT J. HAVIGHURST¹

ABSTRACT

This is a study of the psychological significance of leisure activities to men and women in the age range forty to seventy. A set of 19 "significance variables" is defined and used as bases for rating scales in analyzing the two favorite leisure activities of each member of the sample of adults for the Kansas City Study of Adult Life. In addition, their content is classified in eleven categories. Age, sex, and social status are more closely related to content of leisure than they are to significance. On the other hand, personal adjustment and social mobility are more closely related to the significance of leisure than to its content.

Most of the research on leisure activities has aimed at learning about the content. There have been a number of studies of what people do in their leisure time and how much time they spend on it, while, on the other hand, the significance of leisure has been relatively neglected.

It has been established that the content of leisure activities is closely related to sociological variables or group categories, such as sex, age, and social class. But the significance of leisure may not be so directly related to the variables. Even though what a person does in his leisure may be limited and to some extent defined for him by his sociological characteristics, its significance may be more closely related to his psychological characteristics. For instance, the fact that working-class people enjoy fishing while the upper middle class is more likely to enjoy golf or volley ball or sailing may tend to obscure the fact that these activities have much the same significance—they are a welcome change from work, they give one a chance to be with friends, they provide sheer enjoyment, they require mild physical activity, and they fit in well with the expectations held by other people. In other words, most of the working-class people who enjoy fishing do so for the same reasons as do most of the upper-middle-class people who enjoy golf and sailing.

The purpose of this study was to explore the interrelations of two ways of analyzing leisure activities—content and significance—and to relate them to age, sex, social class, and personality characteristics.² "Significance" was a general term applied to a set of nineteen variables, listed below, which are characteristics of leisure activity, whether consciously or not. They were all judged to be personally or socially significant and therefore worth studying. The study aimed at answering the following questions: (1) Does the significance of leisure activities as measured by objective ratings vary with age, sex, and social class? (2) Does the significance of leisure activities vary with variables in personality? (3) Does the significance of leisure activities vary with the content of these activities?

Two general propositions are to be tested by the answers to these questions: (1) the significance of leisure activities is more closely related to personality than to age, sex, and social class; and (2) several different leisure activities can have the same significance: people of different age, sex, and social class can derive similar values from their leisure, even though the content of the activities is different.

PROCEDURE

A stratified random sample of men and women aged forty to seventy were inter-

¹ The writer acknowledges the assistance of Conrad Curtis, Hallowell Pope, and Ruth Granatz in the analysis of data, and of the following people who rated the interviews: Betty Orr, Marjorie Ravitts, Conrad Curtis, Hallowell Pope, and John Smothers.

² A third way of analyzing leisure activities, also used in this study and to be reported separately, was to ask people about their reason or motives for their favorite activities.

viewed as part of the Kansas City Study of Adult Life.³ For this part of the study, a 2-hour interview,⁴ known as the "Social Role Interview," was held with 124 women and 110 men, distributed by socioeconomic status as follows: approximately equal numbers of the upper middle class, lower middle class, upper-lower class, and about half as many lower-lower-class people, defined by an "Index of Status Characteristics" of the Warner type.⁵

The purpose of the Social Role Interview was to get information on everyday activities and their significance to the individual. Approximately a quarter of the interview dealt with the leisure activities of the respondent: he was asked to describe the three or four he liked most and to say with whom he did them and why he enjoyed them. The interview went on to explore the respondent's activity and attitudes in a variety of common pursuits, such as sports, radio, television, movies, reading, activities around the home and while on vacation. Other parts of the interview dealt with the everyday life of the respondent as parent, spouse, homemaker, worker, citizen, friend, church member, and association member. This gave a good background against which the significance of one's leisure could be judged, and something like this is essential in any attempt to assess the qualities of a leisure activity.

In this kind of study a different method must be used from that used in a study of content. For the study of content of leisure it is useful to ask people not only what they do but also how often. This was done by Clarke,⁶ who found a relation between fre-

quency of participation and occupational prestige for most of the leisure activities on his list. But for a study of significance of leisure it is necessary to go beyond questions of sheer quantity. A variety of questions must be asked about the quality of leisure activities, what they mean to the individual, what place they have in his life. For this purpose it is useful to ask people to talk about their favorite leisure activities. Practically all the respondents described at least two favorite activities. Consequently, the analysis of significance was made primarily on the basis of these two.

THE SIGNIFICANCE VARIABLES

A set of *significance* variables was defined and applied by means of rating scales to the interviews. Thus the ratings on significance were objective in being judgments made by disinterested people who had read a variety of interviews and were in a position to compare one person's leisure activities with those of others. It was important that the entire interview be read as a basis for the significance ratings, so that the relation of leisure to work and other aspects of life could be seen and so that whatever one said about his leisure could be noted. Often a person said important things about his leisure when talking about his friends, his family, his clubs, or his church, as well as when talking explicitly about the subject.

Defining the significance variables.—The significance variables were derived from three sources: (a) the discursive writings of contemporary sociologists on the phenomenon of leisure in modern society and its significance;⁷ (b) cogitation about leisure and its relation to work, especially in the middle and later years of life; and (c) a number of the interviews with Kansas City people and the more or less obvious leads given by them

³ In the original sample as drawn, approximately 25 per cent of the people declined to be interviewed, and approximately 20 per cent moved away, died, or could not be located for an interview. Therefore, some caution should be used in generalizing from this particular study group to a larger population.

⁴ For the interview schedule see Robert J. Havighurst, "The Social Competence of Middle-aged People," *Genetic Psychology Monographs* (in Press).

⁵ W. Lloyd Warner, Marchia L. Meeker, and Kenneth Eells, *Social Class in America* (Chicago: Science Research Associates, 1949).

⁶ Alfred C. Clarke, "Leisure and Levels of Occupational Prestige," *American Sociological Review*, XXI (June, 1956), 301-7.

⁷ For example, David Riesman and Warner Bloomberg, Jr., "Leisure and Work: Fusion or Polarity?" in W. Allison Chalmers (ed.), *Industrial Relations Research Association Annual* (1956)

as to their leisure and its meaning to them.

The writer looked for dimensions by which leisure activity might be analyzed and which seemed to possess sociological or psychological interest. The significance variables eventually were named and identified by capital letters and used as a basis for the five-point rating scales. One set of significance variables was applied to each of the two specific leisure activities most preferred by the respondent. The second set of significance variables was applied generally to the entire leisure life of the individual, as far as it could be seen through the interview. The two sets appear below.

RATING SCALES FOR SIGNIFICANCE "VARIABLES"

Applicable to specific activities. These are five-point scales, with definitions of each point on the scale.

A. Autonomy versus other-directed

(1) Chooses activity with purpose and regard for its function in one's personal life; (2) acts on own initiative, without much reflection on the function of the activity; (3) activity represents a weak but visible choice; (4) chooses activity but is pushed toward it by circumstances of his position; activity related to work; (5) activity determined by others or by propinquity.

B. Creativity (new solution, novel behavior)

(1) Produces new and interesting results (material, non-material, makes things, arranges things, does things with and through people); (2) there is an element of novelty, though not to a high degree; alters approach to activity occasionally; (3) some evidence of variety through choice; behavior not completely stereotyped; (4) relies on habit, with occasional innovations; (5) rote activity (TV several hours a day, routine newspaper reading).

C. Enjoyment versus time-killer

(1) Gets strong pleasure out of activity; (2) gets mild pleasure out of activity; (3) enjoys it slightly but sees it mainly as a time-filler; (4) enjoyable partly because it allows escape from unpleasantness; (5) time-killer, no enjoyment.

* No judgment as to desirability is to be inferred by position on the scales.

D. Development of talent

(1) Activity develops or maintains outstanding talent; (2) activity develops or maintains positive and fairly complex ability; (3) activity develops or maintains a fairly complex ability at a mediocre level; (4) activity requires a perceptible amount of complex ability; (5) activity requires no complex ability.

E. Possibility for future versus limited to present

(1) Activity promises to be more rewarding in the future; (2) activity will grow slightly in reward value; (3) activity will continue to be rewarding; (4) activity will continue but will lose reward value; (5) activity must soon be abandoned and will have no valuable effect in the future.

F. Instrumental versus expressive

Instrumental: Manipulation of the object world to meet human needs.

Expressive: Expressing emotion as an end in itself. Showing affection, appreciation, enjoyment. Supportive, acceptable behavior.

(1) Makes or produces something for a useful purpose or for pleasure of others; production is essential in process; may or may not be pleasurable; (2) plays a game or participates in an activity for some goal beyond the game or activity (church committee, philanthropic board); (3) does something for a goal beyond the activity but gets a great deal of pleasure out of it also; balance between product and process; (4) does something for pleasure of self, with an element of pleasure for others or of use for others; (5) does something for the sheer pleasure of it or associates with others in a pleasure-seeking activity; focus is on pleasure for self.

G. Physical energy input

(1) Maximum physical activity; physical activity is of the essence; (2) physical activity is considerable; (3) active but limited; (4) physically passive but requires effort to participate (e.g., going to baseball game); (5) physically passive, purely spectator, at home.

H. Relation of leisure to work: complementary-competitive

(1) Recreation grows out of work and makes work more successful, e.g., reading for a scholar; (2) recreation helps in

career but also enjoyable in itself, e.g., play golf or cards for the sake of "contacts"; (3) fits in with expectation of general occupational status—appropriate for a person with this kind of job; (4) *no relation* between recreation and work; non-congruent and has no function by being non-congruent; (5) recreation offsets work—a real relief and contrast; requires a shift of roles from the work-roles, e.g., going fishing to be alone.

J. Gregarious versus solitary

(1) Essentially gregarious; (2) mainly gregarious but occasionally alone; (3) balanced, e.g., swimming, movies; (4) mainly solitary, occasionally involves social interaction; (5) essentially solitary.

K. Service versus pleasure

(1) Primarily to serve others; (2) service to others with an element of self-development or pleasure; (3) primarily to develop one's self or do something useful for one's self; (4) pleasure, with an element of self-development; (5) primarily for personal pleasure.

L. Status, prestige

(1) Builds up general social prestige; (2) brings prestige in present, immediate status group; (3) has mild prestige value; (4) has no prestige value; (5) reduces general prestige.

M. Relaxation from anxiety-arousing tensions

(1) Provides no relaxation; (2) provides a trace of relaxation; (3) partial escape from tension of life but not a complete letdown; (4) somewhat relaxing; minimal tension; (5) gives complete relaxation from tension, e.g., minister who goes fishing.

N. Ego-integration versus role-diffusion

(1) Activity develops the individual as an integrated person; (2) helps one to maintain one's level of integration or life-style; (3) fits neatly into established life-style; congruent with life-style; (4) represents a mild discontinuity in life-style; (5) definitely disintegrative; does not fit the person's life-style.

General rating scales for nature and use of leisure

I. Financial cost

(1) A great deal, e.g., golf at a private country club, \$500+; (2) costs some-

thing but not much, \$100-\$500, e.g., golf at a municipal course, television; (3) costs little or nothing, less than \$100, e.g., playing cards without gambling, fishing (locally).

II. Feeling of much versus little leisure

(1) A lot of leisure, almost too much; (2) plenty of spare time—a feeling of relaxation—can play whenever I like; (3) some leisure—taken as matter of course, would prefer more; (4) "I have to fight for leisure time"; very little spare time; (5) no spare time at all.

III. New experience versus repetition

(1) Always looking for new experience in leisure; tends to drop an activity after a while because it loses its novelty; (2) has done at least one new thing in leisure time recently; (3) feels no need for novelty but can accept it; has at least one habitual activity; (4) slightly resistive to novelty; (5) likes to do things over and over; has a routine for leisure, and enjoys it, e.g., long-time gardener or card player.

IV. Vitality versus apathy

(1) Vigorous, active, pleasure-seeking in use of leisure; (2) different from 1 in having lesser degree of vigor (or related to fewer activities); (3) finds leisure agreeable but not exciting; (4) feels that leisure is uninteresting; (5) apathetic—expects and gets nothing from leisure.

V. Expansion versus constriction of interests and activities

(1) Leisure pattern shows expansion of interests and activities, high level; (2) shows active interest or vigor in things he does, but no expansiveness; (3) leisure pattern shows plateau of interests and activities; (4) imminent decrease but still seems on plateau; (5) decreasing range of interests in leisure.

VI. Things versus other values

(1) Predominant interest in things versus social relations or other non-material values; is definitely interested in handling or working with objects; (2) more interest in things than non-material values, may have perfunctory interest in television, watching sports, or playing games, along with an active interest in working with things; (3) balance of interests between things and non-material values; has at least one strong interest

in making, collecting, or cultivating material objects; (4) more interest in non-material than in material things, that is, more interest in social relations, music, reading, than in working with things; (5) no interest in material objects or in working with materials.

Reliability of the significance ratings.—

The rating scales for the significance variables were defined and discussed by the raters and were tried out on several interviews with mutual discussion. After that the ratings were done independently, by a total of six different people, four men and two women. Every interview was read and rated by two people, and the ratings were averaged.

The percentage of complete agreement between two raters on a single variable ranges from 28 to 93, with an average of 56 per cent complete agreement. The percentage of agreement within one step on a five-point scale ranges from 70 to 100, with an average of 89. There were no systematic differences between raters in the levels of their ratings. With this degree of agreement between raters, the average of two ratings has a substantial degree of reliability. Agreement between raters was greatest on variables E, G, F, K, N, and I and lowest on variables M, B, A, D, and VI.

Significance variables in relation to content.

—In rating the individual on the significance of his two favorite leisure activities, the judges read the interview and attempted to discover the significance for that particular individual. For instance, suppose the respondent (a woman) said that one of her favorites was her informal bridge club. She told why she liked it and what it meant to her. The judge, after reading the whole interview, made a rating of its significance in her life. On variable A, "Autonomy versus other-directed," 53 people had to be rated for participation in an informal social group. The judges rated 19 of these at 1 or 1.5 (average of two judges) on the A scale, 26 at 2 or 2.5, and 8 at 3–5. Thus some people were judged to have chosen this activity with purpose and regard for its function in

their personal lives, while others were judged to have taken it up because directed into it by others or because of mere propinquity.

For some types of activity there was less spread of significance rating. TV, for instance, was mentioned by 40 people as one of their two favorites. The judges rated all these people at 3–5 on the instrumental-expressive scale. Even of this relatively stereotyped activity, there was a considerable variation of significance ratings.

PERSONALITY VARIABLES

There were four psychological variables on which data were available and which might be related to significance ratings. These will be called "personality variables," though the fourth—social mobility rating—is less related to personality than the other three. They are:

Personal adjustment—a rating made by a judge reading the whole interview.

Attitude score—a self-report rating scale on personal adjustment, filled out by the respondent at the close of the interview.

Manifest complexity—a rating made by judges on reading the entire interview, based on complexity of personal relationships, complexity of activities and interests, present and past, and complexity of imaginative and emotional experience as indicated by reading, artistic activity, and planning for the future.

Social mobility rating—a rating on a five-point scale of social mobility during the lifetime of the respondent, from strong upward mobility, to stability and on down to strong downward mobility.

These ratings were made by a different set of judges at a different time than when the significance ratings were made.

CONTENT CATEGORIES

This study employed rather broad categories of content or type of leisure activity. The two favorite activities of the respondents were listed and placed in the following eleven categories as to content:

1. Participation in formal groups—social clubs, fraternal organizations, church

- groups, discussion groups, or adult education classes
2. Participation in informal groups—sociability stressed, and no rules or organization, no fixed meeting times, etc.—such groups are card-playing cliques, neighborhood groups, tavern groups
 3. Travel for the sake of enjoying travel
 4. Participation in sports
 5. Watching sports (not on TV)
 6. Television and radio
 7. Fishing and hunting (mainly fishing)
 8. Gardening—flowers or vegetables or landscaping
 9. Manual-manipulative activities—sewing and handwork for women, do-it-yourself carpentry and home repairs, woodworking, etc., for men

sion to personality variables, the frequencies of the ratings for autonomy of the two favorite activities were tabulated in relation to scores for manifest complexity, personal adjustment, attitude score, and social mobility rating. It was found that there was a relation, significant at the .01 level, between autonomy ratings and manifest complexity ratings, high being associated with high, and low with low. Similarly, high autonomy was associated at the .01 level of confidence with personal adjustment ratings and with personal attitude scores. Finally, upward social mobility was related at the .07 level of confidence with high autonomy in leisure activities. On the other hand, the ratings on inter-

TABLE 1*
LEISURE, PERSONALITY, AND SOCIAL VARIABLES

	SOCIAL			PERSONALITY			
	Sex	Age	Social Class	Personal Adjustment	Manifest Complexity	Social Mobility	CONTENT
Content.	+++	++	+++	+	++	+
Significance variables. . .	+	+	++	+++	+++	++	+++

* + = a small degree of relationship (only two or three of the content or significance variables are reliably related to a social or personal variable). ++ = a fair degree of relationship. +++ = a high degree of relationship (more than half of the leisure variables are related reliably to a social or personal variable).

† Estimated.

10. Imaginative—almost entirely reading but might include music and art-appreciation activity
11. Visiting relatives and friends—travel may be involved, but the main object is visiting

The results are summarized in Table 1, which shows the interrelationships of the significance, content, personality, and social variables.

SIGNIFICANCE VARIABLES RELATED TO PERSONALITY

The two favorite leisure activities were tabulated in relation to the personality variables and the social variables, and the resulting frequency tables were analyzed by means of chi-square to test the null hypothesis that the relation between the significance variables and the other variables might be chance relations. Results are shown in Table 2. For example, in studying the relations of the autonomy versus other-directed dimen-

est in things versus interest in non-material values were not related to any of the personality variables at the .10 level or lower.

Wherever the null hypothesis was confirmed, the table reports a "high" probability that any relations shown in the data are due to chance. It will be seen that most of the significance variables are related to complexity, personal adjustment, and personal attitudes scores, with a statistical significance at the 1 per cent level. Seven of the significance variables are related to social mobility, with probabilities ranging from .01 to .08.

Ratings on manifest complexity of the individual are closely associated with all but one of the significance variables. Some of these significance variables contribute directly to the complexity score, such as creativity, talent development, and new experience; but most of the significance variables do not seem to be involved in a definition of complexity of life. Rather, they tell some-

thing about the complex person—that his leisure activities are autonomous, creative, instrumental rather than expressive, gregarious, service- rather than pleasure-oriented, although he gets a high degree of pleasure from them, and that his leisure promotes ego-integration.

The adjustment ratings and the personal adjustment scores from the attitude inventory show generally relations similar to the

no relaxation in it. This gives a picture of a striving, energetic personality who is behaving at leisure much as a mobile person behaves at work and other areas of life.

SIGNIFICANCE VARIABLES RELATED TO AGE, SEX, AND SOCIAL CLASS

In studying the relations of the significance variables to age, sex, and social class, a chi-square test was also used. In general,

TABLE 2*
SIGNIFICANCE OF FAVORITE LEISURE ACTIVITIES IN RELATION TO PERSONALITY VARIABLES

VARIABLE	PROBABILITY THAT THE RELATIONSHIP IS DUE TO CHANCE			
	Complexity	Adjustment	Personal Attitudes	Social Mobility
Autonomy versus other-directed.....	.01	.01	.01	.07
Creativity.....	.01	.01	.01	High
Enjoyment versus time-killer.....	.01	.01	.01	.06
Develops talent.....	.01	.01	.01	.05
Possibility for future versus limited to present	.01	High	High	High
Instrumental versus expressive.....	.00	.01	High	.08
High physical energy input.....	.01	.01	.01	High
Relation of leisure to work.....	.01†	High	High	High
Gregarious versus solitary.....	.05	.01	.01	High
Service versus pleasure.....	.01	.05	High	High
Status and social prestige.....	.01	.01	.01	.05
Relaxation from tension versus no relaxation	High	High	.01‡	.01§
Ego-integration versus role-diffusion.....	.01	.01	.01	High
Financial cost.....	.01	.01	.01	High
Feeling of much versus little leisure.....	.05	High	High	High
New experience versus repetition in leisure..	.01	.01	.01	High
Vitality versus apathy.....	.01	.01	.01	High
Expansion versus constriction of interests and activities.....	.01	.01	.01	.01
Interest in things versus non-material values	High	High	High	High

* Relationship is between high degree of complexity, adjustment, personal attitudes, and upward mobility, with the first-named the significance variable, unless indicated otherwise by a footnote.

† High complexity is positively related to the two ends of the scale—leisure as complementary to work and leisure as a relief or contrast from work.

‡ High personal attitudes score is positively related to the two ends of the scale—"provides complete relaxation" and "provides no relaxation from tension."

§ Upward social mobility is positively related to "no relaxation."

|| Highly complex people take leisure as a matter of course, those low in complexity claim they have no spare time.

significance variables. People with the higher adjustment ratings and personal attitude scores tend in their leisure to be autonomous, creative, active, gregarious, to seek new experience, and to find ego-integration.

The mobile people show a few interesting relationships between social mobility and significance of leisure. Upward-mobile people tend to be autonomous, instrumental rather than expressive, service-oriented, at the same time enjoying their leisure, and finding

the results indicated a moderate degree of relationship with social class and a low degree of relationship with age and sex. The following positive results were obtained.

Autonomous versus other-directed.—There is a slight trend for the upper-lower and lower-lower classes to be rated as less autonomous than the middle classes. No age or sex differences.

Creativity.—Class differences are obvious and supported by a chi-square significant at

the .01 level. The two middle classes are rated at the most creative end of the scale, while the lower-lower class is best characterized by a cluster of ratings at point 4, "relies on habit, with occasional innovations." No age or sex differences.

Development of talent.—There is a regular trend downward from the "talent" end of the scale as social class moves from upper-middle to lower-lower.

There is no age trend, but there is a reliable sex difference ($P = .01$), with men receiving more ratings around point 2, "activity develops or maintains a positive and fairly complex ability." Women are rated more frequently at points 3 and 5 of the scale.

Enjoyment versus time-killer.—There is a class difference significant at the .01 level. The upper-middle-class ratings are largely at points 1 and 2, "gets strong pleasure" and "gets mild pleasure out of activity." Lower-class ratings spread from point 2 toward point 3, "enjoys it slightly but sees it mainly as a time-filler."

Instrumental versus expressive.—No sex or class differences, but an age trend which approaches significance at the .05 level. The forty to fifty age group clusters toward the expressive end of the scale, while the sixty to seventy group clusters toward the instrumental end.

Physical energy input.—No age or class differences, but an obvious difference between the sexes, with men clustered at the "active" end of the scale.

Relation of leisure to work.—The only reliable difference exists between the sexes. Women are usually rated at 2 or 3 on the scale, which means that their recreation "fits in" with their housework but is seldom used as a relief or contrast from work. Men, on the other hand, are generally rated at 4 or 5 on the scale, indicating that their leisure is used as either a relief from or a contrast to their work or simply that it has no relation to it.

Service versus pleasure.—No age or class differences, but women have more ratings at

the "service" end of the scale than men do.

Status or prestige.—No age or sex differences. Upper-middle class clusters at point 2, "brings prestige in present, immediate status group." Upper-lower and lower-lower cluster at points 3 and 4, "has mild prestige value" and "has no prestige value."

Financial cost.—As would be expected, there is a clear-cut relation between social class and amount of money spent on leisure activities. There is a less clear relation between age and financial cost, with a chi-square significant at the .02 level, indicating that the forty to fifty group spends more money on leisure activities than do the older groups. No sex differences.

Feeling of much versus little leisure.—There is a sex difference ($P = .01$). Women feel that they have more leisure than men. The latter claim they would prefer more leisure than they have. No age or class differences.

New experience versus repetition.—There is a clear social class difference, with upper-middle and lower-middle clustered at points 2 and 3, indicating a certain degree of novelty; while lower-class people cluster at points 4 and 5, indicating a preference for habitual and repetitive activities. No age or sex differences.

Vitality versus apathy.—There is a class difference ($P = .05$), indicating slightly more vigorous, active, pleasure-seeking on the part of higher-status people. No age or sex differences.

Expansion versus constriction of interests.—There is an age trend ($P = .05$), with the fifty to sixty and sixty to seventy groups progressively decreasing their range of leisure interests. There is also a social class difference ($P = .05$), with those of higher status retaining their interest and vigor in leisure activities and those of lower status tending toward a constriction of interests. No sex differences.

Variables with no age, sex, or class differences.—Possibilities for future versus limited to present, gregarious versus solitary, relaxation from anxiety-arousing tension, ego

integration versus role-diffusion, and things versus other values.

SIGNIFICANCE RELATED TO CONTENT OF LEISURE ACTIVITIES

It is obvious that most of the significance variables must be related to content, since the content varies from such stereotyped activities as listening to the radio or playing cards to such creative activities as making ceramic ware or being program chairman of a club. In the statistical sense, content is related to significance ratings. When the distributions of significance ratings for various categories of content were examined, it was seen that each content category had its own peculiar distribution of ratings on most of the significance variables.

More revealing is the fact that there is, nevertheless, a considerable range of ratings on even the most stereotyped activities for most of the significance variables. For instance, although card playing and listening to the radio tend to be rated low on the scales of autonomy, creativity, development of talent, and energy input, yet in a few cases these activities received quite different ratings, as for one who is a tournament-player in cards or makes it a point to know and listen to the best musical programs on the radio or engages in a specialty of listening to football games on the radio. Every activity was evaluated and rated as it figured in the life of the individual who was being interviewed.

Thus it is necessary to make the following apparently contradictory statements: Significance variables are closely related to content of leisure activities; a given activity has a variety of significance ratings depending on the person; different leisure activities can have the same significance for certain people.

CONTENT OF LEISURE ACTIVITIES RELATED TO SOCIAL AND PERSONAL VARIABLES

When the content categories were studied in relation to the social and personal variables, it was seen that the relations were

quite different from those of significance variables related to social and personal variables. The content categories are more closely related to social class, sex, and age than they are to personal adjustment, social mobility, and manifest complexity.

Content of leisure and social class.—The characteristic upper-middle-class activities, when judged by the frequency with which they are mentioned as favorites are formal associations, participation in sports, and gardening. Lower-middle- and lower-class people tend to favor manual-manipulative activities and television. Fishing is a special favorite of lower-middle- and upper-lower-class people. Lower-lower-class people exceed other classes in their preference for "visiting friends and relatives" and for gardening. In the case of gardening, the lower-lower and the upper-middle groups express the same strong preferences, but the latter favor flower gardening and landscaping, while the former favor vegetable gardening. The upper-middle-class emphasis on formal associations is among women rather than men. The imaginative activities, mainly reading, are favored about equally by the two middle classes and the upper-lower, but the lower-lower class expresses little interest in it.

Sex and content of leisure.—The principal sex differences are the greater concentration of women on formal and informal associations and on reading, while the men exceed women in interest in sports, fishing, and gardening.

Age and content of leisure.—Age differences are found in five categories. Formal associations lose attractiveness as age changes from forty to seventy, though not among women until they reach the sixties. Informal groups are most attractive to men in the fifty to sixty group but are equally attractive at all ages for women. In participant sports there is a sharp falling-off for men in their sixties. On the other hand, people in their sixties favor the more solitary interest in gardening and the manual-manipulative activities.

Content and personal variables.—The relationships between content categories and personal adjustment scores and social mobility ratings were relatively weak. This means that people of quite different personal adjustment were about equally likely to favor a given type of activity, as compared with other types. Such relations as did appear were minor when compared with the relations between significance variables and personal adjustment. Only in one of the personal variables—manifest complexity—was there a fair degree of relationship to content. This would be expected, for some categories, such as television and watching sports, do not contribute to a high manifest complexity rating, while other leisure activities, such as reading and participation in groups, do.

The results support the two propositions which were stated at the beginning of this paper.

1. The significance of leisure activities is more closely related to personality than to the social variables of age, sex, and social class. Thus leisure activity is an aspect of personality. It is a response to personality needs, being one of the ways by which people express themselves. However, the significance variables are related to socioeconomic status to a fair degree, and socioeconomic status is itself correlated with personal adjustment and complexity of life. Thus there is a configuration of significance of leisure activity, personal adjustment, complexity of life-style, and socioeconomic status. Generally speaking, good personal adjustment, complex life-style, and higher socioeconomic status are correlated with the following characteristics of leisure activity: autonomy, creativity, enjoyment, prestige-giving, novelty, vitality, expansion of interests.

2. Several different leisure activities can have the same personal significance; therefore, people of different age, sex, and social class can derive similar values from their leisure, even though its content is different. For example, an upper-middle-class man in

his forties spends his leisure as follows: he attends a luncheon club once a week; he plays golf in season and perhaps handball; he spends a good deal of time on week ends looking after his lawn and caring for his shrubs and flowers. The upper-lower-class man of the same age goes fishing on week ends and on vacation; he works around the house a good deal, redecorating it and adding a new room at the back; he watches television several hours each evening and on week ends when he is not doing something else.

In rating these two men on the significance variables, we must study the interview with each to understand the significance of a particular leisure activity in his life. When their two favorite activities are analyzed in this way, their ratings may be compared. For these two men treated as hypothetical cases, it may turn out that their ratings on the significance variables are generally similar, with the following striking characteristics: They both rate high in their enjoyment of leisure. They both rate toward the expressive pole of the instrumental-expressive variable, that is, they both do things mainly for sheer pleasure rather than for some useful purpose or for the pleasure of others. They both rate rather high on relaxation, which means that their leisure is relaxing for them. Also their ratings are fairly high on ego integration, which means that leisure activity helps them to maintain their life-style and level of integration.

They rate toward the vitality end of the scale and also toward the expansion end of the variable, expansion versus constriction of interests and activities. The principal differences in significance ratings between them are that the upper-middle man gets a rating more toward the gregarious end; and that the upper-middle-class man has a feeling of not having enough leisure, while the upper-lower-class man feels that he has a comfortable amount of it.

Thus we see that, although its content differs, yet leisure has much the same significance for these two men. This is likely to

THE NATURAL HISTORY OF STANDARD METROPOLITAN AREAS¹

HENRY S. SHRYOCK, JR.

ABSTRACT

A widespread demand has grown up in the United States for statistics on large cities and their surrounding areas of high population density with which these central cities have a substantial amount of day-to-day contact. Since 1947, a federal interagency committee has been meeting the demand by defining Standard Metropolitan Areas. The general criteria used in determining which cities shall have S.M.A.'s and what surrounding areas shall be included should be rooted in a meaningful concept of the metropolitan area. The program has also had to take account of the interests of the local areas affected, many of which interests are non-statistical in nature.

Standard metropolitan areas (S.M.A.'s) have been adopted by the public in a fashion truly remarkable in view of the fact that they have been established for less than a decade. Their use far exceeds that of the metropolitan districts that were defined for the censuses from 1900 to 1940.

In the first place, a large amount of statistics has been published for individual S.M.A.'s by the Bureau of the Census from the 1950 censuses of population and housing, the 1947 Census of Manufactures, the 1948 Census of Business; and many tables are likewise being prepared from the 1954 economic censuses. These statistics have been widely used. Furthermore, the Bureau of Labor Statistics has used S.M.A.'s to present data on employment, wages, building permits, hours of work, etc.; the Bureau of Employment Security, to present data on labor markets; and other series have been shown for these areas by the National Office of Vital Statistics, the Social Security Administration, the Federal Reserve Board, the Federal Communications Commission, the Housing and Home Finance Agency, and the Business and Defense Services Administration.²

Partly because S.M.A.'s have assumed such wide importance, they have created

a certain amount of controversy. From some quarters there has been a demand for a greater variety of statistics for S.M.A.'s. From others there have been complaints that statistics for these artificial areas are not a substitute for statistics for cities or large counties. There have been arguments on what counties are to be included in a specific S.M.A., on whether S.M.A.'s are always best defined in terms of whole counties, and on the fundamental concept itself. Furthermore, there is often intense local interest in the S.M.A., which may take the form of seeking an S.M.A. for a particular city or seeking more territory for a certain S.M.A. or of seeking a separate S.M.A. for a city included in a larger city's. The major part of this interest and of the resulting local activity has little to do with the *statistics* provided for S.M.A.'s.

This article reviews the development of the S.M.A. program and the issues concerning specific criteria and broader matters of concept. Moreover, the reader would be given a very incomplete picture of the natural history of the S.M.A. if there were not some discussion of their *Realpolitik*—the interaction between the federal interagency committee and local and, to a lesser extent, national organizations.

Shortly after World War II the question was raised within the Bureau of the Census

¹ Adapted from a presidential address to the Population Association of America, Ann Arbor, Michigan, May 19, 1956. The views expressed in this paper are those of the author and not necessarily those of the Bureau of the Census or of the Federal Committee on Standard Metropolitan Areas.

² Robert W. Burgess, "Use of Geographic Regions in the United States," paper read at the XXIX session of the International Statistical Institute, Petropolis, Brazil, June, 1955.

as to whether the metropolitan districts of the Census of Population and Housing and the industrial areas of the Census of Manufactures were not sufficiently alike in general definition and function for the public to be better served if they were reduced to a common set of areas. An exploratory working group was appointed. In its first sessions, statisticians representing various subject-matter interests emphasized the unique features of each type of area and argued that general-purpose areas would not serve certain essential needs. Great credit must be given to Morris H. Hansen for his leadership in mediating the conceptual and technical differences at this stage. One argument upheld by the proponents of metropolitan districts was that whole counties provide too coarse a delimitation of metropolitan areas, an objection that the establishment at about this time of another new statistical area—the urbanized area—helped greatly to answer. The latter gave a meticulous definition of the physical boundaries of urban settlement, far better than could be achieved in terms of minor civil divisions—townships, etc.—of counties.

Later, other federal agencies participated in the proceedings. Foremost here was the Bureau of Employment Security, which was producing statistics for the "labor-market areas," which had been first developed by the U.S. Employment Service and its affiliated state employment services. They sometimes used whole counties and sometimes minor civil divisions as building blocks.

The Bureau of the Budget was invited to send an observer to the meetings. Soon the idea developed that all federal agencies producing statistics for some kind of metropolitan area should be encouraged to use the new set of general-purpose areas, which thus were christened "Standard Metropolitan Areas." The Bureau of the Budget then went on to organize the Federal Committee on Standard Metropolitan Areas, late in 1947, to which all interested federal agencies were invited to send representatives. In ad-

dition to those agencies already mentioned, these included the Bureau of Foreign and Domestic Commerce, the Business and Defense Services Administration, the Bureau of Labor Statistics, the Housing and Home Finance Agency, the Federal Reserve Board, and the Agricultural Marketing Service. The last-named is interested in the program chiefly because S.M.A.'s constitute key pieces in the over-all mosaic of state economic areas. The Bureau of the Budget has provided the chairmen of the committee, who have been successively Thomas J. Mills and Walter F. Ryan.

Meanwhile, the original working group has become the Technical Subcommittee of the Federal Committee. Here representatives of the agencies most vitally concerned have worked together to draft criteria and define specific S.M.A.'s to be approved by the full committee.³

One of the essential features of the procedure of setting up S.M.A.'s is a review of the proposed definition by informed local opinion. A representative of one of the participating federal agencies visits the area and talks with key people, such as city officials, officials of the chamber of commerce, newspapers, radio stations, appropriate research workers in universities, and so on. He may call them together for a meeting. The definition of the S.M.A. is not made public and does not become final until local review is completed. On the other hand, local opinion does not have the power of veto. At the present time, local interests are supposed to direct their attention to whether the proposed definition meets the criteria, and they are encouraged to bring forth any pertinent data that may have been overlooked. Local criticism frequently does not confine itself to these agenda, however.

For the 1950 Census, every city of 50,000 or more either had a S.M.A. of its own or was part of another city's S.M.A. There

³ An excellent history of the S.M.A. program in its earlier years is given by Robert C. Klove, in "The Definition of Standard Metropolitan Areas," *Economic Geography*, XXVIII (April, 1952), 95-104.

were 172 S.M.A.'s in all—168 in continental United States, 3 in Puerto Rico, and 1 in Hawaii.⁴

The S.M.A. is defined as follows:

Except in New England, a standard metropolitan area is a county or group of contiguous counties which contains at least one city of 50,000 inhabitants or more. In addition to the county, or counties, containing such a city, or cities, contiguous counties are included in a standard metropolitan area if according to certain criteria they are essentially metropolitan in character and socially and economically integrated with the central city.

The criteria of metropolitan character relate primarily to the character of the county as a place of work or as a home for concentrations of nonagricultural workers and their dependents. Specifically, these criteria are:

1. The county must (a) contain 10,000 non-agricultural workers, or (b) contain 10 per cent of the nonagricultural workers working in the standard metropolitan area, or (c) have at least one-half of its population residing in minor civil divisions with a population density of 150 or more per square mile and contiguous to the central city.

2. Nonagricultural workers must constitute at least two-thirds of the total number of employed persons of the county.

The criteria of integration relate primarily to the extent of economic and social communication between the outlying counties and the central county as indicated by such items as the following:

1. Fifteen per cent or more of the workers residing in the contiguous county work in the county containing the largest city in the standard metropolitan area, or

2. Twenty-five per cent or more of the persons working in the contiguous county reside in the county containing the largest city in the standard metropolitan area, or

3. The number of telephone calls per month

⁴ Since 1950, 6 new S.M.A.'s have been established: some are cities that have attained at least 50,000 inhabitants according to special censuses conducted by the Bureau of the Census, whereas the others had an annexation or a change in political status. The territory of 5 former S.M.A.'s was increased by the addition of another county. The name of one S.M.A. has been changed without change in territory.

to the county containing the largest city of the standard metropolitan area from the contiguous county is four or more times the number of subscribers in the contiguous county.

In New England, the city and town are administratively more important than the county, and data are compiled locally for such minor civil divisions. Here towns and cities were the units used in defining standard metropolitan areas, and most of the criteria set forth above could not be applied. In their place, a population density criterion of 150 persons or more per square mile, or 100 persons or more per square mile where strong integration was evident, has been used.⁵

A slightly different version is contained in a mimeographed announcement put out by the Bureau of the Budget.⁶

In essence, then, an S.M.A. starts out with a city of 50,000 or more. It includes the whole county containing that city. It also includes any contiguous county⁷ that meets the criteria of a metropolitan character and of economic and social integration with the central county.

The criteria of economic and social integration are defined by examples. It is rather generally agreed that a certain minimal amount of worker-commuting between the candidate outlying county and the central county provides an excellent indicator of integration. The difficulty has been to obtain any data at all on commuting for many outlying counties, and it has been much more difficult to obtain commuting data that cover the whole employed labor force. Hence it has been necessary to examine data somewhat less satisfactory conceptually, in-

⁵ U.S. Bureau of the Census, *U.S. Census of Population, 1950, Vol. II: Characteristics of the Population, Part 1: United States Summary*, pp. 27-28 (Washington: Government Printing Office, 1953).

⁶ "Standard Metropolitan Area Definitions," dated July 28, 1950.

⁷ The outlying county may either be contiguous with the "central county" or with a qualified contiguous county and so on without any fixed limit on the number of outlying tiers. In practice, the most remote tier has been the third, in the case of the New York-northeastern New Jersey S.M.A.

cluding the intensity of telephone communication, the frequency of rush-hour trains and busses between the outlying and central counties, department-store charge accounts in the outlying counties, and even the extent to which the women of the outlying county patronize the hospitals in the central county when having their babies. Each of these indicators has its shortcomings. Hence there has admittedly been an element of judgment on the part of the committee in reaching decisions as to whether an outlying county is integrated or not.

Almost all the present areas were established before the 1950 Census data became available. Additional or more up-to-date facts have also been gathered, and, in the light of them, the committee is re-examining the present areas. Practically all the counties now included, it appears, meet the criteria of metropolitan character. It is possible, however, that different decisions might have been reached for some counties if integration had been examined in the late 1940's in the same way as it is now. There is no denying that attitudes and procedures have evolved in the intervening years. In fact, in the early exploratory discussions of the working committee, consensus among agencies on a particular county was almost a sufficient condition for a decision.

The committee is reviewing not only the new data now available but also the criteria themselves. For one thing, two agencies may disagree on the exact meaning of a criterion, some of which, it must be admitted, are ambiguously worded. Finally, questions are sometimes raised as to what our criteria are to define. What is the underlying concept of a metropolitan area or of a metropolis? Is this a useful or a meaningful concept?

Standard metropolitan areas are not trading areas. They are not newspaper circulation areas. Such areas would go well beyond most of the present S.M.A.'s. As Bogue and others have shown in their analyses of metropolitan regions, there is a broader hinterland over which large cities have various types of dominance, but these are beyond the area of day-to-day contact.

On the other hand, S.M.A.'s are not confined to the built-up area laid out in urban street patterns: that is the urbanized area. Furthermore, S.M.A.'s are not supposed to be homogeneous areas. They consist of a dominant nucleus and a subordinate outlying area.⁸

An issue before the interagency committee is whether an outlying county should be included in an S.M.A. if it is economically integrated, even if it does not have a metropolitan character. The conceptual issue may be illustrated as follows: Suppose there is a county where a qualifying proportion of the workers commute to jobs in the central city. These workers may be part-time farmers or exurbanites living on estates. The county is thinly settled. The land use is essentially rural, not urban or even "suburban"—to use that term without defining it. Few workers have non-agricultural jobs within the county; in other words, there are no factories, offices, and stores in the county employing a large labor force, either absolutely or relatively. To a visitor driving through the county, it does not "look" metropolitan. It consists mostly of open country with a sprinkling of villages and hamlets. There are no new real-estate developments or shopping centers. During the war, fairly large numbers of workers who lived in the open country or in small towns drove 30, 40, or even 50 miles to congested production centers. Some of this type of commuting has continued, especially in the Middle West and Southwest. One study in Oklahoma found that over 1,000 persons commuted from 50 to 100 miles to work at a naval ammunition depot.⁹

To the labor economist, a county where an important number of the resident workers derive their livelihood (or a substantial part of it) by commuting to jobs in a city,

⁸ The non-metropolitan state economic areas, however, are supposed to be relatively homogeneous groups of counties with no dominant center.

⁹ James D. Tarver, *A Study of Rural Manpower in Southeastern Oklahoma* (Oklahoma Agricultural Experiment Station, Tech. Bull. T-56 [Stillwater, September, 1955]).

however distant, should be included in that city's S.M.A. They are part of the city's labor market. On the other hand, many of them may live on farms and in season may be full-time farmers or farm laborers.¹⁰ Moreover, the majority of the workers living in the county do not commute. Human geographers, ecologists, demographers, and housing market analysts may therefore take a different view. To many of them it may seem that this kind of county is not part of the metropolitan area.

It should be pointed out here also that putting the county in a S.M.A. takes it out of a non-metropolitan state economic area and thereby affects the analysis of data for such areas. The National Office of Vital Statistics, for example, is now using the metropolitan-non-metropolitan dichotomy as a convenient cross-classification in tabulating and analyzing vital statistics.

A related problem occurs when the central city is located near a county line and has a closely built-up residential or industrial suburb just across the boundary. The rest of this outlying county is essentially agricultural in nature or at least has little relationship with the central city. Again, some labor economists would like to include this county in the metropolitan area so as not to lose any important part of the labor-market area. Good examples of this type are Crittenden County, Arkansas, near Memphis, and Rankin County, Mississippi, near Jackson. This problem is created by the use of whole counties as building blocks for S.M.A.'s. Also parts of the "central county" itself may not be integrated with the central city.

Perhaps more time should be devoted by the committee to examining the concept of social and economic integration. A more immediate problem, however, is the lack of adequate data for applying operational defi-

nitions. It is generally agreed that commuting to work is one of the chief phenomena to be considered. Unfortunately, there is a woeful lack of statistics on this subject in the United States. Possibly the 1960 Census will supply some much-needed information on county of work by county of residence. Such data, however, would be expensive to collect and to tabulate and would be of no help in revising S.M.A. definitions in time for the basic 1960 Census reports. A more immediate prospect is the proposal of the Bureau of Employment Security to collect from employers in large cities information on place of residence of their employees. The affiliated state agencies would do the field work. The funds are not yet available, however; and there are problems to be solved in securing a representative sample.

The definition of S.M.A.'s in terms of whole counties has been used everywhere except in New England. There the committee made an exception in view of the greater administrative and traditional importance of the town. Moreover, the large cities in New England are so close together that two or more of them are often in the same county or have suburbs within the same county. On the other hand, there are many series of data that are not compiled for the New England towns. The local areas in New England generally favor retaining the use of towns instead of counties. Some national users, however, who are interested in maximum comparability have urged the federal interagency committee to get rid of this exception; in fact, some of them have approximated the New England S.M.A.'s on a whole-county basis in their own analysis.

There is a school of thought that favors more use of parts of counties in other regions as well. For example, San Bernardino and Riverside counties have a combined area of 27,000 square miles, most of which is desert; the Duluth-Superior S.M.A. contains some fair-sized mining towns in the Mesabi Range, 50 miles from the central cities; Greensboro and High Point and Wilkes-Barre and Hazleton are pairs of cities in the same county that have little economic and

¹⁰ To farmers and farm laborers, the urban work-day plus the time required for commuting may not seem excessive relative to the conventional working hours in agriculture. For various reasons, they may prefer to keep their homes in the country. Whether city residents would be willing to move out 50 miles or more and commute is another matter.

social integration. Once one begins making exceptions, however, it might be hard to determine where to draw the new line. Moreover, one of the major advantages of whole counties is that many series of statistics are available for them that are not available for minor civil divisions, a fact which, alone, may go far to explain the greater public acceptance of S.M.A.'s than of the old metropolitan districts.

We must balance this important consideration against the fact that more exact delineations of metropolitan areas can undoubtedly be obtained by splitting counties. Admittedly, counties tend to be larger in some regions than in others. Richard L. Forstall, of Rand McNally Company, has some interesting delineations that make a fairly generous use of parts of counties.¹¹ For some particular purposes, such areas are probably more useful than the corresponding S.M.A.'s.

No matter how the rules are drawn, of course, there will always be marginal cases where the rules do not seem to do justice to the underlying realities. And yet rules are necessary if the objective of *standard* areas is to be met. A detached view of the matter is obtained if we use the analogy of the mathematical concept of the limiting case. Limiting cases are covered by the definition, but they are often far removed from the ideal type.

The question may fairly be raised as to whether the S.M.A. has had a natural history like Topsy or whether there has been a clearly perceived underlying concept that guided the committee. A statistical Pirandello might even label the subcommittee as "Six Characters in Search of a Concept." It is fairly clear, though, that there has been an underlying concept, even though it may have evolved somewhat during the lifetime of the program. At the same time, the necessity remains for periodic reappraisal of the concept and hence of the definition. This

should be done before the 1960 Census. There is always danger that piecemeal revisions of the definition to meet expressed needs or demands may produce a drift away from a defensible concept.

One might well ask, for example, whether the term "metropolis" is justifiably applied to such cities as Sioux Falls, Pittsfield, and Laredo. Paradoxically, while the number of large cities in the United States has been proliferating as the result of population growth and urbanization, the implied status of "metropolis" has been extended to smaller and smaller places. Looking back at the 1910 Census, we find that "metropolitan districts" were first created only for cities of 200,000 or more.

Other issues concerning metropolitan areas include, for example, suggestions that a few very large S.M.A.'s be split into sub-areas and, conversely, that some neighboring S.M.A.'s be grouped into constellations for which combined statistics would also be shown. The decision as to which cities within a S.M.A. are classified as central cities has affected not only the name of the S.M.A. but also analytical uses of the statistics. For example, growth trends in the central cities and in the outlying parts have been compared in studies of suburbanization.

Another suggestion is that urbanized areas instead of incorporated cities be used as the nuclei of standard metropolitan areas and that the criterion be stated in terms of minimum size of that nucleus. This certainly has considerable merit from the standpoint of ecological reality, but it represents another potentially inflationary force with respect to the number of areas. Furthermore, the proper boundaries of the urbanized area may be subject to argument, whereas the boundaries of the incorporated city are objective facts.

There are also questions as to how extensively S.M.A.'s should be used in tabulations of census data. With finite resources for tabulation and publication, choices must sometimes be among the S.M.A., the urbanized area, and the incorporated city when presenting statistics on a certain subject.

¹¹ Map Publications Editorial Department, *United States Metropolitan Areas of 100,000 or More, 1955* ("U.S. Series," No. 2 [Chicago: Rand McNally Co., October 31, 1955]).

Bogue, Dudley Duncan, and others have suggested that the metropolitan-non-metropolitan dichotomy be a part of the basic classification by type of residence so that we should go beyond the traditional urban, rural-non-farm, and rural-farm classification.

The tendency for metropolitan areas to be extended to smaller and smaller cities brings us back to the *Realpolitik* of the program. There is no doubt that most cities are anxious to have an S.M.A. conferred upon them by the federal government. In some cases the local agency or person is interested in the additional statistics that are made available for S.M.A.'s. In many cases, however, the interest is not statistical.

A city government, a chamber of commerce, a newspaper, or a radio station usually regards the establishment of a S.M.A. as enhancing prestige—an attitude reflected in advertisements carried in such publications as *Sales Management* and *Advertising Age*. A medium-sized city held a public celebration with a parade when its S.M.A. was officially announced by the Bureau of the Budget. Many cities, or, rather, certain local interests desire to see more outlying counties added, to make a larger S.M.A., for there is competition to be the largest in the state or to attain some other distinction, such as a certain minimal population, say 200,000. They are motivated by the belief that lucrative national advertising is placed only in the newspapers of central cities of S.M.A.'s of this critical size or that new firms are more likely to locate in an area with a large labor force.

Sometimes these expansionist forces are faced with opposition, as, for example, when a secondary city in, or about to be included in, a S.M.A. campaigns for a S.M.A. of its own or at least for exclusion from that of the larger city, motivated by considerations of prestige, the fear that the city's newspapers will not get a share of national advertising, and so on. In several instances, a county or group of counties wanted a separate area because, unemployment being

more severe there than in the rest of the area, it could then be classified as a "labor-surplus area," which classification would assure it of preference in the awarding of new federal contracts, for the Bureau of Employment Security classifies the employment situation in a S.M.A. as a whole.

There are definite rules concerning the naming of the S.M.A.'s, and cities are often much concerned about being included in a name. In one case, the largest city was strongly opposed to having the name expanded to include that of the second largest city.

Thus the Federal Interagency Committee always has under consideration several appeals for a change. Interested agencies in the local area submit data to prove that a change is now justified on the basis of the official criteria. Often, however, they attack a particular criterion as being unfair or as not recognizing local uniqueness. Occasionally, they may even demand that the federal government cease setting up such arbitrary statistical areas as unfair to the medium-sized cities or infringing on the sovereignty of the outlying city or county. The local agency usually begins by writing or telephoning the Bureau of the Budget or one of the federal agencies on the committee. Frequently it works through its congressional delegation. In one instance, an appeal was even carried to the White House—which backed up the committee's decision. A recent development is the filing of identical petitions with the Bureau of the Budget by municipalities, civic groups, and churches.

The S.M.A. has proved a very useful type of statistical area. The underlying concept is that of a large city with its satellite cities and outlying suburbs. These suburbs go beyond the urban territory to include a farther zone where many of the residents have daily social and economic contact with the central city, the proximity of which affects their way of life and uses of land. The S.M.A. is an approximation to this area, in which whole counties are used as the building blocks.

There are certainly instances in which the actual S.M.A. does not conform very well to

this ideal type. The use of a whole county may provide only a fair approximation to what is wanted. Sometimes, however, a whole county has been improperly included or excluded. (In my opinion, errors of inclusion have been more frequent than those of omission.) The Federal Interagency Committee has done remarkably well in view of the conflicting pressures to which it has been subjected and of the scarcity of data on the important matter of integration.

There is, unfortunately, a tendency for local areas to view the establishment of an S.M.A. as a sort of gold star awarded by the federal government. When the situation changes so that a particular city or county no longer qualifies on a given criterion, exceptionally strong resistance can be expected if what seems a retrogression in status is to be recommended.

The problem of growing numbers of S.M.A.'s is vexing. It has been suggested that the S.M.A.'s be stratified into two or more groups on the basis of size or some related criterion. Then the really metropolitan areas could be distinguished as "principal areas" or by some other discriminating term. Once a discriminating title is used, however—even if it should be as ungainly as "megalopolis"—its attainment may well become a goal for those local groups that are interested mainly in prestige. A better plan would be to restrict certain statistical pro-

grams and certain tabulations to the larger S.M.A.'s, the total population being, as before, published for all areas. So far there appear to have been no complaints from the chambers of commerce in the smaller metropolitan areas because the Bureau of the Census failed to publish detailed analytical cross-classifications for them in 1950.

The very fact that S.M.A.'s are now given so much recognition by the public tends to endanger the program as a source of useful and comparable statistics for planning and scientific studies. If certain local interests are convinced, rightly or wrongly, that their having a S.M.A. with a specified name and content will promote their welfare, it is not surprising if they make strenuous efforts to achieve their aims. To the social scientist their motives may sometimes seem selfish and their methods deplorable. The social scientist's own research interests in nationally comparable metropolitan areas are also selfish, but in a different sense, although even social scientists are not always immune to local "boosterism." We hope that they will continue to be critical of the program in a scientific spirit but that they will help the federal government to achieve a set of areas, based on logical conceptual principles and useful to them and others for administration, planning, and research.

BUREAU OF THE CENSUS

METROPOLITAN GROWTH AND DECENTRALIZATION

LEO F. SCHNORE

ABSTRACT

The current literature on decentralization in the United States suffers from a lack of historical perspective; as a consequence, testable hypotheses phrased in dynamic terms are exceedingly rare. Historically, decentralization was a response to technological innovations, particularly to new facilities for the movement of persons, commodities, and information. Urban centers are assuming a new pattern of spatial relations, but full understanding of the process requires further research designed to test dynamic hypotheses. The greatest deficiencies are to be found in the inadequate empirical study of transportation and communication.

A great deal of effort has been devoted to research in metropolitan growth and decentralization in recent years. In particular, the detailed statistical studies by Thompson, Bogue, and Hawley have provided a clear image of the main demographic facts. However, a search of the literature reveals two important omissions: nowhere is there available a succinct historical recapitulation that provides a summary description of metropolitan development from its beginning to the present time; and, as a probable consequence, the literature contains very few explicitly developmental hypotheses regarding metropolitan growth and decentralization. This paper is addressed to these two broad problems. First, a brief narrative account of metropolitan growth and development is offered. Second, a number of implications derived from the review are set out in the form of concrete problems for research. In each problem the focus is upon process, in a frank effort to offset the static orientation of the available literature. Moreover, a number of the hypotheses refer specifically to the influence of transportation, a factor frequently mentioned but rarely studied in the metropolitan context. The historical treatment is deliberately phrased in very general terms. Most of the statements are well established, although diverse in origin; others rest upon more limited evidence; and a few, while frankly speculative, are phrased as questions for empirical research and not as final answers.¹

¹ Both the historical narrative and the derived outline of research problems are based on the

A HALF-CENTURY OF METROPOLITAN GROWTH

Metropolitan development can best be conceived as a new form of urban growth especially characteristic of twentieth-century America.² It must be recognized, of course, that a large increase in urban population antedated this century. Cities have grown faster than rural areas since 1820. The first decade of this century, however, marked the end of one important phase of urban development, being the last decade in which migration from other countries contributed large numbers to the growth of American cities. The decade from 1910 to 1920, including as it did World War I, witnessed the stemming of the great streams of migrants from overseas, and over-all urban growth was slowed as a result.³

premise that the general pattern of metropolitan development should be established before individual variations are examined in detail. This is not to derogate case studies of individual areas, an extremely valuable source of hypotheses. However, an excessive concern with apparent exceptions appears to be premature at this point; a more fruitful approach is the documentation of major trends. Once the broad set of relationships has been firmly established, the exceptions become variations around these central tendencies, which are themselves subject to explanation.

² See N. S. B. Gras, *An Introduction to Economic History* (New York: Harper & Bros., 1922); and R. D. McKenzie, *The Metropolitan Community* (New York: McGraw-Hill Book Co., 1933).

³ Natural increase was apparently not a compensating factor of any consequence during this period, for city dwellers failed to reproduce at replacement levels. There is evidence, however, that this long-

Restrictive legislation in the 1920's, including a rigid system of quotas, prevented the resumption of international migration on a prewar scale, but the national population continued to increase, and cities grew rapidly, but now, however, largely by internal migration. The attenuated growth of cities in the 1930-40 decade was also the result of a decline in migration, but in this instance it consisted of a lessened flow of internal (i.e., rural-to-urban) migrants. Job

TABLE 1*

RATES OF INCREASE IN POPULATION, BY
METROPOLITAN STATUS, 1900-1950

METROPOLITAN STATUS	1940- 50	1930- 40	1920- 30	1910- 20	1900- 1910
A. Interdecade Rates of Population Increase					
Total United States.....	14.5	7.2	16.1	14.9	21.0
Non-metropolitan.....	6.1	8.5	6.0	6.7	13.6
Metropolitan.....	22.0	8.4	27.5	25.9	32.5
Central cities.....	13.8	5.5	24.2	27.9	37.1
Rings.....	34.2	13.4	33.2	22.4	25.6
Urban.....	26.0	8.0	42.6	35.9	49.2
Rural.....	45.2	21.3	22.0	9.4	8.4
Incorporated.....	34.1	13.2	28.6	24.1	45.0
Unincorporated.....	46.5	22.3	21.2	7.8	5.6
B. Ratios of Total National Increase					
Non-metropolitan.....	0.43	1.18	0.37	0.45	0.65
Metropolitan.....	1.52	1.17	1.71	1.74	1.55
Central cities.....	0.95	0.76	1.50	1.87	1.77
Rings.....	2.36	1.86	2.06	1.50	1.22
Urban.....	1.79	1.11	2.65	2.41	2.34
Rural.....	3.12	2.96	1.37	0.63	0.40
Incorporated.....	2.35	1.83	1.78	1.62	2.14
Unincorporated.....	3.21	3.10	1.32	0.52	0.27

* Source: Leo F. Schnore, "Patterns of Decentralization" (unpublished doctoral dissertation, University of Michigan, 1955), Table 101, p. 214.

opportunities in urban areas, drastically reduced during the depression, multiplied during the early 1940's, largely due to wartime demands. Urban employment was maintained at fairly high levels in the years immediately following World War II as the nation returned to a peacetime economy, and it was further stimulated by the outbreak of hostilities in Korea.

What have been the metropolitan con-

established fact did not hold during the most recent intercensal decade (see Donald J. Bogue and Emerson Seim, "Components of Population Change in Suburban and Central City Populations of Standard Metropolitan Areas, 1940 to 1950," *Rural Sociology*, XXI [September-December, 1956], 265-75).

comitants of twentieth-century urban growth? Part A of Table 1 shows interdecade rates of increase within the entire continental United States. In this table the total land area of the nation is represented according to metropolitan status. The first distinction is that between metropolitan and non-metropolitan parts. The next subdivision in the table distinguishes between the metropolitan central cities and their surrounding "rings." Growth rates for urban and rural parts of the ring are then shown separately. Finally, within the "rural" portion of the ring, separate growth rates are given for incorporated and unincorporated areas. Part B expresses all these rates as ratios of the national increase, providing a control over variation in over-all growth between decades.⁴

Table 1 shows that the presently defined metropolitan areas have captured a disproportionately large share of the total national increase in population throughout the entire fifty-year period. Within metropolitan areas, however, central city growth has become progressively slower, while the ring has tended to grow more and more rapidly. It is this over-all pattern of differential growth in favor of the peripheral area that is usually labeled "decentralization," although these rates reflect only *net* changes arising from a variety of sources.

A part of the slowed city growth is the result of the cities' failure or inability to annex the surrounding densely settled areas.

⁴ Official definitions of the 168 Standard Metropolitan Areas, recognized by the Census Bureau, have been retrojected to 1900, so that area is held constant. The "old" (1940) census definition of "urban" is used throughout. Because of differences in areal definition, these rates differ in minor detail from those given in Donald J. Bogue, *Population Growth in Standard Metropolitan Areas, 1900-1950* (Washington: Government Printing Office, 1953), Table 1. Bogue's data refer to 162 metropolitan areas with "county-equivalent" areas used in New England in place of the town-based areas defined by the Census Bureau. Because the official definitions of the Standard Metropolitan Areas are used here, the data are not directly comparable with those reported in Warren S. Thompson, *The Growth of Metropolitan Districts in the United States, 1900-1940* (Washington: Government Printing Office, 1947).

Probably more significant, however, are two complementary trends in migration and residential mobility: a tendency for residents of the central city to move in increasing numbers to various parts of the adjacent ring area and a tendency for migrants from outside the metropolitan area to move directly to the ring rather than to the city itself. The total effect is a *relative* decentralization or net peripheral growth in excess of that of the center. As far as migration and mobility are concerned, decentralization has two sources—outward *relocation* from the center and growth via *accretion* at the periphery. As yet, however, the relative contributions of these two distinct types of movement have not been firmly established.⁵

Despite our inadequate answers to these basic questions, however, recent studies have established some important relationships between the extent of decentralization and certain structural and functional characteristics of metropolitan areas. One way to present these is to take a deliberately oversimplified view of metropolitan development as a more or less continuous process and to introduce the findings of recent research in historical sequence.

At the turn of the century most urban centers were still rather compact and self-contained entities. Most of the larger cities were at deep-water sites, although a few had begun to develop inland at railway intersections. The residents were concentrated near the principal workplaces, living in tenements and row houses, and most of them walked to work or rode on public vehicles. The horsecar was still very much in evidence on the city streets, although it was being

replaced by the electric streetcar. However, the automobile was still a novelty, and its price was beyond the means of all but the wealthy. Some of the latter who could afford the time and the cost of the trip had already begun to live outside the congested city and to travel to their places of business by automobile or by interurban railway.⁶

These railways—powered by steam or electricity—spread out from the largest cities in radial strings, and along their lines began to appear clusters of dwellings. In the interstices lay wide areas of open country, much of it in farms. Small villages scattered throughout this open country served the immediate needs of the farm population. These subcenters lay at intersections of rural roads and near the railway lines, and through them were distributed the processed goods required by the agricultural population. They also served as the primary collection points for the produce of the agricultural hinterland.

Larger urban subcenters within the orbit of the central city provided the hinterland with less frequently needed goods. In addition to serving as collection and distribution points, these larger places were frequently engaged in the processing of goods, particularly if water power was immediately available. Most industry, however, was still concentrated in the large city, where the economies of steam power could be best realized.⁷ As a rule the larger subcenters had direct railway service to the central city; over these lines flowed the overwhelming bulk of interurban freight. At any rate, the whole arrangement of urban and village agglomerations came to resemble a planet and its satellites.

The larger cities were still growing more rapidly than the smaller places, chiefly through migration, and the rural population was suffering continued losses as more and

⁵ The relative importance of these two migrant streams are known for only a few areas and for a limited and probably atypical time period. Migration data from the 1940 Census can be examined in terms of the 1935 places of residence of migrants living in metropolitan central cities and rings in 1940 (see Warren S. Thompson, *Migration within Ohio, 1935-1940* [Oxford, Ohio: Scripps Foundation for Research in Population Problems, 1951]), and Amos H. Hawley, *Intrastate Migration in Michigan, 1935-1940* [Ann Arbor: University of Michigan Institute of Public Administration, 1953].

⁶ Adna F. Weber, *The Growth of Cities in the Nineteenth Century* (New York: Columbia University Press, 1899).

⁷ National Resources Committee, *Our Cities: Their Role in the National Economy* (Washington: Government Printing Office, 1937), pp. 29-30.

more efficient methods of farming were put into use and as smaller subsistence farms were consolidated into larger holdings. The surplus agricultural population flowed toward the city, probably in a series of intermediate moves, to be joined there by migrants from foreign countries.⁸

Metropolitan areas during the early years of the century were thus characterized by an axiate or star-shaped form of settlement. Most urban places beyond 10 or 15 miles remained largely independent of the center. Within that zone, however, interurban railways were gradually able to provide more regular service; as time passed, more and

statistics (Table 2). At the turn of the century, there were more than four times as many railway locomotives as motor vehicles in operation. As late as 1910, there were more miles of railroad track than miles of surfaced highways in use. After 1920, however, the number of motor vehicles increased significantly, while the number of locomotives began to decline. Similar trends can be discerned when the two types of route are compared.

World War I brought two particularly significant developments. First, migration from abroad, which had provided a large share of the city's manpower needs, was practically cut off. The demands of war, however, compelled urban manufacturing centers to increase their output. To staff the mills and plants, they had to depend on attracting people from other parts of the nation, and for the first time the migrant streams began to include large numbers of Negroes, particularly from the South.¹⁰

The second crucial development occasioned by the war was the rapid increase in the number of motor vehicles. Burdened to their capacity, the railroads were simply unable to carry all the freight that had to be shipped. Motor trucks, which had been used primarily for local hauling within the city, were pressed into service to move less bulky goods between urban areas. An extensive program of highway improvement on all levels of government was put into effect, and hard-surfaced roads began to crisscross the areas surrounding the largest centers. In effect, truck transportation was subsidized by governmental funds, for highways were publicly financed. In addition,

⁹ Homer Hoyt, "The Influence of Highways and Transportation in the Structure and Growth of Cities and Urban Land Values," in Jean Labatut and Wheaton J. Lane (eds.), *Highways in Our National Life* (Princeton: Princeton University Press, 1950), p. 202.

¹⁰ Thus began a South-to-North movement that continues unabated. Most Negro migrants continue to move directly to the central city rather than to the metropolitan ring. Racial differentials in the various migrant streams involved in decentralization have yet to be fully reported.

TABLE 2*

RAILROAD AND MOTOR VEHICLE ROUTES
AND CARRIERS, 1900-1950

Year	Railroad Trackage (Miles)	Surfaced Highways (Miles)	Railroad Loco- motives (No.)	Motor Vehicles (No.)
1950.....	223,779	1,714,000	42,951	48,566,984
1940.....	223,670	1,367,000	44,333	32,035,424
1930.....	249,052	694,000	60,189	26,531,999
1920.....	252,845	369,000	68,942	9,239,161
1910.....	240,293	204,000	60,019	468,500
1900.....	193,348	128,500	37,663	8,000

* Source: U.S. Bureau of the Census, *Historical Statistics of the United States, 1789-1945* (Washington: Government Printing Office, 1949), Tables K-29, K-34, K-175, K-182, and K-229, and *Continuation to 1952 of Historical Statistics of the United States, 1789-1945* (Washington: Government Printing Office, 1954), same tables.

more people working in the city found it possible to live outside its formal boundaries. These early suburbanites lived in new residential developments within walking distance of the railway commuter stations. In Hoyt's words, "as these communities gradually coalesced in solid bands, the settled area maps of the New York and Chicago metropolitan areas showed long finger-like appendages extending out, with large vacant areas lying in between. This was the result of the faster travel time on the suburban railroads than on other means of transportation."⁹

An appreciation of the importance of the railroad during the early part of this century can be gained from a review of historical

⁸ Early urban research, of course, concentrated on the clusters of ethnic settlements within large cities. However, comparative data on segregation are notably deficient for more recent periods.

some of the methods learned under the pressure of the wartime demand for motor transport were applied to the production of private automobiles. Many of the techniques of modern mass production—later adopted in almost every sector of the economy—were first developed in the automobile industry.

The years immediately following World War I, although marked by a few minor fluctuations, ushered in a period of expansion. Enormous strides were made in industrial productivity, and, as national production increased, significant advances occurred in real wages. The techniques of mass production and increased mechanization reduced the manpower required in industry. Since a similar trend was continuing in agriculture as an effect of the introduction of power machinery, the surplus population from both agriculture and industry gradually shifted into occupations providing for the distribution of goods and services.¹¹

Spatial changes followed from these technological innovations. At the same time that these fundamental transitions were taking place in the national economy, the physical pattern of the large city and its surrounding area began to undergo crucial alterations. Decentralization, which had occurred first in only the largest centers, became a significant aspect of the growth of many smaller cities in the 1920's; both industry and population were scattering as a response to the development of the motor vehicle. The hard-surfaced route, of course, was adaptable to the movement of people as well as to the carrying of goods. The elaborate networks of main arteries and feeder routes around large and middle-sized cities permitted a number of the functional components of the community to break away from the center. Most of them located at intersections in the highway network. Urban subcenters appeared in increasing numbers and grew at rates in excess of that of the center. New construction was started in volume in the

periphery of both large and middle-sized cities. As residential population gathered in sufficient densities, retail and service establishments arose to provide the suburbanites with urban conveniences.

With the increased ease of travel, however, some of the larger subcenters underwent a significant transition. They lost their high degree of independence and fell under the dominating influence of the metropolis. For example, many establishments devoted to the provision of luxury goods abandoned operations in the subcenters, being unable to compete with the metropolis, which was now easily accessible to a wide market. At the same time, the principles of mass production were increasingly adapted to distribution, and chains of retail outlets began to appear, particularly in the convenience-goods lines. Such units, under a single ownership and directed from a site in the central city, could take advantage of the economies of mass buying and standardization. During the same period, significant changes in communication came about with the development of the radio and the telephone. Instantaneous contact with a broad area became possible, and the independence of subcenters was diminished accordingly.

At the same time, industry became increasingly free to locate away from the city itself, as cheap electrical power replaced steam.¹² The telephone permitted peripheral location of production facilities while the functions of management and control could remain in the center. In addition, the widespread ownership of the automobile meant a more mobile labor force. Heavy industry, which tends to operate more efficiently in one-story buildings occupying large areas, apparently was particularly attracted by the lower costs of land in the ring, where the competition of alternative uses was less intense.

However, not all elements of the community were equally free to participate in this outward movement: during the early phase of decentralization, many activities were

¹¹ For the original distinction between "primary," "secondary," and "tertiary" industries and occupations see Colin Clark, *The Conditions of Economic Progress* (2d ed.; London: Macmillan Co., 1951).

¹² National Resources Committee, *op. cit.*

bound to the center as securely as in previous years. The retailing of luxury goods and the provision of infrequently needed services were particularly obliged to remain in central locations, in order to maintain maximum access to a large potential market. The functions of management and direction also appeared to cling to the central location, perhaps in order to facilitate contact with other units engaged in communication, finance, and marketing.

At any rate, the depression of the 1930's appears to have accentuated the trends of differential growth incipient in many areas in previous years. With decreased job opportunities to offer, central city growth dropped to a low level, with many cities sustaining net losses. The growth of the residential population of the metropolitan ring, although reduced from the levels of the previous decade in many instances, tended to remain above that of the center and of the nation as a whole. Although there is little evidence of a genuine "back-to-the-farm" movement during this decade, it appears that there was considerable piling-up of potential migrants in the outlying areas.

It is clear, however, that, within metropolitan rings, "rural" growth exceeded urban during the depression decade, and the growth of unincorporated rural areas was in excess of that of small incorporated places.¹³ The threat of war and the subsequent armament drive in the last years of the decade probably pushed the interdecade growth rates of many central cities to higher levels than would otherwise have been realized. In spite of the probable resumption of heavy cityward migration toward the end of the thirties, ring growth tended to exceed the growth of even the smaller metropolitan cities. A distinct majority of the cities of 50,000 and over were now exhibiting the pattern of relative decentralization formerly seen around only the larger cities.¹⁴

¹³ The old (1940) definitions of "urban" and "rural" used here tend seriously to overstate the rural component; many people classified as rural are actually urbanites by any reasonable functional definition.

Finally, the most recent intercensal decade (1940-50) has witnessed a progressive diffusion of the patterns that had begun years earlier in the largest centers. Not only metropolitan centers but the larger satellites within the metropolitan orbit are decentralizing. The growth of "rural" and unincorporated area continues to outstrip that of the urban and incorporated places. The physical form of the metropolitan area, which had been axiate in pattern, is filling in, and the area resembles a great amorphous mass, although outlines of the older star-shaped pattern can still be discerned. More important, the functional boundaries of the metropolitan area, as indicated by the outward shift of high growth rates, appear to have shifted from a ring of approximately 10 miles to one of 20-25 miles in diameter.¹⁵

Throughout these fifty years of transition, a number of structural and functional circumstances appear to have been related to centrifugal growth, the most important of which is sheer size of population. In fact, most of the other factors associated with centrifugal growth are themselves associated with metropolitan size. Regional location also appears to be a factor of real significance. Other variables associated with size and regional location, however, show associations with decentralization that remain when these two variables are controlled.

In addition to the primary variables, recent research has indicated that the areas in which decentralization occurred first and proceeded furthest tend to have densely populated central cities, in which growth has been slow during the last fifty years. They are most frequently older coastal cities. Manufacturing activity within the area has been decentralizing throughout most of the period.¹⁶ All these findings, when taken to-

¹⁴ Donald J. Bogue, *Metropolitan Decentralization: A Study of Differential Growth* (Oxford, Ohio: Scripps Foundation for Research in Population Problems, 1950).

¹⁵ Amos H. Hawley, *The Changing Shape of Metropolitan America: Deconcentration since 1920* (Glencoe: Free Press, 1956).

¹⁶ *Ibid.* In each instance the direction of association between a given variable and centrifugal

gether, suggest the importance of what might be termed the "maturity" of metropolitan areas. Those areas which have exhibited the earliest and most extreme evidence of decentralization appear to have reached an advanced stage of maturity that is merely reflected in the structural and functional characteristics enumerated here. In a rough sense, in fact, decentralization is an index of the maturity of metropolitan areas.

SOME RESEARCH IMPLICATIONS

In addition to the research needs touched upon previously, a number of other problems present themselves for empirical scrutiny. A whole category of research problems can be subsumed under the rubric of the sources of differential growth within metropolitan areas. In addition to the relative contributions of natural increase and net migration, we need to know the origins of migrants by areal and functional types. Another whole range of empirical questions emerges when we consider the demographic and functional composition of the various parts of the metropolitan area and the migrant streams that flow between them. Imposing as these problems are, however, they relate only to the residential population of metropolitan areas.¹⁷

In addition to a concern with the redistribution of residential population, of course, a full description of the changing

growth tends to be the same as that found between the variable in question and metropolitan size. The direction of these relationships remains the same, although reduced in extent, within size classes. Although no single area can be found to possess every one of the characteristics, they tend to be associated with one another.

¹⁷ Albert J. Reiss, Jr., has recently suggested a number of excellent topics for research in this area (see "Research Problems in Metropolitan Population Redistribution," *American Sociological Review*, XXI [October, 1956], 571-77). A number of his topics, however, are static in orientation. Another valuable discussion is contained in Henry S. Shryock, Jr., "Population Redistribution within Metropolitan Areas: Evaluation of Research," *Social Forces*, XXXV (December, 1956), 154-59.

organization of the metropolitan area must treat the other sociological units that constitute the total community. It appears that all the typically urban activities—the so-called "secondary" and "tertiary" functions of fabrication, distribution, and control¹⁸—have been subjected in some degree to the same forces of decentralization that have so dramatically altered the residential settlement pattern within local areas. The reduction of the friction imposed by distance has had noticeable effects in almost every sphere of life.

With respect to secondary (manufacturing) activities, there is obviously increasing freedom to locate at the margins of the community. As Bogue points out, "economists and industrialists have discovered that under modern conditions of transport it is no longer necessary for great industries to be located within the limits of the central city. There is a broad zone of indifference, probably several miles in diameter, which is locationally suitable."¹⁹ Tertiary activities (trade, services, etc.) are probably affected similarly by changes in locational tolerance, but they have been less carefully studied. The decentralization of functions such as wholesaling, storage, and distribution deserves more research. These activities have been traditionally viewed as centrally oriented, but recent developments in metropolitan organization warrant a reconsideration of this assumption. Faster and more frequent transportation, for example, may have decreased the need for central warehouse facilities. The handling of freight since the development of the motor truck has become a much more flexible operation, and a great deal of storage is apparently affected en route, without the necessity for maintaining large stockpiles and inventories immediately at hand.

Many administrative functions may also be increasingly free to leave the center and

¹⁸ Clark, *op. cit.*

¹⁹ Donald J. Bogue (ed.), *Needed Urban and Metropolitan Research* (Oxford, Ohio: Scripps Foundation for Research in Population Problems, 1953) p. 40.

locate at the periphery of the metropolitan community. For example, the central offices of large insurance companies, whose chief contacts are with agents scattered throughout the nation, may represent a type of administration that can be as efficiently managed in the ring as in the center. The control and direction of other industries, however, which require frequent contact with lawyers, brokers, news media, advertising agencies, and out-of-town buyers may continue to require central location. Further research should identify the other units with which a given function is in most frequent contact—via both transportation and communication facilities—in addition to its requirements for space, in both amount and kind. These facts would provide valuable clues to the amount of decentralization to be anticipated among various functions.

Functional differences between suburban and satellite places still remain to be explored, and detailed knowledge of them is necessary for a full description of the social and economic organization of the expanded community.²⁰ In addition, more should be learned of the growth tendencies of different types of subnuclei in the metropolitan ring. Employing satellites, for example, show patterns of growth notably different from those of exclusively residential suburbs.²¹ The growth of more specialized areas, including educational and recreational centers, may show divergent patterns in keeping with their narrowly specialized roles in the whole metropolitan area.

Trends in population growth within the central city itself are worthy of further exploration. Physical congestion in the center has frequently been advanced as a cause of decentralization. Most large cities have high

proportions of habitable land that remains vacant,²² but a substantial portion of it is in small parcels held for speculation, forcing prices beyond the limits possible for residential development.

At the same time, a more accurate description of the role of congestion can probably be gained by turning attention to traffic, itself a product of the separation of land uses seen in residential decentralization.²³ Traffic densities probably exert a greater influence than the more frequently measured densities of residential population. The daily massing of great volumes of people and vehicles in central areas may inhibit movement to such an extent that the center loses its traditionally favored position as the point of maximum accessibility to the entire metropolitan area.

A closely related area of metropolitan research offers great promise for cross-cultural comparisons. A number of studies of the daily journey to work have been conducted in both Europe and the United States.²⁴ The studies in this country have been based primarily upon by-product data from traffic research, and being limited to areas with particularly serious traffic problems, the American investigations probably represent a biased sample of all urban areas. Many of the European studies, however, have been based upon census materials, for the census schedules of most European nations include at least one question regarding the place of work of members of the employed labor force.²⁵ This is one of the rare instances in which the United States census lags behind data-collection in other nations of the world. Information on place of work in future censuses in this country would permit a much more complete description of the functional organization of the entire metropolitan area;

²⁰ See Sanford M. Dornbusch, *A Typology of Suburban Communities: Chicago Metropolitan District 1940* ("Urban Analysis Reports," No. 10 [Chicago: Chicago Community Inventory, May, 1952]), and Leo F. Schnore, "The Functions of Metropolitan Suburbs," *American Journal of Sociology*, LXI (March, 1956), 453-58.

²¹ Leo F. Schnore, "The Growth of Metropolitan Suburbs," *American Sociological Review*, XXII (April, 1957), 165-73.

²² Harland Bartholomew, *Land Uses in American Cities* (Cambridge: Harvard University Press, 1955).

²³ Donald L. Foley, "Urban Day-Time Population: A Field for Demographic-ecological Analysis," *Social Forces*, XXXII (May, 1954), 323-30.

²⁴ *Ibid.*; and Kate K. Liepmann, *The Journey to Work* (New York: Oxford University Press, 1944).

²⁵ Foley, *op. cit.*

the daily circulations and exchanges—centrifugal, centripetal, and lateral—between the various subparts of the area could then be accurately determined.²⁶

Previous remarks imply that long-distance commuting is restricted to upper-income groups. However, the rapid increase in automobile ownership in all social strata in our society has made suburbs and satellites, as well as the unincorporated places in the ring, accessible to those with moderate incomes and even to some with lower incomes. Wherever zoning regulations are not in effect, cheap housing can be built on cheap land. Scattered research has suggested that the European pattern on part-time farming by urban workers may become established in the vicinity of many American industrial cities, particularly near those in which factory work is seasonal.²⁷ Ride-sharing arrangements between urban workers who live in the ring are still another device permitting peripheral residential location of families which otherwise could not afford the high cost of transportation to the center.²⁸

²⁶ The rapid advances made in sampling techniques should permit this valuable addition to the 1960 Census. Technical problems can be solved by experimentation with alternative methods in the Current Population Survey and in special state and local censuses prior to adoption in the federal decennial census.

²⁷ Nathan L. Whetten and R. F. Field, *Studies of Suburbanization in Connecticut*, No. 2: *Norwich: An Industrial Part-Time Farming Area* (Storrs: Connecticut State College Agricultural Experiment Station Bull. 226 [1938]); W. R. Gordon, *Satellite Acres* (Kingston: Rhode Island State College Agricultural Experiment Station Bull. 282 [1942]); Walter Firey, *Social Aspects of Land Use Planning in the Country-City Fringe* (East Lansing: Michigan State College Agricultural Experiment Station Bull. 339 [1946]); Glenn H. Beyer, *Housing and Journey to Work* (Ithaca: Cornell University Agricultural Experiment Station Bull. 877 [1951]).

²⁸ Leo F. Schnore, "The Separation of Home and Work: A Problem for Human Ecology," *Social Forces*, XXXII (May, 1954), 336-43. The so-called "marginal labor force" appears to be physically marginal to industrial cities. This suggests that the "rural-urban fringe" is amenable to identification in functional (occupational) as well as areal terms. The latter conception, in fact, may be extremely misleading in many instances.

More generally, trends in housing have had an important place in this entire development, but here research is seriously deficient. With the passage of the years, the techniques of mass production have been adapted to the construction of dwellings. We need to know the dynamic causal factors operating to bring about areal differentials in construction of various types within the metropolitan community. Such knowledge would throw light upon the problem of the redistribution of residential population, as well as other functional components, such as industrial and commercial establishments.²⁹

In this connection, some research should be directed toward delineating more carefully some of the differences in socioeconomic level between suburban and satellite places within the metropolitan ring. Rental and income data are now available for at least the larger incorporated places in the ring, and these can be easily supplemented by statistics on education and occupational and ethnic composition. Many observers have suggested the emergence of rigid segregation in the suburbs along social and economic lines. Indeed, it is said that the mass production of suburban housing attracts persons of similar status,³⁰ an economic compulsion toward segregated living which seems to be further implemented by zoning. Whether these trends are any more coercive, however, than the forces that have long made for segregation *within* the city is a matter for future research.

This discussion has made use of the newly developed census concept of the Standard

²⁹ Dorothy K. Newman, "Metropolitan Area Structure and Growth as Shown by Building-Permit Statistics," *Business Topics*, IV (November, 1956), 1-7. Within metropolitan areas, new housing construction is especially rapid in the ring. Although Newman presents data for only a small number of areas, all but 8 of the 168 currently recognized Standard Metropolitan Areas have higher proportions of newly constructed dwelling units in the ring than in the central city.

³⁰ Leslie Kish, "Differentiation in Metropolitan Areas," *American Sociological Review*, XIX (August, 1954), 388-98.

Metropolitan Area. Every use in research of such a statistical reporting unit, of course, constitutes something of a test of its validity. There is some evidence that the Standard Metropolitan Area encompasses only the zone of most rapid growth in recent years, but both Bogue and Hawley have shown that the influence of the metropolis is reflected in the growth rates of areas many miles beyond the commuting zone. Beyond this zone of primary interchange, of course, lies a much broader hinterland in which integration with the center is expressed through indirect contacts. Thus research cannot be confined to the Standard Metropolitan Area alone. The area of direct contact with the center, however, appears to be well delineated by the definition. At any rate, the utility of these areas as reporting units is enhanced by the vast amount of data assembled on a county basis by other governmental and private agencies. Fuller use of the data will permit at least tentative answers to many current questions.

Many problems of administration and planning arise out of our ignorance of the details of change in the form of the community. The provision of accurate descriptions of community structure is the responsibility of sociological research, but the task is far from complete. If we are ever to solve the host of practical problems so rapidly developing in the wake of decentralization, we shall have to assemble more and more facts. But, even more important, we shall have to provide conceptual schemes with which to interpret these facts.

The problems raised in this paper point

to the fact that we need a complete theory, subsuming both structural and functional aspects of all the constituent units of the community. Moreover, such a theory should be capable of generating testable hypotheses referring to both static and dynamic relationships between variables. Technological innovation, land-use conversion, segregation, population growth and redistribution—all these are terms referring to *processes*. Any sound conceptual approach must be phrased in dynamic terms such as these, if for no other reason than that the modern metropolitan community is constantly changing.

The construction of such a theory will be no mere intellectual exercise. As one demographer has recently asserted:

One of the reasons for such strong disagreements and conflicting recommendations about so-called "decentralization" is that specialists in the field of urban population and human ecology have failed to produce a theory of urban growth that is valid for the mid-20th century. Perhaps we have been overly concerned with perfecting a static theory of city structure. . . . Our study of structure needs to be accompanied by a rigorous program of research into growth and change. Research in urbanism and metropolitanism should have dynamic as well as static aspects.³¹

One can only agree with these ambitious goals and hope that the discussion contained in this paper will contribute to their ultimate achievement.

UNIVERSITY OF CALIFORNIA (BERKELEY)

³¹ Bogue, *Needed Urban and Metropolitan Research*, p. 38.

SUBORDINATION AND AUTONOMY ATTITUDES OF JAPANESE WORKERS

JAMES C. ABEGGLEN

ABSTRACT

This study is an examination of differences in attitudes toward traditional relationships within social groups between workers from rural and those from urban backgrounds in large Japanese industrial firms. They lead to a reconsideration of the process of migration from rural villages to industrial employment and of the role of the younger son in the rural families of Japan as a psychological source of social change.

The study of attitudes of employees reported here was undertaken in the course of an examination of the social organization of the large Japanese factory.¹ The formal organization of large industrial units in Japan is very much in the traditional mode, with paternalistic group organization and traditional styles of obligation and reward in the incentive system. Important changes in recent years in the Japanese social system, accelerated by urbanization and industrialization, raise questions of the fit of employee attitudes and goals—especially the attitudes of younger, urban-reared workers—to the organization, since under changing social conditions the patterns of organization developed in an earlier era may well lose their effectiveness to control and direct the behavior of people trained in newer modes of interaction.

To what extent do stresses, actual or potential, arise in these industrial units as a result of a lack of congruence between the structure and the attitudes and expectations of its members? In what directions might such stresses impel change in the pattern of industrial organization? To address these questions, it was hypothesized that there would be a continuum from acceptance of traditional interaction patterns by workers

from rural backgrounds to more autonomous and rationalized attitudes on the part of urban-reared offspring of industrial workers.² The hypothesis is simple and obvious. The study shows that the hypothesis is excessively simple and that the relation between worker attitudes and background is a good deal less obvious.

THE STUDY SAMPLE

A total of 175 completed questionnaires was obtained at six sites. Locations and number of subjects included 20 employees of the Tokyo office of a synthetic fabric manufacturing company; 40 employees of a Shikoku factory of the same firm; 20 employees of an electrical equipment manufacturing plant in Tokyo; 20 office employees of a wire and cable manufacturing firm in Osaka; 25 employees of an iron- and steel-processing firm in Osaka; and 50 apprentices in an electrical equipment manufacturing firm some distance from Osaka in the Kansai district. Each firm has over 2,500 employees. In the two offices selection was made at random from among the non-supervisory group; in the factories, selection was made at random from laboring personnel, separating men and women to provide sufficient representation of women in the total sample.

¹ Mrs. Asano Michiko and Messrs. Hachiya Saburo, Kimura Yutaake, Kitagawa Kazue, Howard F. Van Zandt, and Yamada Junichiro were especially generous in aiding in the collection of these data, as in many other ways during the research period in Japan. The study was made under a fellowship granted by the Ford Foundation; the statements and opinions are those of the author and not necessarily those of the Ford Foundation. A report on the larger study is in preparation.

² A similar thesis is put forth in the UNESCO report on the attitudes of young Japanese in the postwar period: "Personal autonomy seems more widespread in the towns than in the country-side; not only do urban living conditions and a higher level of education make independence easier psychologically, but also as a new cultural trait, it was accepted first in the towns" (Jean Stoetzel, *Without the Chrysanthemum and the Sword* [New York: Columbia University Press, 1955], p. 232).

Two rural factories are included, as well as one from each of Japan's two major metropolitan centers, Osaka and Tokyo, in which cities the offices also are located. There is then some range in the sample, but it is in no way systematically representative of Japan's industrial work force and is skewed to an overrepresentation of younger workers and female employees.

THE ATTITUDE QUESTIONNAIRE

The method employed and about half the items used on the attitude questionnaire are adapted from a scale developed for use in

TABLE 1

There is hardly anything lower than a person who does not feel a great love, gratitude, and respect for his parents.....	1.8
A good foreman looks on his workers as a father does his children.....	1.9
No sane, decent person could ever think of hurting a close friend or relative.....	2.0 (3.5)
A good worker cannot help but be grateful to his company for giving him a job.....	2.3
Prostitutes are hardly better than criminals and ought to be severely punished.....	2.3
Obedience and respect for their parents are the most important virtues children can learn.....	2.4 (4.0)
What the youth needs most is strict discipline, rugged determination, and the will to work and fight for family and country.....	2.6 (4.0)
Young people sometimes get rebellious ideas but as they grow up they ought to get over them and settle down.....	2.7 (3.5)

the study of prejudice and antidemocratic attitudes in the United States.³ In constructing the questionnaire, five subcategories of personal relations were taken to be important to the general question, and five items were designed for each of the categories. The first includes attitudes toward the nation and toward national symbols, from the strongly ethnocentric and nation-

alistic, to antinationalistic slogans or beliefs. The second area is that of attitudes toward family authority and interaction, from subordination to familial and especially paternal directives and strong family loyalties, to rejection of familial obligations. Third, attitudes toward heterosexual relations and the social role of women were examined, with the extreme position supporting male dominance and sharp social differentiation between the sexes. The fourth category deals with job relations, from antiunion and promanagement attitudes colored by paternalism. Finally, a set of items was included to measure a generalized acquiescence with authority, submission to strong leadership, and acceptance of a sharp division of groups into leaders and followers.

Subjects were asked to indicate the extent to which they agreed or disagreed with each item, scoring 1 for strong agreement, to 6 for strong disagreement. Thus, in general, low total scores on the scale indicate support of nationalistic, paternalistic, and traditional behavior. High scores indicate rejection of nationalism and feelings of disaffection and dissatisfaction with the traditional family system, support for equality between male and female, a rejection of paternalism at work, and general dissatisfaction with figures of authority and authoritarian behavior. The poles of the continuum might be characterized as subordination—acceptance of traditional relations and interactions—at one extreme and autonomy—an emotional dissociation from traditional authority and interaction patterns—at the other.

STRONGEST AGREEMENT

The eight attitudes listed in Table 1 are those most commonly held by the whole group. The average score of the total group on each item is given to the right, and the approximate score on a comparable American item for a similar sample, if available, is given parenthetically.

The four items that elicited strongest agreement from these subjects and, indeed, most of the items strongly agreed with by

³ T. W. Adorno *et al.*, *The Authoritarian Personality* (New York: Harper & Bros., 1950), pp. 57-279.

the total group have in common the recognition of group obligations and the respect and gratitude owed family, firm, and nation by children, workers, and citizens. Contrasting data are not available, but it is difficult to imagine American employees of a large industrial firm agreeing that their foreman or supervisor should look on them as a father does his children, and, in all probability, gratitude to the firm for their jobs would not quite be the response of American employees of, let us say, U.S. Steel or Dupont.

More impressive than these attitudes (and dynamically related to them, it might be argued) is the agreement on the propriety and, indeed, the necessity of youth's recognition of their familial obligations, of disciplining themselves to satisfy these obligations, and of controlling their feelings and actions to the ends socially prescribed. Such comparisons as are available reveal much stronger agreement among this Japanese group than among equivalent American groups in the expression of familial obligations. The general response strongly supports the well-known analysis of Japanese ethos by Benedict.⁴

But conclusions regarding the reciprocal bonds of obligation and duty in family and work relations should not be overstated, for further examination of the questionnaire shows that they are tempered in specific contexts. Before proceeding to this issue, however, a note must be added regarding the item "Prostitutes are hardly better than criminals and ought to be severely punished." It was assumed that with a general recognition of sexual equality would go a tempering of punitive attitudes toward prostitutes. Whatever the merits of the assumption, in fact persons generally accepting the feminist position tended to agree strongly that prostitutes should be punished, while those with more traditional views of the role of women were at the same time more tolerant of prostitution. Some of this reaction is, of course, the result of the

traditional acceptance of prostitution in Japan. Judging from discussion with respondents, however, a more important factor is the view, by rather militant young women workers especially, that prostitutes degrade women generally and hamper the achievement of equality. Thus they are singled out for punishment as betrayers of their sex, in a sense, and so this item on the questionnaire has a negative correlation with the over-all score.

INTERMEDIATE RESPONSES

The nine items in Table 2 received intermediate scores, with no strong group feeling of either agreement or disagreement.

TABLE 2

Human nature being what it is, this country will always need strong leaders who will show the people what to do.	3.2
What this country needs most, more than laws and political programs, is a few courageous, tireless, devoted leaders in whom the people can put their faith.	3.3 (4.5)
It is difficult to imagine how any man could work for or take orders from a woman.	3.6
Japan is the most cultured nation in the world.	3.6
The worst danger to Japan in the last fifty years has come from foreign ideas and agitators.	3.6 (3.0)
It is only natural and right for each person to think his family is better than any other.	3.7
If women are granted full equality and take part in business and politics freely, it will destroy the family system on which our nation is based.	3.7
Family discussion is all right, but in the end it is the husband and father who must decide what is right. . .	4.2
Whom a young person should marry can be decided best by the parents	4.3

Two statements about heterosexual relations fall into this intermediate group—"It is difficult to imagine how any man could work for or take orders from a woman" and "If women are granted full equality . . . it will destroy the family system. . . ." The

⁴ Ruth Benedict, *The Chrysanthemum and the Sword* (Boston: Houghton Mifflin Co., 1946).

response to these suggests, in general, a move toward greater equality of status for women in Japan, not confined to those of higher status but general among the others as well.

It is interesting to note, too, that attitudes toward the nation fall into this intermediate category. The item "Japan is the most cultured nation in the world" was taken from comments by the education

TABLE 3

Workers' conditions and wages will be best when they leave the decisions on these matters to the knowledge and judgment of the men who run the company.....	4.5
The emperor is more than a symbol of Japan and should be restored to his former position.....	4.6
War results in a defeat for the side that feels defeated; if Japan had not felt defeated, she would not have been defeated.....	4.9
Japan needs to return to a system of strict separation of boys and girls in school until they are older.....	5.0
It is only natural and right that men from leading families with college education should become company executives.....	5.0
People can be divided into two classes: the weak and the strong..	5.1 (2.5)
The best guarantee of our national security is for Japan to have the biggest army and navy in the world and the secret of the atom bomb..	5.2
Labor unions have become altogether too powerful and should be suppressed by the government.....	5.5

minister on his return from a world tour in 1956 but, while typical of a species of Japanese xenophobia, it is not generally supported by these subjects. Further, the statement that "the worst danger to Japan... has come from foreign ideas and agitators" not only has rather limited support but is agreed with less by these subjects than was its equivalent in America.

Of more importance are perhaps the two statements dealing with family relations which are disagreed with slightly by the total sample. While general expressions of family obligation and duty are strongly

agreed with, as indicated above, these two items are more particular—"Whom a young person should marry can be decided best by the parents" and "... In the end it is the husband and father who must decide what is right." The scoring outcome suggests that, within a general recognition of obligation and duty to family and within a general acceptance of a patriarchal family, specific decisions and actions are excepted. Thus, for example, while there is quite strong agreement that "obedience and respect are the most important virtues," the propriety of a family decision about marriage partners and the general rule that father knows best are not wholly concurred in.

STRONGEST DISAGREEMENT

The disparity between general attitudes and specific behavior is further illustrated by the eight items that elicited strongest disagreement from the total group of workers (Table 3).

The item most strongly disagreed with is the last-named, regarding the advisability of suppressing labor unions. It should be noted that this questionnaire was used shortly after a general "struggle" by Japan's largest and rather left-wing unionists, a "struggle" that included a bitter strike in the mining industry and aroused considerable ire among conservatives. Still, and in the context of industrial paternalism noted earlier, this item on union strength is strongly rejected, as is, too, the item stating that decisions about workers' wages and conditions are best left to management. Here, as with the items dealing with specific family problems, while obligations and duties are admitted, exceptions reveal coexisting, seemingly contradictory attitudes.⁵ These

⁵ A study of worker attitudes recently conducted in Japan reports a positive correlation between identification with the union and with management in two large factories. That is, workers with positive attitudes toward management were generally also positive in attitude toward the union, and so with negative attitudes (see Odaka Kunio, *Sangyo ni okeru Ningen Kankei no Kagaku* ["Science of Human Relations in Industry"]) [Tokyo: Yuhikako Kabushiki Kaisha, 1953], pp. 329-34).

need not be added to the fatiguing lists of "contradictions" in Japan or of things Japan does "backward." In the West, too, social logics often defy scientific logic. It might be better merely to conclude that serious errors are possible when inferring (as Benedict necessarily did in her wartime study) specific attitudes and actions from what is seen as the larger ethos of the Japanese people.

The persistent lack of support for nationalistic attitudes is most marked on the item concerning Japan's defeat. This statement was taken almost verbatim from a widely reported speech by a wartime leader on his recent release from Sugamo prison, and the theme that Japan might well have continued her fight to success, had she been stalwart in spirit, was clear in the literature commemorating the eleventh anniversary of the war's end. Still this, with the items on national security and on a change in the emperor's status, was roundly rejected.

A note might be added concerning responses to the statement that "Japan needs to return to a system of strict separation of boys and girls in school until they are older." During 1956 there was increasing agitation, led by cabinet members, for just this reversal of an Occupation-sponsored change. Delinquency, aberrant sexual behavior, and general disorder among youths were all laid to the postwar mingling of children in schools, rejecting the Confucius-sanctioned separation at seven years that had been the Japanese rule. As part perhaps of the general acceptance of changes in heterosexual relations and in the role of women, this group of Japanese workers strongly disagreed with the suggestion to restore tradition.

In sum, while the sample and the instrument preclude wide generalization, traditional attitudes seem most strong at present in the more intimate contexts of family and work group. There is a marked rejection of traditional or ethnocentric views at the national level and a fair degree of acceptance of new modes of male-female relationships. Further, while the recognition and acceptance of obligations and of authority within

more intimate groups remain very strong, specific behavior appears to be excepted.⁶

Age and sex differences in attitudes.—In general, it is a sound principle in studying Japan to assume that men and women in feelings and actions require separate analysis and discussion. However that might be, in the case of this questionnaire no significant differences were observed. Fifty-three per cent of the total sample are males, and, although there is a slightly higher mean score for males, the difference is well within the range of chance. Further, within the various education, age, and background subgroupings, male and female subscores resembled each other closely. As might be expected, while, in general, scores on individual items were similar for men and women, the items relating to separation of boys and girls in school and to the difficulties found by men in taking orders from women were disagreed with more by females than by males.

The mean scores of the 45 per cent under twenty years of age and the 55 per cent over twenty do not differ significantly. Nor did there appear to be any regular shift in mean score when the age groups were further divided or when age and sex or age and background were analyzed. In other words, the attitudes measured are not, insofar as evidence is available, a function of youthful rebellion or of normal maturation of attitudes where differences may be noted: throughout the age range there is a common level of attitudes.

Attitudes and education.—To examine the relationship between questionnaire scores and education, the total group was divided into two subsamples, one of individuals whose education was no more than the minimum amount required in Japan by law (at present through middle-school graduation). The other group is predominantly of higher-school graduates and includes all

⁶ This explanation is relevant as well to the contradictions in attitudes reported by Stoetzel (*op. cit.*) and to the differences in attitude toward marriage between his sample and that of this study (see esp. pp. 181-90 of the UNESCO report).

persons whose education extended beyond the legal minimum. The average score for the middle-school group is 89.4, and for the higher-school group the mean is 94.3, a slight difference, which, however, is statistically significant.⁷ Thus it may be maintained that the attitudes at issue here are in some part a function of education, with better-educated individuals tending to be more autonomous and less tradition-oriented than middle-school graduates.

Social background and attitudes of workers.—The hypothesis that provided the initial direction for this study is that the worker born in the agricultural village of Japan and reared in the more tradition-bound rural family and village milieu will score lower on the questionnaire on the average and will be more subordinate in his attitudes toward parental, job, and national authority; while the urban-reared worker, product of the industrial cities of Japan and subject to the impact of the rationalization and impersonalization of urban life, will score higher on the questionnaire, being more nearly autonomous in his attitudes toward traditional relationships.

To test this hypothesis, the total sample was divided into two groups: all respondents born in villages and all children of farmers—together comprising about 42 per cent of the total—and all born in towns and cities whose fathers were in "urban" occupations. While the mean score for the urban group is slightly higher than that of the rural, the difference is not greater than might be expected by chance. The data do not support the hypothesis.

Since the scores have been shown to be related to education and differences in educational level between the two groups might obscure the results of this rural-urban comparison, a further division of the groups was undertaken. It would be expected that better-educated, urban-reared workers would score higher on the scale than better-educated, rural-reared workers. Middle-school-educated, urban-reared workers would be

expected to have a higher mean score than middle-school-educated, rural-reared workers.

Again the hypothesis is not supported. Better-educated workers do not differ significantly in mean score by social background. More important, the mean score for middle-school-educated rural workers (91.4) is significantly higher⁸ than is the mean score for middle-school-educated urban workers (87.9). That is, rejection of traditional interaction and of the traditional authorities is definitely more common among less-well-educated rural-reared workers than among their urban fellows. The relationship is therefore just the opposite of that predicted.

In one respect these results are a vindication of the typical recruitment policies of large firms in Japan, for at present workers are carefully separated by educational background in all large companies, laboring occupations being open only to workers with a minimum amount of formal education. The argument that higher formal education makes workers less suitable for the workshop and its job relationships is supported by these data.

Apart from this, however, there is a certain irony in the results. At present, Japanese management makes a considerable effort to recruit its so-called "permanent" factory employees⁹ from rural backgrounds. An explanation typically offered by management is that youths from the villages have more "stable natures." By this is meant largely what is reflected in low scores on the attitude scale: acceptance of traditional relationships, of paternalistic job relations, and a general attitude of passivity and subordination. From this study it would appear that the recruiting of laborers with rural backgrounds has the opposite effect, selecting those who fit the present factory system less well than even urban-reared workers.

⁸ *t* equals 2.21, significant beyond the .05 level when *n* equals 117.

⁹ Briefly defined, these are employees permanently in the company's employ who will not be discharged or laid off unless on retirement or request.

⁷ *t* equals 3.35, significant beyond the .01 level when *n* equals 175.

Both these policies of industrial management and the initial hypothesis of this study assume that the agricultural village of Japan is least affected by those changes—in family structure, ideology, world view, and general interaction patterns—that industrialization has wrought in the life-patterns of urban populations. Thus it is argued that the village-reared laborer will be least disaffected from traditional modes, beliefs, and behaviors. This may well be the case with those who remain in the village. If the sample of this study had included workers in the small village shops, agricultural workers, or sons and daughters who remained on the paternal landholdings, sharp and substantial differences between scores of the rural and the urban groups might well have been noted.

However, the persons in this study from rural backgrounds have not remained in the village: in their youth, at age fourteen, fifteen, or sixteen, they migrated to the cities or sought employment there through the government employment offices in the village. There is, then, it may be argued, a selection process that invalidates the hypothesis. The workers from rural backgrounds who are now in large factories are exactly those persons most likely, even required, to be in a state of rebellion against tradition.

In this view the comparison of middle-school graduates from rural backgrounds with middle-school graduates from urban backgrounds would eventuate as in the foregoing analysis. At issue in this sample, it would appear, is not the question of rural-urban differences in attitudes but the problem of the psychological prerequisites of social change and of the preconditions of mobility in members of a rural, peasant society.

The items on which rural-reared middle-school graduates exhibit more autonomous reactions than do urban-reared workers are listed here in the order of disagreement:

Young people sometimes get rebellious ideas but as they grow up they ought to get over them and settle down.

Whom a young person should marry can be decided best by the parents.

Family discussion is all right, but in the end it is the husband and father who must decide what is right.

No sane, decent person could ever think of hurting a close friend or relative.

It is only natural and right for each person to think his family is better than any other.

Each statement is a strong expression of the intimate and binding relations between family members and of the obligations of children to parents. These are, for the most part, the items most agreed with by the total group studied. Yet it is in responses to these items that differences between the rural, less-well-educated workers and a comparable group from urban backgrounds are most marked, with workers from rural backgrounds disagreeing most.

It is not possible to demonstrate here what appears a likely thesis; that the statements listed here are precisely those with which workers who remain in the village would agree strongly. For a youth who leaves the village, however, while "young people sometimes get rebellious ideas," they do not, or he does not, recover from them as they get older. They carry these ideas out, and it is this attitude that helps to make mobility to the city possible. So with arranged marriage, paternal decisions, and the disposition not to hurt friends and relatives.

The configuration of attitudes in response to this questionnaire seems then to provide some insight into the larger problem of social change and the psychological meaning of the striking cityward migrations of rural youth so conspicuous in Japan, suggesting a modification of the conception that urban living gives rise to the more impersonal and autonomous attitudes characteristic of industrial, non-agricultural societies. The worker who moves into the city and into industrial employment from the traditional villages of Japan seems from these data to be an autonomous and tradition-free individual, relative even to urban-reared workers. It is consistent to suppose that migra-

tion and the separation of the individual from traditional modes of occupation and group relations has as a precondition a psychological separation from traditional attitudes.

The position of younger and older sons.—This contrast between attitudes of rural and urban workers with less education and the greater autonomy of the rural-reared group poses the problem of an explanation. The development of autonomous attitudes among urban populations has been assigned to a number of factors, a few of which are the reduction of the family unit to the isolated conjugal family, the shift from ascribed to achieved status, the change from education at home to formal schooling, and the relatively impersonal basis of interaction in the urban, industrial milieu. None of these applies to the rural population whose shifts in attitude are found to be significantly greater than those of a comparable urban group.

In the individuals who migrate from a rural village the psychological prerequisite must be a capacity to maintain some measure of autonomous identity. In the relatively closely knit and well-defined system of relationships in the village, he is confronted with a limited range of personal decisions and choices in establishing his adult role. In a complex industrial and urban setting what he becomes is by virtue of the increased social differentiation and complexity of relationships more dependent on individual choice and decision. To be able to maintain himself in the city he must cut himself off, psychologically, from the more safe and less demanding village and develop a considerable capacity to maintain autonomy, to hold to behavior and attitudes different from and independent of those of his village. He must also perceive the possibility of occupational change and occupational differentiation. Whereas in all societies both social and psychological influences converge to maintain sons in occupational roles and status like the fathers', these migrants from villages, as prerequisite to the migration, seem to refuse to identify themselves with their fathers'

occupations and to drive themselves toward different occupations.

Although the thesis seems not yet to have been systematically developed, it might be argued that the role of the younger son in a rural family is functionally important. The rules of primogeniture are closely adhered to in rural Japan. (It is questionable whether Occupation-compelled legal changes have actually affected even the formal aspects of the rule, and they have almost certainly not changed the actual roles of the several sons in a family.) Under primogeniture the older son is trained to follow the father's occupation; he works with the father, learning the traditional techniques of the job, and he undergoes the training, both intellectual and emotional, for eventual assumption of the father's social and occupational role. The role of the older son is certainly father-dependent in these social respects, whatever the psychological interaction in a particular family may be.

The younger son is relatively less involved in this system. He will not succeed to the family property or to the role in the family for which the older son is being trained.¹⁰ There seems, then, to be among these younger sons the preconditions for discontinuity in occupation and in attitude. There is, further, the fact that, in a crowded and land-starved country like Japan, the younger son is free to move and is under some economic compulsion to move to an occupation different from his father's. This might not be sufficient to account for entrance into an industrial occupation but will reinforce the emotional concomitants.

This argument suggests a specific hy-

¹⁰ "The creation of a *bunke* (branch family) subordinate to the *honke* (main family) is possible only under certain closely interrelated moral, legal and economic conditions; these must be such as to make it generally understood that a young man cannot enter into the category of adult by his own efforts, that he needs help to begin with and that this will constitute an *on* (obligation) for him and will establish a relationship of hierarchical dependence. In a wage-earning economy, for instance, there is little likelihood of a younger son having to look to his father for help to start life on his own" (Stoetzel, *op. cit.*, pp. 58-59).

pothesis susceptible of examination with the data at hand: that the scores of younger sons from rural backgrounds will be significantly higher on the attitude scale than the scores of older sons from rural backgrounds.

Attitudes of younger and older sons.—Comparisons are possible of the attitudes of younger and older sons from rural backgrounds for twenty-three subjects who have older male siblings and of fifteen subjects who do not have older brothers. In view of the relationship between education and questionnaire scores, it is important to note that, while only 10 per cent of the subjects with older male siblings attended higher school, half of the subjects without older male siblings did so. Observed differences in attitudes in the direction of greater autonomy on the part of younger sons, then, are in spite of, rather than because of, the education-attitude relationship.

The mean total score for younger sons on the attitude scale is 95.7, while the mean total score for older sons from rural backgrounds is 85.9. The difference between the means is significant beyond the .05 level.¹¹ The hypothesis that younger sons from rural backgrounds will be less tradition-bound and more autonomous is supported by data.

Items differentiating the two groups, in rank order by the size of the difference in score between younger and older sons, follow.

Young people sometimes get rebellious ideas, but as they grow up they ought to get over them and settle down.

No sane, decent person could ever think of hurting a close friend or relative.

The emperor is more than a symbol of Japan

¹¹ *t* equals 2.48 between the .05 (2.03) and .01 (2.72) levels of significance when *n* equals 38.

and should be restored to his former position.

Family discussion is all right, but in the end it is the husband and father who must decide what is right.

There is hardly anything lower than a person who does not feel a great love, gratitude, and respect for his parents.

Obedience and respect for their parents are the most important virtues children can learn.

This is consistent with the foregoing statement of the role of the younger son who moves to the city. Special note might be made of the item on the emperor's position. It is probable that the role and power of the emperor are seen as extensions of the father role and that, as the authority of the father is rejected more strongly by these men than it is by the older sons, so the authority of the emperor is rejected sharply because they see it as part of the same complex of traditional authorities which they are escaping.

In discussing social change and the part played by individual motivation and attitudes in social change, there is some difficulty in bringing together psychological and sociological variables. From a consideration of the prerequisites of movement from the rural village of a peasant society to employment in an urban, industrial setting, it is suggested that the social and psychological role of the younger son where rules of primogeniture are adhered to may be an important condition of individual participation in social change, providing a paradigm for the analysis of some of the psychological factors in individual participation in social change. An examination of differences in attitude between Japanese workers from rural backgrounds who are older and younger sons supports the general argument.

MASSACHUSETTS INSTITUTE OF TECHNOLOGY

THE SOCIAL-DISTANCE PYRAMID: RELATIONSHIPS BETWEEN CASTE AND CLASS

FRANK R. WESTIE AND MARGARET L. WESTIE

ABSTRACT

Sociological theories of the relationship between social class and prejudice toward minorities are so incoherent and contradictory that clear-cut hypotheses as regards expected empirical relationships between class and prejudice cannot possibly emerge. The selection of one interpretation (of findings after the fact of investigation) from its diametric opposite can hardly be more than arbitrary. Two studies are presented here which are guided by a procedure which, it is hoped, avoids baseless hypotheses and the arbitrary selection of particular interpretations from a number of possibilities. The empirical findings, when graphically arrayed, took the form of a pyramid, which is here called "The social-distance Pyramid," showing the greatest social distance between Negroes and whites of low status and progressively less distance between those of higher status.

This article presents findings and interpretations of two studies in a larger research program¹ designed to establish empirical relationships between social class²

¹ Completed investigations in this program were subsidized by the Graduate School of Indiana University; present phases are being pursued under a Faculty Research Fellowship of the Social Science Research Council.

This is the second of two articles on the relationship between theory and research. The first describes a procedure designed to permit the investigator to pursue empirical research with some degree of guidance from theory despite a high degree of theoretical incoherence in the area of investigation (cf. F. R. Westie, "Toward Closer Relations between Theory and Research: A Procedure and An Example," *American Sociological Review*, XXII, No. 2 [April, 1957], 149-54).

² The term "social class" is used loosely to describe the groups of varying status studied. Actually, we studied categories of people classified according to an adapted form of W. L. Warner's Index of Status Characteristics (cf. W. L. Warner, M. Meeker, and K. Eels, *Social Class in America* [Chicago: Social Research Association, Inc., 1949], chap. viii). We would hold, however, that there is considerable correspondence between our socioeconomic status categories and what is usually meant by "social class," especially at the extremes of the range, at one end of which are people living in slums, at the other end in veritable mansions. For a detailed description of the classification technique, sampling procedures, scale construction procedure, etc., see Frank R. Westie, "Negro-White Status Differentials and Social Distance," *American Sociological Review*, XVII (October, 1952), 550-58. Sampling was by socioeconomic status areas and, in the case of both the study of Negroes and the study of whites, included adult males only—174 whites and 92 Negroes (cf. Frank R. Westie and David H. Howard, "Social

and prejudice. Its primary purpose is to narrow the extremely broad range of theoretical propositions concerning class and prejudice.

In the first study, persons in three randomly drawn samples of the Indianapolis population from each of three socioeconomic levels were interviewed regarding the degree of social distance they would maintain between themselves and Negroes varying status in the Negro community. In the second study, Negroes of various socioeconomic status were interviewed regarding the distance they would prefer between themselves and whites in various positions in the white community.³

The diagram in Figure 1, a "social-distance pyramid,"⁴ is reminiscent of diagrams

Status Differentials and the Race Attitude of Negroes," *American Sociological Review*, XIX [October, 1954], 584-91).

³ The primary instrument is a 500-plus item booklet which contains four social distance scales which purport to measure social distance in four areas of interaction: (I) residential, (II) position (extent to which subject would be willing to have attitude-object occupy positions of power and prestige), (III) interpersonal (distance preferred in acquaintance, friendship, etc.), and (IV) physical (degree of "physical aversion"). Combined, these scales yielded a test-retest reliability coefficient of .96. In the present paper, attention is focused on the *reciprocal* relationships of Negroes and whites.

⁴ Most of the relationships depicted in Fig. 1 meet accepted standards of statistical significance. For the tabular data from which the diagrams are derived, see the articles listed in n. 2; for the measurement techniques, see Frank R. Westie, "A

SOCIAL DISTANCE PYRAMID

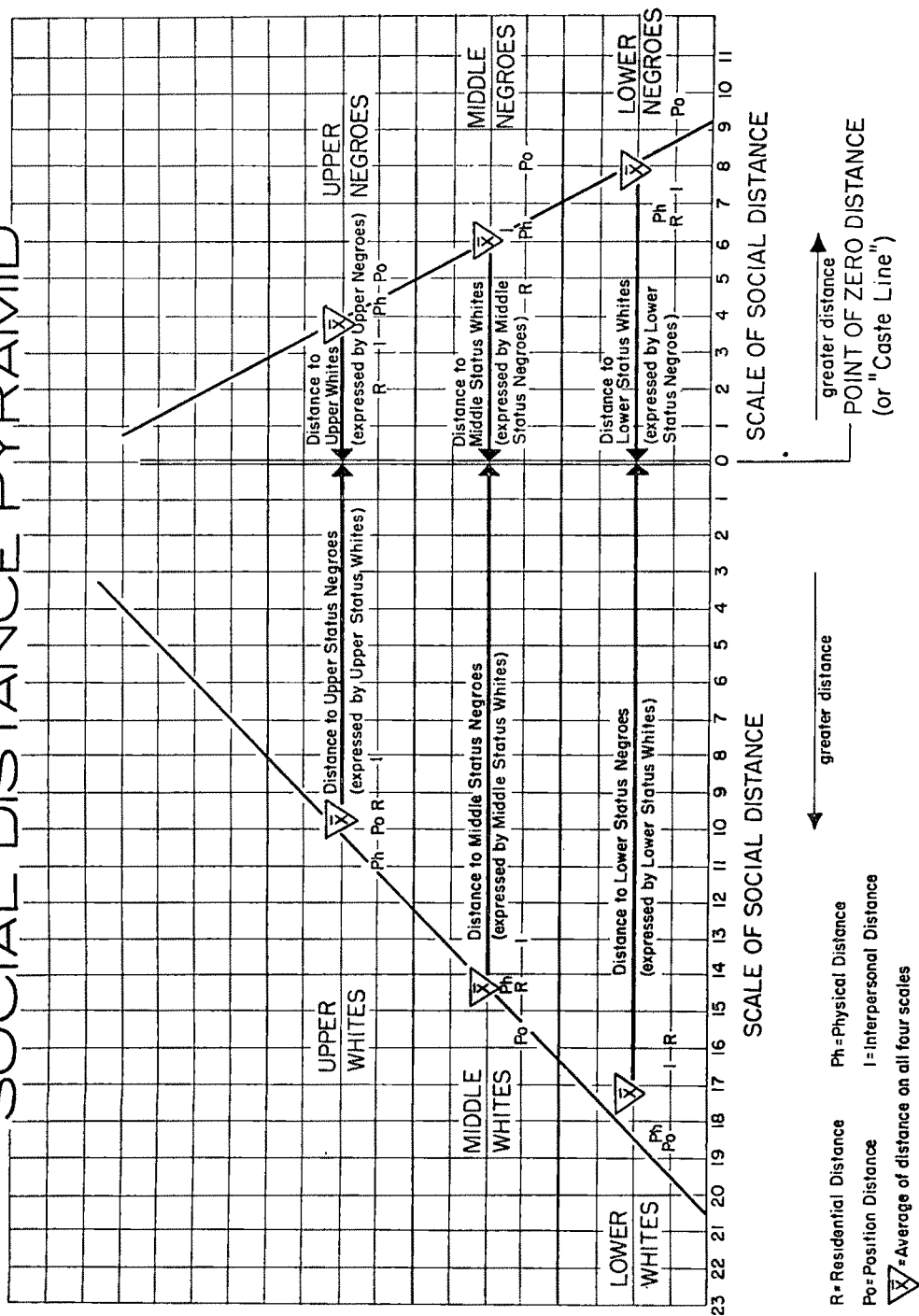


FIG. 1.—Social-distance measures derived through analysis of scale responses of whites to Negroes (*left-hand side of diagram*) and Negroes to whites (*right-hand side of diagram*). Respondents are classified according to occupational rank.

constructed by W. L. Warner, Gunnar Myrdal, and others to illustrate the relationship between caste⁵ and class in America. The diagram presented here, however, differs from the previous diagrams of this kind, in that it represents a system of relationships empirically established rather than hypothesized.

The social-distance pyramid depicts the degree of social distance preferred by persons of three socioeconomic levels of their own community in relation to persons on approximately the same level in another community. The left-hand side of the pyramid represents the white community; the right hand represents the Negro community. They are separated by the "caste line," which is an extension of the "point of zero distance" on the scale of social distance represented by the graduated line which forms the base of the pyramid.

As one moves away from the caste line, to right or left, distance increases. The inverted triangles representing the particular classes within each community are placed either closer or farther from the caste line according to the distance that respondents of the particular status preferred to maintain between themselves and persons on equivalent levels in the racial outgroup.

The chart was constructed by analyzing the responses of persons of a given socioeconomic status category on one side of the caste line to persons of equivalent socioeconomic status on the other side of the caste line. Thus, for example, the arrow extending to the right from the high-status whites represents the distance the latter pre-

ferred to maintain between themselves and upper-status Negroes. Each arrow represents the distance that persons of a given status preferred to maintain between themselves and those of the same status in the racial outgroup.

The caste line and the point of zero social distance are equated here because it follows that where there is no social distance between Negro and white the caste line ceases to exist. When a Negro is confronted by the caste line, he is in reality up against situations which require the maintenance of certain traditional social distances between Negroes and whites. Perhaps the system is most threatened in the minds of whites when the distance between Negroes and whites approaches the zero point. When a person socially defined as Negro "passes," he "jumps the caste line" or, stated otherwise, goes beyond the point of zero distance.

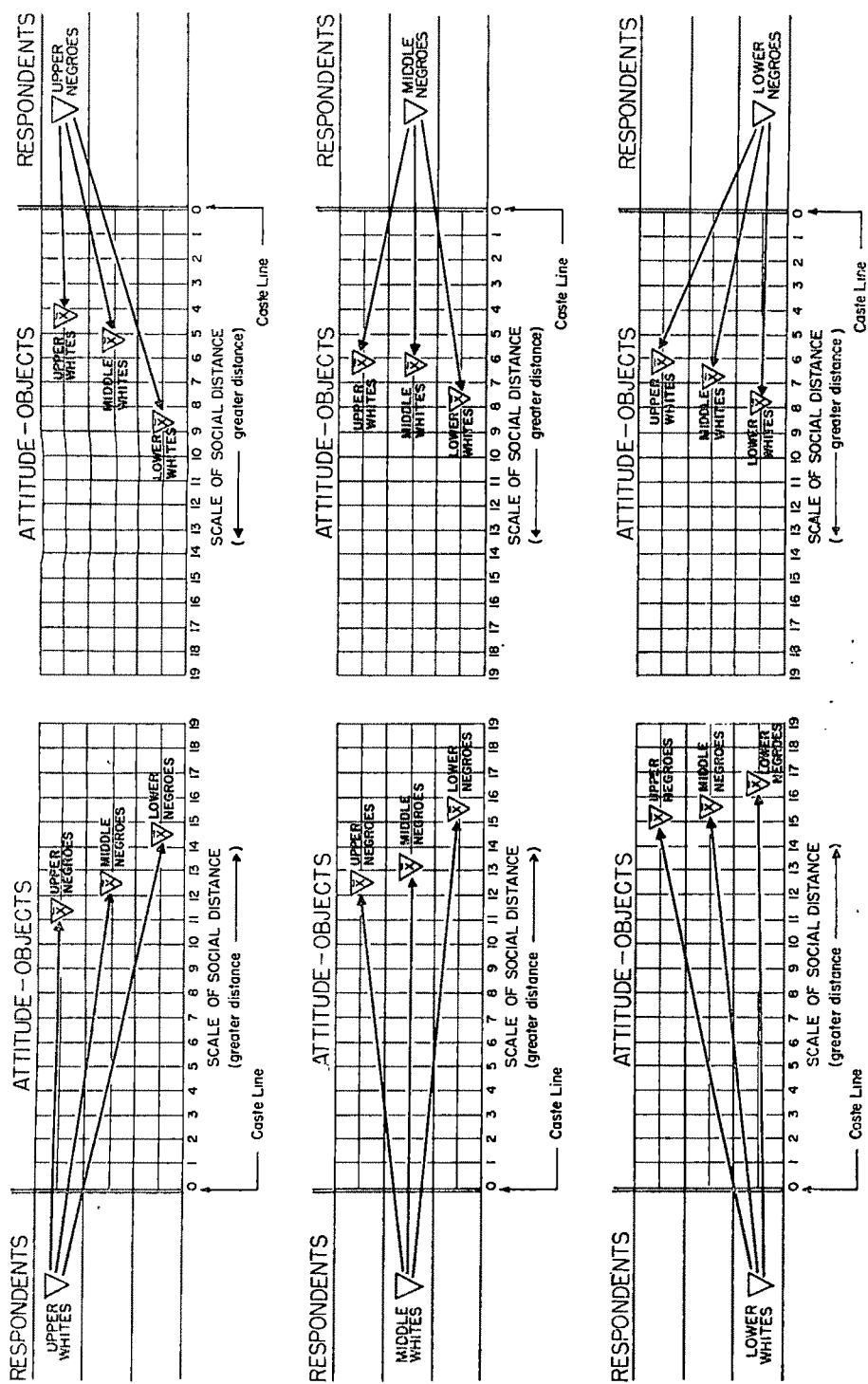
The relationships represented in Figure 1 may be summarized as follows: (1) upper-status whites would maintain least distance between themselves and high-status Negroes, while, reciprocally, high-status Negroes would maintain least distance between themselves and whites of similar position; (2) lower-status whites would maintain greatest distance between themselves and Negroes of low status, while Negroes of lower status would maintain greatest distance between themselves and whites of low status; (3) in all three of the relationships between "equivalent" classes, there is a social distance differential,⁶ i.e., at any given level the distance preferred by whites from Negroes is greater than that preferred by Negroes between themselves and whites.

The diagrams in Figure 2 depict the relationship of each class on one side of the caste line to all classes on the opposite side. These relationships may be summarized as follows: (1) The higher the status of the Negro as attitude-object, the less the distance preferred by whites of all classes. Similarly, the higher the status of the white as attitude-object, the less the distance preferred by Negroes of

Technique for the Measurement of Race Attitudes," *American Sociological Review*, XVIII (February, 1953), 73-78. The socioeconomic classifications of our respondents, in both the white and the Negro studies, were made according to Warner's seven-point scales for ranking occupation, source of income, house type, dwelling area, and education (cf. Warner, Meeker, and Eels, *op. cit.*).

⁵ The term "caste" is used here as a mere verbal convenience to describe the two groups studied, whose status in relation to each other is fixed almost immutably by biological ancestry. We do not equate the caste system of India with the institutionalized relations between American Negroes and whites.

⁶ Cf. E. S. Bogardus, "Social Distance Differential," *Sociology and Social Research*, XXXII (May-June, 1948), 882.



▽ Average of distance on four scales

FIG. 2.—Social distance between each class on one side of the caste line and all classes on the opposite side. Respondents are classified according to socioeconomic status of residential area.

all classes. (2) The higher the socioeconomic status of the responding white, the greater the alteration of response with variations in the status of the Negro. Similarly, the higher the status of the responding Negro, the greater the alteration of response with variations in the status of the white as attitude-object.

Sociological theory dealing with the relationship between social class and prejudice toward minorities is so incoherent and contradictory that clear-cut hypotheses, as regards expected empirical relationships between class and prejudice, cannot possibly emerge. In the face of such theoretical contradiction the selection of one interpretation (of findings, after the fact of investigation) from its diametric opposite can hardly be more than arbitrary. The program of which the studies reported here are a part seeks to avoid the formulation of theoretically baseless hypotheses and the arbitrary selection of particular interpretations from a number of possibilities.

This procedure involves (a) explicitly listing a comprehensive range of "presupposed empirical relationships," many of them diametrically opposed to one another, which might possibly turn up in the research at hand and (b) explicitly listing a range of interpretations, many of them diametrically opposed to one another, for each possible empirical finding. Then, through investigation, the relationships which actually obtain are selected from the system of presupposed empirical relationships listed; all other initially proposed empirical relationships are discarded. The array of alternative interpretations attached to each of the presupposed relationships in the original presentation are also eliminated from consideration as interpretations of the findings.

In the present paper, space permits discussion only of those interpretations of empirical relations which survived the test, and in the interest of economy we have chosen to examine those of the differences in prejudice between upper and lower class, giving less consideration to the question "Why does the middle class lie in between?" Possible interpretations, worthy of further research, are

grouped under four headings under which the larger portion of them conveniently falls.⁷

Psychological insecurity.—The upper-class persons feel secure because they are probably less recently mobile and, being at the top, are not likely to strive as hard as those in the intermediary ranks. In their greater security they have less need to use the Negro as a scapegoat to justify their social and economic failures; they do not need a negatively valued Negro group to enhance their own status; and, being less likely to be frustrated in their designs for living (largely status-related in America), they do not need the Negro as an object of aggression. Moreover, whatever their aggressions, if any, they can be vented upon persons of lower status within the majority group.

Ambiguity enters the picture, however, when certain related, though contradictory, interpretations are considered: It may be that the lower-class person, because he is relatively the *least* mobile in the local community, has *greater* security, not less, as to class status than have persons of high status. The lower-class person need have no fear of slipping down the ladder, for, being on the bottom rung, he has no place to fall. Thus it is the lower-class person who should be *least* prejudiced if status insecurity is the explanation. Moreover, the middle class is less prejudiced than the lower class, despite a wealth of evidence that points to the middle class as the most mobile and the most insecure. Furthermore, if prejudice is to be "explained" in terms of psychological insecurity, then there is reason to believe that upper-class Negroes should be *more* prejudiced against whites, not less, than persons on lower levels of the Negro community. It may very well be true, as is often suspected,

⁷ Most of these interpretations are to be found in the sociological literature, some based upon empirical research. More often, however, they are untested assumptions on a level of abstraction considerably higher than that of the present discussion. This is not intended as an exhaustive listing of the surviving interpretations: e.g., interpretations in terms of differentials in economic security are not given the separate treatment they deserve, though considered indirectly under the heading "Competition."

that persons on the upper level of the Negro community are most frustrated in their designs for living. The upper-class Negro's success in climbing the socioeconomic ladder is compromised and seriously limited by the caste line. The arbitrary and absolute nature of the dictum that "a Negro is a Negro regardless of his individual accomplishments" is probably better and more bitterly known to upper-class Negroes than to any others. Yet these are the people who manifest the least degree of social distance of all groups in the social-distance pyramid. One might argue that their preference for less social distance is simply the next step in their social mobility. It happens, however, that they also manifest less prejudice, as indicated by other measures.⁸ Moreover, they allege that they would insist on less distance between themselves and lower-class whites as well as between themselves and more highly placed whites than does any other Negro group.

Education.—According to these interpretations, the upper-class person is better educated than those below him, and education is conducive to a more liberal outlook in general. Moreover, the educated are less likely to indorse the myths that sustain the prevailing system of race relations. In the case of the attitudes of Negroes whose status is high in the Negro community, the argument of education may take the following form: Education is one of the most important criteria of class in the Negro community; thus upper-class Negroes are conspicuously better educated than Negroes below them. Educated Negroes are aware of the Negro's stake in being free of prejudice: he scores lowest on prejudice against whites for the same reason that educated Jews, relative to educated persons of other religious affiliations, score lowest on scales measuring prejudice toward Negroes. The upper-class Negro realizes he cannot afford to be prejudiced.

⁸ Although a distinction can be made between social distance and prejudice, we have chosen to use the terms interchangeably, inasmuch as the distinction makes little difference in the problem at hand.

Conformity.—Some sociological theories suggest that the upper class expects greater conformity from its members than does the lower class to certain social norms significant for American race relations. If so, then (a) the middle- or upper-class person is more thoroughly indoctrinated with the value system implied by the term the "American Creed" and has internalized these values to a greater extent than has the lower, and (b) whether or not he has internalized these to an appreciable extent, he at least is more likely than the lower-class person to create that impression, because (c) the upper class has a more rigid system of controls over the individual (one of the most important of which is withdrawal of status) to guarantee conformity.

But the question soon comes to mind: Doesn't the upper class insist on and guarantee conformity to the social norms which require maintenance of social distance between Negroes and whites? Indeed, to fail to maintain distance might be regarded as disreputable, and this in a milieu where respectability is a primary value. Actually, persons on virtually all levels above the very lowest probably feel constrained to conform at least ostensibly to the egalitarian values of the "American Creed" and simultaneously to the discriminatory norms in the relations between whites and Negroes. One project in the present program is designed precisely to test conformity. Our pretest findings suggest that the classes *do* differ in the degree to which the general values subsumed under the "American Creed" are consciously verbalized and also in the degree to which they recognize the conflict between their expressed ideology and their discriminatory attitudes.

Conformity as an interpretation, if valid, would explain why the lower-class person expresses more prejudice than those above him. It does nothing, however, toward accounting for the difference in prejudice between persons of highest status and those of intermediary position. Assuming the middle-class person to be the greatest conformist, it is quite possible that his intermediary opinions result from his unwillingness, on

the one hand, to express his prejudices⁹ as frankly as does the lower-class person (hence he gets a lower prejudice score than does the lower-class person), while, on the other hand, he is unwilling, also because of his conformism, to deviate from the path of respectability when it comes to indorsing social-distance items that represent extreme departures from the status quo. In the pre-text of a study on conflict in values we found many lower-class persons who "don't want to have *anything* to do with 'em" (Negroes), while we often find middle-class persons who would be willing to have a Negro as a "close personal friend" but who, "of course," would not want to have Negroes "live in my neighborhood." It may be this kind of inconsistency that puts the middle-class person "in the middle" as to prejudice.

Competition.—Prejudice may be seen as a function of competition for status-related values, both economic and non-economic, both types of which are defined in the society as unable to be shared. Competition is most intense at the lower levels of both the white and the Negro hierarchy because the scarcity of these values is most acute here and competition between Negroes and whites is most direct. In Indianapolis, for example, Negroes and whites often compete for the same jobs, the same neighborhoods, the same houses, and use of the same public facilities. Face-to-face contacts on the lower level take place under circumstances conducive to definitions of one another as competitors.

If the foregoing interpretations of lower-class attitudes are to any extent correct, it follows that the upper classes of both white and Negro communities are less prejudiced against one another than are those of the

lower class because competition is less intense, the scarcity of the objects of competition being less and competition, if it prevails at all, being highly indirect. Actually, white and Negro white-collar workers, businessmen, and professional persons are only rarely in competition with one another. Negroes rarely occupy white-collar positions in white business enterprises, although they do, of course, occupy such positions in larger Negro businesses. White and Negro businessmen typically do not compete for the same customers.

The general empirical relationship represented in Figure 2—that social distance decreases as the status of the attitude-object increases—is also consistent with the interpretations based on competition. As the status of the Negro as attitude-object increases, he is further removed from competition with all categories of whites. This is especially true, however, of the relationship between lower-class whites and Negroes.

We are not sure which of these interpretations is correct, nor have we any idea how the ability to explain one compares with that of another. Moreover, we have no idea how particular interpretive variables (if valid) interact with other variables to produce the empirical outcome described by the pyramid. We hope eventually to establish the degree and nature of the connections between those which prove to have some degree of validity. No one of these interpretations or sets of interpretations can adequately explain all the findings.

Even though the primary question raised in this paper is a theoretical one—"What is the significance of findings for sociological knowledge on a higher level of abstraction?"—our knowledge on this higher level of abstraction is so tenuous that the problem remains an empirical one: How, specifically, do classes in the local community differ as to insecurity of status, economic insecurity, conformity, "inner" directedness, and how are these differentials related to the differentials we have found?

⁹ We are also pursuing studies of the relationship between (a) certain autonomic physiological responses (galvanic skin response and certain electrocardiographic measures) as manifested in race relations situations and (b) race attitudes as indicated by the devices employed in the projects reported in the present paper. These physiological studies may tell, among other things, something about differential degrees of equanimity of response to race relations items.

MALE AGGRESSION IN DATING-COURTSHIP RELATIONS

EUGENE J. KANIN

ABSTRACT

Approximately 62 per cent of a group of university Freshmen women reported experiencing offensive male sexual aggression during the year prior to university entrance. Proneness to sexual aggression appears associated with a lack of parental sex guidance and the absence of older male siblings. Certain characteristics of the pair relationship also increase the probability of aggression. The adjustive reactions of the offended female are strongly influenced by prior parental guidance and the severity and provocation of the aggressive episode.

This paper reports an investigation of male sex aggression in dating-courtship relationships with high-school girls. The particular type of pair relationship, characterized by male sex aggression, was recently studied on a university campus where 55.7 per cent of the female respondents reported having been involved in an offensively aggressive episode during the academic year.¹

The current study is partly an attempt to test hypotheses suggested by the earlier investigation, hypotheses concerned mainly with the protective influence of the family, the provocation of the aggressive episodes, and situational factors, such as the influence of alcohol and the site of occurrence. The question also arose as to whether the prevalence and patterning of such episodes as reported by university women could be observed at the precollege level. First-semester Freshmen university women were requested to report offensively aggressive episodes experienced during their high-school Senior year and the summer preceding university entrance.

A six-page schedule was distributed to

ten university classes in sociology and English during September and October, 1956. Information was requested concerning aggression at five levels of erotic intimacy: attempts at necking (N), petting above the waist (Pa), petting below the waist (Pb), sexual intercourse (Ia), and a more violent attempt at sexual intercourse accompanied by "menacing threats or coercive infliction of physical pain" (Iv). At all levels the respondents were asked to report only such forceful attempts which they considered "offensive and displeasing." No inquiry was made into the degree of success attained by these forceful endeavors.

One hundred and eighty anonymous schedules were distributed and received in the ten classes, the male members having been dismissed. After completing the schedules, the respondents were asked whether they had one close friend on campus, also a Freshman, who they thought would be cooperative in filling out and returning a schedule in a self-addressed stamped envelope. The percentage of schedules returned in this fashion was high: 118 schedules were distributed, and 97 (82.2 per cent) were received. The returns of this two-staged sample were lumped together, since they showed similarity of response. Of the 277 schedules received, 15 were discarded as incomplete, the remaining 262 were completely analyzed.

Of these 262 usable schedules, 163 (62.2 per cent) reported offensive episodes at some level of erotic intimacy during the Senior year of high school and the summer preceding college entrance. An analysis of the

¹ Clifford Kirkpatrick and Eugene Kanin, "Male Sex Aggression on a University Campus," *American Sociological Review*, XXII (February, 1957), 52-58. Other evidence concerning the incidence of male sex aggression of various types may be found in Alfred Kinsey *et al.*, *Sexual Behavior in the Human Female* (Philadelphia: W. B. Saunders Co., 1953), pp. 116-20; T. Landis Judson, "The Nondelinquent Child and the Sexual Deviate," *Research Studies of the State College of Washington*, XXIII (March, 1955), 95; G. V. Hamilton, *A Research in Marriage* (New York: Boni, 1925), pp. 334-41; B. Apfelberg *et al.*, "A Psychiatric Study of 250 Sex Offenders," *American Journal of Psychiatry*, C (1944), 762-70.

distribution of offended girls shows that a considerable number of respondents experienced the more severe degrees of aggression (see Table 1).

The offended girls were asked to specify in which of the four time groupings, each covering three months, e.g., September-October-November, etc., the episodes occurred. A seasonal trend was readily observed. A gradual but consistent increase from the fall of 1955 to a high peak in the summer of 1956 was noted for all levels of erotic intimacy. A 3×4 table² showed that episodes of greater offensiveness, attempts at intercourse, and attempts at intercourse accompanied by violence were

• TABLE 1

OFFENDED GIRLS BY MAXIMUM
OF EROTIC INTIMACY

Maximum Level	No.	Per Cent
Necking.....	39	23.9
Petting above the waist....	54	33.1
Petting below the waist....	22	13.5
Attempted intercourse.....	34	20.9
Attempted intercourse with violence.....	14	8.6
Total.....	163	100.0

more heavily concentrated in the spring and particularly in the summer months, when over half of them occurred ($\chi^2 = 13.68$; d.f. = 6; $.05 > P > .02$). The spring and summer months, of course, bring on an increase in social contacts and hence can be thought of as providing a greater exposure to aggressive experience. Moreover, these high-school Seniors, approaching university entrance, have probably also increased their dating, especially with older, college males.

CHARACTERISTICS OF THE OFFENDED

The differential characteristics of the offended and non-offended girls were compared. Being first-semester Freshmen, the

² The levels of erotic intimacy were telescoped into three categories—(1) necking and petting above the waist, (2) petting below the waist, and (3) attempted intercourse and attempted intercourse with violence—in order to satisfy requirements for chi-square.

university women constitute a rather homogeneous group with respect to academic class and age, mean age being 17.8 years. Dating frequency can be hypothesized as a variable influencing susceptibility to erotic aggression. The offended girls reported a mean dating frequency for the month previous to the interview of 12.51 compared to 11.25 for the others (C.R. = 1.0). Although the mean number of dates per month rose with increasingly offensive levels of aggression ($N = 11.5$, $P_a = 12.2$, $P_b = 12.8$, $I_a = 13.5$, $I_v = 13.8$), the relationship between dating frequency and the maximum level of aggression gave an eta of only .12. These data indicate that dating frequency is not a significant variable affecting proneness to offensive aggression.

In addition to dating frequency, it was hypothesized that perhaps the number of different men dated per month would be closely associated with differential proneness to offensive experience. Replying to a question concerning total number of men dated in the previous month, the mean figure for the offended is 5.17 in contrast to 4.65 for the non-offended (C.R. = .4). The mean number of men dated, according to the maximum level of aggression, revealed no consistent pattern. That the number of different men dated should not be associated with proneness to offensive aggression is explained by the fact that 44 per cent of the episodes are associated with the more permanent date relationships of the "regular-date" and "pinned" variety. Girls involved in the more monogamous pairings would naturally not report dates with a great number of different men.

With respect to church attendance, a classification of the offended and non-offended according to regular attendance failed to yield a significant chi-square ($\chi^2 = .824$; d.f. = 1; $.50 > P > .30$). These data would not lend support to the notion that religious participation acts against erotic exploitation.

The family can be expected not only to instill in the young female a conscience sensitive to the moral sentiments of a society but

also to provide information useful in checking or thwarting exploitation. Two such protective influences in the family can be investigated: parents and older siblings. The presence of older siblings may be thought of as associated with a differential proneness to erotic aggression while dating. Older brothers may provide insight and clues into the male culture and thus render the younger female more alert to erotic aggression and the tactics of exploitation. On the other hand, younger girls, if confided in, could well profit from the experiences which older sisters have encountered in the course of their dating. The presence of an older brother was found to be significantly associated with not being offended ($\chi^2 = 3.90$; d.f. = 1; $.05 > P > .02$). Of the offended girls, 24.5 per cent indicated that they had an older male sibling as compared to 35.4 per cent of the others. On the other hand, the presence of an older female sibling failed to distinguish the offended ($\chi^2 = .218$; d.f. = 1; $.70 > P > .50$).

Older brothers may deliberately instruct and admonish their younger sisters about male sex exploitation, or else older brothers may, without conscious intent on their part, give the female an added insight and knowledge into the male subculture and its expectations in dating behavior. The implication drawn here is that older brothers may be responsible for implanting more caution in their younger sisters. There is, however, the possibility that the presence of an older male sibling means greater understanding of male expectations and consequently an increased tolerance of male aggressiveness, rendering the girls less prone to report such episodes as offensive.

Another aspect of primary group influence was probed—the effect of specific parental warnings. It can be hypothesized that girls who have been warned and instructed by their parents would experience fewer aggressive episodes. A breakdown of the responses showed that 87.9 per cent of the non-offended and 76.7 per cent of the offended reported being admonished ($\chi^2 = 5.14$; d.f. = 1; $.05 > P > .02$).

PAIR RELATIONSHIPS OF OFFENDED AND NON-OFFENDED GIRLS

Conditions associated with sexual exploitation, such as greater age, intelligence, and social status, are usually thought of as giving added advantage to the exploiting member of the dating pair. Ehrmann's studies indicate male sex exploitation to be greater with females of a lower status.³ In Table 2 the responses of offended girls con-

TABLE 2

CHARACTERISTICS OF AGGRESSIVE AND NON-
AGGRESSIVE MALES AS REPORTED BY OF-
FENDED AND NON-OFFENDED FEMALES

Characteristics	Aggres- sive Males (Per cent)	Non- aggressive Males (Per Cent)	C.R.
<i>Comparative age:</i>			
Younger.....	6.22	2.69	2.3
Same.....	44.86	74.73	8.7
3-5 years older.....	43.24	21.24	6.6
Over 5 years older...	5.68	1.34	2.5
Total.....	100.00	100.00	
No.....	370	372	
<i>Comparative intelligence:</i>			
Inferior.....	14.05	5.91	3.7
Same.....	60.81	77.96	5.2
Superior.....	25.14	16.13	3.1
Total.....	100.00	100.00	
No.....	370	372	
<i>Comparative social status:</i>			
Lower.....	18.64	4.03	6.5
Same.....	65.68	77.96	3.8
Higher.....	15.68	18.01	0.8
Total.....	100.00	100.00	...
No.....	370	372	

cerning the comparative age, intelligence, and socioeconomic status of aggressors are contrasted with similar data presented by the non-offended for the men they dated during the month preceding this study.

The evidence (Table 2) is that the offenders tend to be older than the offended, significantly more so than the men dated by the non-offended girls, and that the aggres-

³ Winston W. Ehrmann, "Non-Conformance of Male and Female Reports on Pre-marital Coitus," *Social Problems*, I (April, 1954), 155-59; "Influence of Comparative Social Class of Companion upon Pre-marital Heterosexual Behavior," *Marriage and Family Living*, XVII (February, 1955), 48-53.

sive males are of superior and inferior intelligence to a greater extent than are the companions of the non-offended. Although the unaggressive males are largely reported to be of higher status, a larger proportion of aggressive males are ascribed a lower comparative status. In general, there is a convergence of evidence indicating heterogamy to be more characteristic of the offender-offended relationships and homogamy to be distinctive of the aggression-free pairs. The tendency for offended girls to ascribe inferior intelligence and lower social status to their aggressors may be partly a result of the offensive experience. However, it is reasonable

non-offended girls, and it cannot be determined whether or not this largely heterogamous dating pattern is a reflection of "normal" dating relationships. However, these data further support the foregoing findings that heterogamy characterizes offensive aggression.

The investigation of another characteristic of the offender-offended pairs—relationship involvement—found an association existing between date relationship and degree of aggression.

Aggressions at the more advanced levels of erotic intimacy are largely associated with the more durable and involved rela-

TABLE 3
DATE RELATIONSHIP AND EROTIC INTIMACY LEVEL AT WHICH
OFFENSIVENESS OCCURRED BY EPISODES*

DATE RELATIONSHIP	NECKING AND PETTING ABOVE THE WAIST		PETTING BELOW THE WAIST		ATTEMPTED INTERCOURSE AND ATTEMPTED INTERCOURSE WITH VIOLENCE		TOTAL	
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent
First date.....	146	30.4	14	15.6	12	13.8	172	26.2
Occasional date	148	30.8	24	26.7	23	26.4	195	29.7
Regular date, "pinned," and engaged†.....	186	38.8	52	57.7	52	59.8	290	44.1
Total.....	480	100.0	90	100.0	87	100.0	657	100.0

* $\chi^2=23.00$; d.f. =4; $P < .001$.

† The only four episodes reported under the engaged category are to be found in the attempted intercourse and attempted intercourse with violence cell.

to assume that girls in such heterogamous pairings may be more prone to erotic aggression. Differences in social class and intelligence readily impede communication. Certain female mannerisms may be misinterpreted as inviting sexual advances. Furthermore, these females may be viewed as undesirable and/or unavailable marriage partners and hence likely targets for aggression.

Other evidence concerning the characteristics of date pairings where offensive episodes occur can be obtained by examining the academic class membership of the male offenders. Of the 370 offending males, 6.2 per cent were in a lower high-school class, 31.9 per cent were high-school Seniors, 45.4 per cent were college students, and 16.5 per cent were not students at all. Unfortunately, comparable data are not available from the

relationships, such as "regular date," "pinned," and engaged (Table 3). These findings somewhat contradict the popular notion that continuous dating with one man offers security against offensive erotic aggression. Initial dating ventures, characterized by an absence of shared understandings and poor communication, are usually thought to contain the bulk of such behavior. In popular lore "blind dates" and first dates are frequently portrayed as veritable wrestling contests.

The effect of these episodes upon the more involved relationships cannot be definitely determined. Whether these pairings are terminated as a result of offensive aggression or whether a shared understanding develops is not known. Regardless of the outcome, it is known that little repetition of

offensive behavior is tolerated. A tolerance ratio, defined as the number of episodes divided by the number of men involved at a particular level of aggression, shows the offended not likely to tolerate repetitions of aggression at any level, regardless of relationship. Perhaps these young respondents are more prone than older girls to report male aggression, since the pairings are further removed from marriage and hence lack the intense emotional involvement which may foster cumulative exploitation.⁴ With respect to the levels of erotic intimacy, the tolerance ratios are $N = 1.4$, $Pa = 1.4$, $Pb = 1.3$, $Ia = 1.5$, $Iv = 1.1$.

PROVOCATION OF EROTIC AGGRESSION

Pertinent to any discussion of erotic aggression is the role played by the female in the provocation of the offensive episode. To some extent, both the male and the female subcultures contain the notion that sex aggression is somehow the "fault" of the female; in short, girls frequently report that they "let it get out of hand," and men reply that "they [the girls] were asking for it." That there is misunderstanding and imperfect communication can be seen in the finding that only 75 per cent of the episodes beyond the level of necking were preceded by some erotic activity in which the female voluntarily engaged.⁵ Twenty-five per cent of the episodes, then, were to some degree abrupt sexual advances not preceded by consent to sex play. Undoubtedly, a substantial number of these advances are not provoked, as evidenced by the following excerpts:

After a few quiet walking dates we obtained a car and went for a ride. He stopped the car, and suddenly he was wild and raving. We struggled

and fought, and I screamed to no avail. After biting him on the back three times so that blood came through, I used my shoe. He then came to his senses in his pain . . . I can't understand our first quiet dates.

The boy that brought me home stopped outside my house and wouldn't let me get out of the car. I tried to leave, but he grabbed me and wouldn't let me go until I would kiss him. I struggled, but he forced me to kiss him while he ran his hand over me. Then he let me go.

Offensive necking episodes were not considered as preceded by consent to some form of erotic play, and, if added to the above-mentioned unprovoked episodes, then 58 per cent of the total number of reported offensive episodes were not stimulated by prior sexual play to which the female consented. Perhaps playing an important role in stimulating such abrupt male aggression is the image of female responsiveness held by the male. Such imagery may be influenced by the stereotype of the moving picture and pulp-magazine heroine being forcefully embraced and kissed, momentarily resisting but soon to melt in the hero's arms and become overly receptive. The role of such media in providing imagery of this sort needs investigation. The male subculture may be further instrumental in bringing about seemingly unprovoked aggression by perpetuating myths which associate various physical qualities, such as well-developed breasts and hips, with female responsiveness.

Of the 372 reported episodes beyond the level of necking, 278 were preceded by consent to lesser erotic activity, and for 227 of these episodes it was possible to determine the relationship. Abrupt male sex aggression is largely concentrated among the more casual date relationships, and aggressions succeeding some previous form of mutually acceptable sex play to be associated with the more durable and involved date pairings (Table 4). The evidence suggests that among the less involved dating pairs offensive behavior may be a consequence largely of poor communication, the male's beliefs concerning female responsiveness, and other factors,

⁴ Greater tolerance was reported by university women (see Kirkpatrick and Kanin, *op. cit.*, p. 55).

⁵ Abrupt sexual aggression was not equally distributed throughout the various levels of erotic intimacy. The percentage of offensive episodes not anteceded by consented sex play are: petting above the waist, 33.3 per cent; petting below the waist, 13.3 per cent; attempted intercourse, 16.9 per cent; attempted intercourse with violence, 31.2 per cent.

such as incorrect information about the dating partner,⁶ which lead to a faulty definition of the situation. Offensiveness reported occurring among the more serious pair relationships, on the other hand, indicates exploitation resulting from emotional involvement.

SITUATIONAL FACTORS

Loss of inhibitions due to the consumption of alcohol could help explain some of these unprovoked aggressions. The following excerpt is typical of the more severe unprovoked attempts at coitus where the male was reported as intoxicated:

The boy was drunk at the time, and, though I had dated him for several months, he had never even touched me before. That was why

sodes, and in 25.0 per cent of the attempted intercourse with violence episodes. In those dates where offensive aggressions occurred, alcohol appears to have contributed to the probability that aggression would occur at a more advanced stage of erotic intimacy.

The role of the automobile in providing seclusion and escape from social controls is well evidenced by these data, which show that 69.3 per cent of the episodes were experienced in an automobile. Of the remaining episodes, 18.1 per cent occurred in the female's home, 4.7 per cent in the male's home, 3.0 per cent out-of-doors, 2.9 per cent in a friend's home, and 2.0 per cent in other places. Two trends may be noted: first, it appears that, as the degree of offensiveness increases, the popularity of the female's

TABLE 4

SEX PLAY PRIOR TO OFFENSIVE EPISODE AND DATE RELATIONSHIP, BY EPISODES

DATE RELATIONSHIP	NO PRIOR SEX PLAY		SOME PRIOR SEX PLAY		C.R.
	No.	Per Cent	No.	Per Cent	
First date.....	27	50.0	27	11.9	5.3
Occasional date.....	15	27.8	56	24.7	.5
Regular date, "pinned," engaged....	12	22.2	144	63.4	6.3
Total.....	54	100.0	227	100.0	

it was quite a surprise when he tried to force intercourse on me. . . . I couldn't get completely away from him, but I did struggle enough so that he could get nowhere. He ripped my clothes and almost knocked me out against the car window, but finally he let me go, and I got out of the car and walked home.

It was found that 106, or 16.1 per cent, of the total episodes were reported to have taken place while the male was reported to have been "under the influence of alcohol." With respect to offensive aggression at the five levels of erotic intimacy, "under the influence" was reported in 16.1 per cent of the necking attempts, 17.0 per cent of the petting above the waist episodes, 18.8 per cent of the petting below the waist episodes, 21.0 per cent of the attempted intercourse epi-

home as the setting for the aggression decreases, since 19.6 per cent of the necking and petting above the waist episodes took place in the home of the girl, while only 10.3 per cent of the attempts at intercourse and attempts at intercourse with violence occurred there (C.R. = 2.5). There also appears to be a slightly increased preference for the male's home as the degree of aggression increases in similar fashion, 3.1 to 9.2 per cent (C.R. = 1.9).

REACTIONS OF OFFENDED

The responses of the offended to a check list of the possible reactions after the episodes were terminated could readily be grouped into four categories (Table 5). The most frequently mentioned adjustment is the termination of dating. Confiding in parents constitutes the least popular path for the offended girl, particularly if offensive episodes in the more advanced levels of inti-

⁶ Males anxious to safeguard their "reputations" are frequently known to impart to their fellows prodigious tales concerning the sexuality of a particular female.

macy were experienced. To some degree, parents are probably accountable for the offended girl's reluctance to rely on them for help and advice. Thoughtful parental guidance could well serve to reduce the guilt and stigma which deters the offended from seeking aid. Classifying episodes according to whether girls were recipients of parental admonitions show that those specifically warned about male aggressiveness were most likely to confide in their parents, 19 as com-

per cent (C.R. = 3.8). Episodes characterized by prior sex play are apparently tolerated to a greater extent because of a more intense emotional involvement. Only 26.8 per cent of these episodes led to termination of the pair relationship, while 60.6 per cent of the unprovoked episodes resulted in dissolution of the relationship (C.R. = 6.0). This suggests that the more involved pairs, characterized by offensive aggressions arising mainly from prior erotic activity, are not

TABLE 5
REACTIONS OF OFFENDED GIRLS

ADJUSTIVE REACTIONS	NECKING AND PETTING ABOVE THE WAIST		PETTING BELOW THE WAIST		ATTEMPTED INTERCOURSE BELOW AND ATTEMPTED INTERCOURSE WITH VIOLENCE		TOTAL	
	No.	Per Cent	No.	Per Cent	No.	Per Cent	No.	Per Cent
Terminated relationship..	159	30.8	27	28.1	36	36.4	222	31.2
Secrecy.....	79	15.3	49	51.0	36	36.4	164	23.0
Discussion and warning re- action re age group.....	180	34.8	16	16.7	17	17.1	213	29.9
Appeals to parents and other authorities.....	99	19.1	4	4.2	10*	10.1	113	15.9
Total.....	517	100.0	96	100.0	99	100.0	712	100.0

* Contains the only two episodes which were reported to the police and the one episode reported to a physician.

pared to 6.8 per cent of the girls not warned (C.R. = 4.1).

It could also be expected that the adjustment to those episodes preceded by consent to sex play would differ from the reactions to the apparently unprovoked episodes. The offended who consented in sex play might entertain feelings of being responsible for provoking the offensive behavior. The findings show that the offended girls who engaged in prior sex play report that they kept secret the knowledge of the episode more often than did the offended who did not admit to some previous type of erotic activity, 50.9 and 12.1 per cent, respectively (C.R. = 8.7). Surprisingly, they were also found to have discussed these episodes with their friends to a greater extent than did the offended who experienced the abrupt sexual aggressions, 18.5 as compared to 6.1 per cent (C.R. = 3.6), but appealed to their parents and other authorities to a much smaller degree, 3.8 in contrast to 21.2

usually terminated by the offensive male behavior.

From the foregoing analysis certain summary statements and conclusions can be drawn.

1. In studying this particular type of person-to-person interaction, it could be expected that offensive experience would be associated more with features of the pair relationship and the factors influencing behavior in pair relationships than with the personal characteristics of the offended. The frequency of dating, the number of men dated, and religious participation were not significantly associated with offensive aggression.

2. Two factors which bear on behavior in dating-courtship relations—parental cautionings and contact with the male culture through older male siblings—were found related with proneness to offensive experience.

3. Characteristics of the pair relation-

ships found related to offensive behavior include heterogamous rather than homogamous pairings, especially with respect to age; involvement in more permanent rather than casual relationships; and a consequent voluntary participation in erotic activity.

4. The reactions involve a paucity of reliance upon parents and the formal agencies of control. Preference is given to pair adjustments and peer-group support. However, the adjustments are strongly influenced by at least two factors: parental warnings and sex play prior to the offensive experience. Warnings from parents also appear to pay dividends in the form of reliance on parents in times of difficulty. Offensive experiences

based on consent to sex play lead to increased secrecy, dependence upon peer-group support, further avoidance of parental confidence, and a reluctance to terminate the relationship.

5. This investigation gives support to some of the findings of the earlier study of university women.⁷ The phases of the earlier study repeated here, the prevalence of offensive aggression, the influence of dating frequency, relationship involvement and level of aggression, and the adjustment of the offended are in general accord.

PURDUE UNIVERSITY

⁷ Kirkpatrick and Kanin, *op. cit.*

ADDITIONAL HIGHER DEGREES IN SOCIOLOGY CONFERRED IN 1956 AND DOCTORAL DISSERTATIONS IN PROGRESS, 1956

The following names were submitted to the *Journal* subsequent to the listing in our July, 1957, issue:

DOCTOR'S DEGREES

NEW SCHOOL FOR SOCIAL RESEARCH

- Sidney H. Asch, B.S.S. City College of New York, 1940; LL.B. Columbia, 1943. "Labor Arbitration Procedure in the United States."
 Aharon Ben-Ami, B.A. Albright College, 1953; M.A. New School, 1955. "Durkheim's Sociological Method and Parsons' Action Theory (a Study in Sociological Theory)."
 Werner Cohn, B.S.S. City College of New York, 1951; M.A. New School, 1954. "Sources of American Jewish Liberalism: A Study of the Political Alignment of American Jews."
 Janice Winifred Harris, B.A. Hunter College, 1939; graduate work, Chicago, 1939-40. "Thorstein Veblen's Social Theory: A Re-appraisal."
 Monte Hilliard Koppel, B.S. Syracuse, 1949; M.A. New York, 1950. "Lima, Peru: A Study in Industrial-Urban Change."
 Thomas Luckman, Abitur. Vienna, 1946-48; Innsbruck, 1948-49; M.A. New School, 1953. "A Comparative Study of Four Protestant Parishes in Germany."
 Harold Mendelsohn, B.S.Sc. City College of New York, 1945; graduate work, Columbia, 1945-46. "Requisites in the Investigation of Social Systems of Mass Communications: Italy: A Case Study."
 Kenneth T. Skelton, B.A. Waynesburg College, 1928; M.A. Pennsylvania State, 1930; Columbia, 1941-42. "Mark Twain: Sociological Realist."

NEW YORK UNIVERSITY

- Elizabeth Anne Betz, A.B. Wisconsin, 1938; A.M. New York, 1950. "Changing Methods of Social Control: A Study of Transitions in Techniques of Dealing with Youthful Deviancy in Selected Areas of West Africa."
 Howard E. Freeman, A.B., A.M. New York, 1948, 1950. "The Prediction of Recidivism among Youthful Offenders in the Highfields Treatment Program."

- Nathaniel H. Siegel, A.B. Brooklyn College, 1950; A.M. New York, 1952. "Relations of Social-Psychological Factors to Treatment and Outcome in Two Correctional Facilities."

MASTER'S DEGREES

NEW SCHOOL FOR SOCIAL RESEARCH

- Vadakkan Verghese Alexander, B.A. Travancore, 1944; B.D. United Theological College, 1950; Th.M. Princeton Theological Seminary, 1952. "The Machinery for the Settlement of Industrial Disputes in the U.S.A."
 Salvator Attanasio, B.A. New School, 1953. "Five German Roman Catholic Parishes."
 Aviva Berezner, Hebrew (Jerusalem), 1950-53; Teachers School (Jerusalem), 1945-47. "The Sociocultural Institutions of the Jewish Community in Yeman."
 Shoshana Burstein, School of Social Work, Hebrew, 1940; Diploma, Teachers College, Tel-Aviv, 1947; School of Education, New York, 1951-54. "Adjustment of New Immigrant Groups in Israel."
 John R. Cowley, B.A. Richmond, 1933. "Sociological Aspects of Divorce Law in United States and New York State: A Uniform Divorce Law in United States Considered."
 Subhash Chandra Mukherjee, Bangabasi College, Calcutta, 1948; City of London College, 1952; Juniata College (Huntington, Pa.), 1954. "Social Security in India."
 Andrew Z. Kanu Orizu, B.S. Bethune-Cookman (Daytona Beach, Fla.), 1951; A.M. New York, 1954. "A Study and Analysis of the African Nationalist Movement in New York City."
 Leslie J. Silverman, B.A. Roosevelt College, 1950. "Social Role of the Salesman."
 Bernard Zvi Sobel, B.A. Brooklyn College, 1955. "The M'Lochim: A Study of a Religious Community."
 Bert Wiedhopf, A.B. Miami, 1954. "Some Attitudes and Stereotypes Relating to the Concept of Mental Illness."

NEW YORK UNIVERSITY

- Roger Baldwin, A.B. Boston, 1952. "The Attitudes of Delinquent Boys toward Their Families."
- Irving Gellman, A.B. New York, 1951. "Social Science in Technical and Economic Assistance Programs."
- Leah J. Glass, A.B. Berea College, 1945. "Effects of Segregated Residential Areas on the Vocational Aspirations of Negro and White Adolescents."
- Martin Haberman, B.A. Brooklyn College, 1953. "The Effects of Work Experience on the Orientation toward Success and Achievement among College Students and Graduates."
- Barbara K. Jacobson, A.B. Radcliffe College, 1941. "Change of Religious and Political Affiliation in a Group of College Graduates."
- Josephine Schoenborn, B.S. Columbia, 1954. "The Disposition of Babies Born to Women in a Correctional Institution."
- William Weiss, B.S. City College of New York, 1951. "Worker and Citizen."
- Rosalyn A. Whitehead, A.B. North Carolina College, 1953. "The Social System of a Small Work Group: A Beauty Salon."

DOCTORAL DISSERTATIONS IN PROGRESS, 1956

NEW SCHOOL FOR SOCIAL RESEARCH

- A. Aziz Allouni, A.B., M.A. American (Beirut), 1941, 1942. "Social Factors Underlying Economic Development in Syria."
- Julian Aronson, B.S.S. City College of New York, 1928; M.A. Cornell, 1929. "Sociological Aspects of Modern Secondary Education in the United States."
- Mala Gitlin Betensky, Abiturium, Poland; Vienna, Maryland, and Maryland Institute; M.S.Sc. New School, 1943. "Problems of Adolescence in Collective Settlements in Israel."
- August C. Blunck, B.A., A.M. New York, 1919, 1920. "American Lutheranism and Democracy."
- Clark H. Bouwman, B.A. Kalamazoo College, 1941; B.S. Western Michigan College, 1946; graduate work, Divinity School, Yale, 1946-47; M.A. New School, 1950. "The Effects of Industrialization on a Small Rurally Oriented Southern Community."
- Bel Cary-Lundberg, A.B. Vassar College, 1925; A.M. Columbia, 1947. "Contributions to the Sociology of Capitalism."

- Robert A. Deutsch, B.S. Long Island, 1947; M.A., M.S.Sc. New School, 1952, 1954. "The Ecological Aspects of Civilian Defense in the Atomic Age."
- Max Feder, B.S. Minnesota, 1922; M.A. New York, 1942. "American Labor and the Social Services."
- Margaret W. Giovannelli, A.B. Indiana State Teachers College, 1935; A.M. Illinois, 1937. "Montaigne: Moral Aestheticism."
- Morris Goldman, B.A. New York, 1949; M.A. New School, 1953. "The Sociology of Negro Humor."
- Joseph Bebee Jowers, B.A. Paine College, 1948; B.D. Drew Seminary, 1951. "Negro Baptists and Methodists in American Protestantism: Aspects and Trends."
- Abraham Kampf, Teachers Seminary (Tel-Aviv), 1945-47; B.S. New York, 1951; M.A. New School, 1953. "Jewish Artists in Paris: A Contribution to the Category of the Stranger."
- Erwin Muller, Abitur, Realgymnasium, Wiesbaden; German degrees Marburg and Berlin; Master of Sacred Theology, Mount Airy Theological Seminary (Philadelphia, Pa.). "The Protestant Church in Germany during 1933-45."
- Leo Rattner, B.A. Denver, 1951; M.A. New School, 1955. "The S-S: A Socio-psychological Study of a Political Organization."
- Leon J. Rosenthal, B.S., LL.B. Rutgers, 1915, 1923. "Insurance Services by Trade and Professional Associations to Their Members."
- Aeta Salba, A.B. Hunter College, 1928. "Titus Oates (Times of Betrayal)."
- Lillian S. Shapiro, A.B. Radcliffe College, 1934; M.S.Sc. Smith College, 1936. "Changing Theories in Child Care and Their Relation to the American Family since 1800."
- Matthew Ies Spetter, Institute for Social Psychology, The Hague; Library School, Amsterdam City and Leicester, England; M.A. New School, 1955. "The Jews of the Netherlands. Part I: Their Acculturation and Integration in Dutch Society"; Part II: "Epilogue: The Destruction of the Patterns of Social Mutuality under the Influence of the German Occupation of the Netherlands from May, 1940, to May, 1945."
- Chalmers K. Stewart, A.B., M.A. Akron, 1932, 1933. "The Independent Socialist League: A Sociological Analysis."

Lester Weinberger, B.S. (S.S.) City College of New York, 1938; B.S. (Libr.) Columbia, 1939; M.A. New School, 1951. "The Teacher's Union: A Study in White-Collar Unionism."

Margot Haas Wormser, B.A. Elmira College, 1943. "Community Studies on Anti-Semitism."

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Harold Alksne, A.B., A.M. New York, 1952, 1953. "Social Heterogeneity within an Economically Homogeneous Group."

Joan M. Chapman, A.B. Miami, 1946. "An Apparent-Member Study of Juvenile Delinquency."

Donald S. Dushkind, B.S.S. City College of New York, 1945; A.M. Iowa, 1946. "Attitudes of Male Probationers between 16 and 19 toward Youthful Offender Treatment in New York City."

Gerald Gordon. A.B., A.M. New York, 1954, 1955. "Socio-psychological Atmosphere and Organizational Appeal."

Howard B. Kaplan, A.B., A.M. New York, 1953, 1954. "An Empirical Typology for Urban Description."

Edward Rothstein, B.E. Teachers College (Conn.), 1938; M.Ed. New Hampshire, 1940. "A Study of the Social Processes in the Recruitment and Training for the Role of Policeman."

NEWS AND NOTES

American Council of Learned Societies.—For the academic year 1958–59 a Special Awards Program to make possible the completion of distinguished works in the humanities has been developed. The studies proposed must fall within one or more of the humanistic disciplines, interpreted in general as follows: philosophy, including the philosophy of science and the philosophy of law; philology, languages, literature, and linguistics; archeology, art history, and musicology (but not applied art or music); history, including the history of science and the history of religions; and cultural anthropology, including folklore. Programs in the social sciences and the natural sciences which have a humanistic emphasis will also be considered.

The Special Awards will be granted to mature scholars of unquestioned ability who are nearing completion of a distinguished and significant work. The ACLS will seek nominations from academic institutions, professional societies, and other sources. Applications from individuals will not be solicited. The amount of each Special Award will be \$10,000. Not more than ten will be granted for 1958–59. Tenure will normally be continuous for a period of at least eight months of uninterrupted work. Within this requirement the grant may be used for maintenance, travel, clerical or research assistance, materials of research or study, or any other relevant purpose.

Applications will be received from individual scholars. In cases in which the program of the individual is related to the plans of his institution, the candidate should obtain the institution's approval. Application forms should be requested from the ACLS Fellowship Program, American Council of Learned Societies, 2101 R Street N.W., Washington 8, D.C. Applications must be received not later than October 15, 1957.

American Nurses' Foundation.—The American Nurses' Association founded the American Nurses' Foundation in 1955, being convinced that concern for nursing research is not limited to the profession itself but involves the entire health field. The profession then established a co-ordinated program of research, carried out

in the public's interest and with the public's support, which has added to the literature of its own and other professions, and helped to extend interdisciplinary research involving social scientists and clinical experts in medicine, psychiatry, clinical psychology, and nursing. Scholars from the relatively new field of medical sociology were drawn into the program as project directors, members of research teams, and consultants. Throughout 1955, the Foundation was wholly supported by the American Nurses' Association, which still provides administrative and operating funds, office space, and staff. The major portion of the Foundation's resources in 1955 and 1956, approximately \$84,000, went into research grants to seven other institutions. In September, 1955, the Foundation staff initiated and conducted its own pilot study of public health nursing. This led to a three-year project approved by the National Institutes of Health, United States Public Health Service, one year of which has been completed.

The first of the educational and information projects is being made under a grant from another foundation. It is a pilot project involving field service to a selected group of exchange visitors to the United States for additional experience in nursing.

Research in the following four major areas will best serve the purposes for which the Foundation was created: effects on nursing of changing health needs and changing patterns of patient care; administrative problems within organizational systems; nursing needs of various types of patients and nursing in different categories of illness; and the nursing profession. New ideas will be welcomed for review.

Projects submitted to the Foundation for its support will be judged by the clarity with which the problem is stated, the merits of the research design, and the qualifications of the research directors who apply. Grants *will not* be made directly to individuals but will be given to universities, colleges, research institutes, and other organizations that qualify under Section 501(c) (3) of the Internal Revenue Code of 1954. Proposals will be reviewed semiannually. They must be in the headquarters of the Foundation

on or before November 1 and March 1. Fourteen copies must be submitted, following an outline provided by the Foundation.

For information apply to the American Nurses' Foundation, Inc., 2 Park Avenue, New York 16, New York.

Bethel College.—David O. Moberg, associate professor of sociology and chairman of the Department of Social Sciences, will serve as Fulbright lecturer in sociology at the State University of Groningen, the Netherlands, during his sabbatical leave in 1957-58. In his absence, his courses will be taught by Harlan Mickelson, doctoral candidate at the University of Minnesota and recently a member of the faculty of Luther College, and Roy Rodgers, of the University of North Carolina, who also will work on his doctorate at the University of Minnesota.

Bowdoin College.—Richard Leighton van Nort has been promoted to the rank of assistant professor in the Department of Sociology.

Brandeis University.—Robert A. Manners, associate professor of anthropology and chairman of the Department of Sociology and Anthropology, will conduct field work in Tanganyika, East Africa, during the academic year 1957-58. He is a member of the Ford Foundation-sponsored Project for Research in Cultural Regularities under Professor Julian H. Steward, of the University of Illinois.

During Dr. Manners' absence, Paul Radin, Samuel Rubin Visiting Professor of Anthropology, will be chairman of the joint Department of Sociology and Anthropology.

Alexander Lesser, visiting associate professor of anthropology, will replace Dr. Manners as professor. During the academic year 1958-59 Dr. Lesser will replace Stanley Diamond, who will then be on leave of absence in Ghana, West Africa, as a member of the Project for Research in Cultural Regularities.

Lewis Coser, associate professor of sociology, will be on leave at the University of California in Berkeley, where he will be visiting associate professor. He will be replaced at Brandeis by Suzanne Keller, visiting lecturer in sociology.

Robert L. Stigler, assistant professor of anthropology, will spend the fall semester conducting a preliminary archeological reconnaissance in northern Iran as prelude to a five-year program of research in that area.

Other members of the joint department at

Brandeis are Jerome Himelhoch, Philip Rieff, and Maurice Stein.

Brooklyn College.—Associate Professor Rex D. Hopper has been elected chairman of the Department of Sociology. The United States Department of State awarded him a visiting professorship for the summer of 1957 in the National University of Paraguay at Asunción.

The following are to be on sabbatical leave for 1957-58 to travel and to conduct special research: Feliks Gross, Alfred McClung Lee, Joseph Jablow, Samuel Koenig, and S. M. Miller. Professor Gross, who has been promoted to full professorship, has been appointed Fulbright lecturer in sociology at the Università degli Studi di Roma. Professor Jablow, who has been appointed assistant professor, is to study the aboriginal land tenure of the Ponco Indians for the United States Department of Justice; he and his wife, Alta Jablow, are also writing a book on African folklore. Professor Koenig is to lecture as visiting professor and to set up a Department of Sociology at Bar-Ilan University, Tel-Aviv, Israel; he will also make a study of the merging culture patterns in Israel as a follow-up to the study he made in 1950-51 under a Social Science Research Grant. Professor Miller is to carry on research and writing concerning work and workers and to study techniques of evaluation as a sociological research tool. Professor Lee will serve as UNESCO Professor of Sociology assigned to the Università Cattolica del Sacro Cuore, Milan, Italy. He is to develop there a university research center under the program of technical assistance.

Hugh H. Smythe has been granted a leave for 1957-58 in order to carry on anthropological field work in Nigeria under the terms of a Ford Foundation grant.

The following staff members are to be placed on continuing tenure on September 1, 1957: Sidney Aronson, Gerald Henderson, and Hugh H. Smythe.

David W. McKinney, Jr., has been appointed instructor.

Sylvia Fleis Fava, instructor, who received her Ph.D. degree from Northwestern University, is participating in a special graduate program on Metropolitan New York City at City College.

Associate Professor Marion Cuthbert is serving as deputy chairman of the department for

the School of General Studies during the sabbatical leave of Professor Koenig.

Irving Goldaber, lecturer, has been appointed assistant director of New York City's Commission on Intergroup Relations. He was formerly assistant co-ordinator of the National Community Relations Advisory Council.

Brown University.—Kurt B. Mayer has assumed the chairmanship of the Department of Sociology.

Vincent H. Whitney has resigned as chairman but continues as professor of sociology. He was elected president of the Eastern Sociological Society.

Harold W. Pfautz has been promoted to associate professor.

Robert O. Schulze, who has been promoted to assistant professor, taught during the summer in the extension program of the University of Michigan.

Surinder Mehta, of the University of Chicago, has been appointed instructor.

Sidney Goldstein, who has been promoted to an associate professorship, has been awarded a grant-in-aid from the Social Science Research Council for a study of labor mobility in the Providence metropolitan area. His book on migration and mobility patterns in Norristown, Pennsylvania, between 1910 and 1940 will be published in the late fall.

University of California, Berkeley.—Kingsley Davis will resume teaching in September, following a year spent as a Fellow at the Center for Advanced Study in the Behavioral Sciences. He will also direct the study on International Urban Research Project.

Wolfram Eberhard's leave of absence has been extended to January 1, 1958, so that he may continue his studies of village life in Pakistan.

Philip Selznick resumes teaching in September, following a year's absence as a Behavioral Science Fellow at the University of Chicago Law School.

Reinhard Bendix will be on leave during the academic year 1957-58 to continue his studies in the industrialization of underdeveloped areas.

Seymour M. Lipset lectured at the Salzburg Seminar in American Studies in Austria in August.

Tamotsu Shibutani will be on leave during the academic year 1957-58.

Cesar Grana resigned his lectureship to accept a position as assistant professor in the College of the University of Chicago.

Leo Schnore, of Michigan State University, has joined the department on a joint appointment between the School of Public Health and the Department of Sociology and Social Institutions. He will teach urban sociology and demography.

Martin Trow, of Bennington College, has accepted a joint appointment in educational sociology between the Department of Education and the Department of Sociology and Social Institutions.

Lewis A. Coser, of Brandeis University, who will be a visiting associate professor during the academic year 1957-58, will offer courses in social theory.

Erving Goffman, of the National Institute of Mental Health, will serve as visiting assistant professor during the second semester of 1957-58. He will teach social psychology.

Nathan Glazer will be visiting lecturer in the department during 1957-58.

Carleton College.—Samuel M. Strong, chairman of the Department of Sociology, gave a course in intergroup relations in the summer session of the University of Alberta. He has been elected chairman of the social science section of the Minnesota Academy of Science for 1958.

Duke University.—The Department of Sociology notes that several recent sociological publications have erroneously listed its founder and first chairman, the late Professor Charles A. Ellwood, as among the pioneers of American sociology who entered the field after a ministerial career. Since these publications include H. W. Odum's *American Sociology*, R. C. Hinkle's *The Development of American Sociology*, and other sources which promise to become definitive reference works in the history of American sociology, the department desires to have this error made a matter of public record. Although deeply interested in religion as a dynamic factor in the development of human society, Professor Ellwood had neither theological training nor a ministerial career. Except for one year spent as part-time secretary of the local Charity Organization Society while instructor in sociology at the University of Nebraska, he had no professional experience or employment outside the field of sociological teaching and research.

Eastern Sociological Society.—The annual meeting for 1958 will be held in Philadelphia on April 19 and 20 at the Hotel Warwick. All mem-

bers are cordially invited to participate in the program. Papers limited to 1,500 words may be submitted to Dr. Dorothy R. Blitsten, Department of Sociology and Anthropology, Hunter College, New York 21, New York, before November 15, 1957.

University of Florida.—T. Lynn Smith, professor of sociology, served as visiting lecturer at the Universidad Nacional Autónoma de México during the month of June, under a co-operative arrangement between the University and the United States Department of State. Under the title *Problemas sociológicos de América Latina*, the United States Information Service at the American Embassy in Bogotá has published four of the lectures he presented in 1956 in Portugal and throughout South America.

Florida State University.—Ernest Q. Campbell is the recipient of a Social Science Research Council Postdoctoral Fellowship for training and research in social psychology at the Laboratory of Social Relations, Harvard University, beginning September, 1957. He taught this summer at Vanderbilt University.

John L. Haer has accepted an appointment with the RAND Corporation, Santa Monica, California.

William F. Ogburn, professor emeritus of sociology of the University of Chicago, who has been a visiting professor at the Indian School of International Studies, New Delhi, India, during the academic year 1956-57, will return as visiting professor in the second semester of 1957-58.

International Society for Social Defence.—The Society will hold its Fifth International Congress at Stockholm, August 25-30, 1958. The subject of the Congress will be intervention by the courts or by other authorities in the case of socially maladjusted children and juveniles, discussed under the following headings: stages in the development of socially maladjusted minors, the competent authorities, and available measures.

The official languages of the Congress will be English and French, for which languages simultaneous interpreting will be arranged. Speeches in another language will be allowed if the speaker himself provides a translation.

Attendance at the Congress will be open to all interested in its subject. Scientific reports under each of the three headings are invited. These reports, written in English or in French, ought not to be longer than twenty pages. They

should be sent to Professor Ivar Strahl, Hjalmar Brantingsgatan 4A, Uppsala, Sweden, before January 15, 1958. All inquiries should be addressed to him.

University of Kentucky.—The Bureau of Community Service and the Department of Rural Sociology have recently conducted three-month training courses in community development for thirteen persons from Pakistan and eleven from Indonesia. These persons are expected to set up community development training centers in their own countries on their return.

The Commonwealth Fund has made a substantial grant for planning studies in connection with the new medical school and medical center which the University expects to open in 1959. A considerable portion will be used for sociological studies, under the direction of Robert Straus, medical sociologist, and Thomas R. Ford, associate professor of sociology.

In collaboration with the Farm Population and Rural Life Branch, Agricultural Marketing Service, USDA, the Department of Rural Sociology is engaged in an extensive study of education in three counties where incomes are low. John R. Christiansen and Sloan Wayland are study directors for the USDA, and the University is represented by Howard W. Beers, head of the Department of Sociology and the Department of Rural Sociology, and James S. Brown, associate rural sociologist.

Sidney J. Kaplan, assistant professor of sociology, has been serving as elected chairman for 1956-57 of the social science division of the College of Arts and Sciences.

John Ball, assistant professor of sociology, has been awarded a grant by the University Research Fund Committee for the preparation of a book on juvenile delinquency.

Joy N. Query, instructor in sociology, has been awarded a University of Kentucky Research Foundation Fellowship.

Ralph Ramsey, extension specialist in rural sociology, again served on the faculty of the regional summer school for extension workers in agriculture and home economics at the University of Arkansas.

James W. Gladden, associate professor of sociology, was on sabbatical leave during the spring semester, which he spent on a study of Negro family life and as a consultant in the South for the National Student Y.M.C.A.

Buford Junker, visiting lecturer in sociology this year, has accepted a position as research

associate in the Administrative Science Center at the University of Pittsburgh.

John R. Christiansen, of the Division of Farm Population and Rural Life, USDA, who has been stationed at the University, has accepted a position on the sociology staff at Brigham Young University.

Gilbert Hardee, who has been in residence during the year, has been appointed to the staff in rural sociology at North Carolina State College, as has also James N. Young.

Howard W. Beers is serving as part-time consultant to the Farm Population and Rural Life Branch, Agricultural Marketing Service, USDA, and to the Office of Experiment Stations, USDA.

Latin American Center for Research in the Social Sciences.—As a result of the second Latin American Seminar on the Social Sciences, which met in Rio de Janeiro in April, 1957, two important institutions, sponsored by UNESCO and the Latin-American governments concerned, have been established. One is the Faculty of Social Sciences, aimed at training of specialists in the social sciences, to be situated in Santiago, Chile; the other is the Latin American Center for Research in the Social Sciences, with headquarters in Rio de Janeiro, Brazil. Both organizations will be governed by a common Consultative Assembly and Board of Directors. Gustavo Lago Matus, professor of the University of Chile, was elected secretary-general of the new Faculty of Social Sciences. L. A. Costa Pinto, professor of sociology of the University of Brazil and vice-president of the International Sociological Association, was elected director of the Latin American Center for Research in the Social Sciences. The research program of the Center, now being formulated, will focus on the implications of social change and economic development in Latin America. Publications, reports on research in progress, and research plans in this general field will be helpful in formulating the future activities of the Center, whose provisional address is Rua Voluntários da Pátria, 107, Botafogo, Rio de Janeiro, D.F., Brazil.

National Science Foundation.—Bertha W. Rubinstein, sociologist, has been given a certificate of merit and an incentive award for outstanding performance as professional assistant in the social science program of the Foundation.

University of North Carolina.—Gordon W. Blackwell, Kenan Professor of Sociology and director of the Institute for Research in Social Science, resigned July 1 to become chancellor of the Woman's College of the University at Greensboro.

Daniel O. Price, research professor of sociology and statistics and director of the Social Science Statistical Laboratory in the Institute for Research in Social Science, has been appointed to succeed Dr. Blackwell as director of the Institute.

Northwestern University.—Raymond W. Mack has been promoted to associate professor. He has been awarded a research grant from the graduate school for a study of interviewer effect in research on race relations. He is on half-time leave from his departmental duties this year to participate in the development of a liberal arts program for business executives returning to college under employer sponsorship. Part of his teaching load is being assumed by Morton B. King, Jr., who has joined the department as visiting lecturer in sociology.

Harold Guetzkow has been appointed professor of political science and sociology, jointly with psychology. He continues the teaching and research in organizational behavior which he developed while at the Graduate School of Industrial Administration at Carnegie Institute of Technology. He will participate in a program of graduate training and research in international relations being established by the Department of Political Science under a grant from the Carnegie Corporation.

Scott Greer has been appointed associate professor of sociology. He is completing his research with the Metropolitan St. Louis Survey. Part of his time will be devoted to the Transportation Center of Northwestern University, which he will serve as staff sociologist.

Richard L. Simpson, who taught previously at Pennsylvania State University, has been appointed assistant professor of sociology.

William F. Byron has become an emeritus professor. He will teach next year in the Department of Sociology at the University of Cincinnati.

Ohio Valley Sociological Society.—At the nineteenth annual meeting held in April in Columbus, Ohio, the following were elected officers for 1957-58: president, James E. Fleming, Kent State University; vice-president,

James McKee, Toledo University; secretary-treasurer, Marvin B. Sussman, Western Reserve University; representative of the Society to the Council of the American Sociological Society, Raymond F. Sletto, Ohio State University; editor of the *Ohio Valley Sociologist*, Robert P. Bullock, Ohio State University.

In 1958 the twentieth anniversary meeting will be held in Cincinnati, Ohio.

University of Pittsburgh.—Manuel C. Elmer, retired head of the Sociology Department, has been appointed to a professorship by the John Hay Whitney Foundation to teach during the 1957-58 academic year at Western College for Women at Oxford, Ohio. The professorship is a part of the Whitney Visiting Professors Program, begun in 1952 by the John Hay Whitney Foundation, to make available to small individual liberal arts colleges the knowledge and experience of educators retiring from larger institutions. Since 1953 the New York Foundation has co-operated in providing funds toward this end.

An Administrative Science Center will be established this fall, with James D. Thompson, Cornell Associate Professor of Administration and editor of *Administrative Science Quarterly*, as its head. The Center is founded on the thesis that the administration of organizations has progressed beyond an art and may now be properly considered as a science. It will serve as a service and a teaching resource for all university schools in which the teaching of administration is important; as a ground for exchange of professional experiences for the faculty of the various schools concerned with administration per se; as a core teaching program; and as a research agency. The staff of the Center will consist largely of behavioral scientists.

Presbyterian-St. Luke's Hospital, Chicago, Illinois.—A Department of Social Science has been formed in the School of Nursing in conjunction with the development of a new curriculum. Hans O. Mauksch, sociologist, was appointed chairman of the department and is also serving as consulting sociologist to Nursing Administration. The other appointments are Otto Schlesinger, psychologist, and Wolf Heydebrand, sociologist. The new curriculum places strong emphasis on social sciences and their application. The department anticipates undertaking a synthesis of the social science contributions to the understanding of disease

processes and patients' experiences. Research is presently being conducted in role perception of nurses and in social processes within the hospital.

Communications are welcome and should be addressed to the Department Chairman, 1439 South Michigan Avenue, Chicago 5, Illinois.

Princeton University.—The department offers a full program of work leading to the Ph.D. in sociology, including a minor in anthropology. Special research opportunities are available through the Office of Population Research, the Industrial Relations Section, and the Bureau of Urban Research.

A number of scholarships and fellowships are available for the support of graduate work. Information and application forms can be secured by writing to the Chairman of Graduate Studies, Department of Economics and Sociology.

The staff of the sociology section now consists of Professors Wilbert Moore, Frank Notestein, and Frederick Stephan; Associate Professors Morroe Berger, Gerald Breese, Marion Levy, Jr., and Melvin Tumin; Assistant Professors Paul Bohannan, Heinz Hartmann, Gresham Sykes, and Edward Tiryakian. Sheldon Messinger, who has been at the Center for Advanced Studies during 1956-57, has been named lecturer.

Wilbert Moore is currently organizing a conference on "Commitments of the Industrial Labor Force in Newly Developing Areas," to be held in Chicago, March, 1958. He is also the recipient of a research grant-in-aid from the Ford Foundation.

Frederick Stephan has been elected president of the National Association for Public Opinion Research for 1957-59. He is currently engaged in the general report of a study of undergraduates at Princeton.

Morroe Berger has recently published a study of the higher civil service in Egypt, entitled *Bureaucracy and Society in Modern Egypt*, based on data collected during a field study conducted in Egypt in 1953-54.

Gerald Breese will be on leave of absence during 1957-58 to serve as director of a team of specialists charged with preparing a master plan for the development of the Delhi-New Delhi metropolitan area in India, sponsored by the Ford Foundation.

Marion Levy, Jr., has received a grant from the Foreign Area and International Relations

Training Program of the Ford Foundation, which will relieve him of some of his teaching in 1957-59 and provide for travel and study in Japan from January to September, 1959.

Melvin Tumin has been appointed Senior Fellow of the Council of the Humanities, under whose terms he will be on leave during 1957-58 to complete his studies of social development and cultural change in Puerto Rico.

Paul J. Bohannan has been awarded a bi-centennial preceptorship under the terms of which he will have a leave of absence in 1958-59 to carry forward his studies in comparative sociology in Africa. He comes to Princeton from the Institute for Social Anthropology at Oxford University, England, and will specialize in developing the program in anthropology at Princeton. At present he is editing a collection of studies of homicide and suicide in Africa, under the title *The Arrow and the Noose*.

Heinz Hartmann has been promoted to assistant professor and research associate of the Industrial Relations Section.

Gresham Sykes has just completed a study of the prison as a social system which will shortly be published under the title *Society of Captives*. He has reached the final stages in his research study of the moral codes of delinquents, conducted with the delinquent population of the Jamesberg Home for Boys.

Edward Tiryakian has been promoted to assistant professor. He is completing a report on his field study of occupational stratification and mobility in the Philippine Islands.

Purdue University.—Harold T. Christensen, head of the Department of Sociology, has been granted a Fulbright Award to do research on the Scandinavian family during the academic year 1957-58. He will be attached to the Sociological Institute, University of Copenhagen, Denmark.

Gerald R. Leslie is returning from his year's leave at Teachers College, Columbia University, and will be acting head of the Department of Sociology during Dr. Christensen's absence. He has been appointed professor.

The leave of Dwight W. Culver has been extended for another year. He continues as executive director of the Panel of Americans, Inc., New York City.

Robert L. Eichhorn has been advanced to the rank of associate professor. In the last year Dr. Eichhorn supervised the survey phase of the Purdue Cardiac Project, a study designed

to determine some of the social and psychological factors related to successful adjustment to cardiac impairment.

James Beshers, who has been awarded the Ph.D. degree by the University of North Carolina, has received a grant from the Purdue Research Foundation to study patterns of internal population migration in Indiana.

Walter Hirsch has been awarded the Ph.D. degree by Northwestern University.

Four new staff positions have been filled: Irving Rosow, who has recently been directing research in the Nuffield Project, Belmont Hospital, England, is joining the staff to teach urban-industrial and occupational sociology and to help develop research projects. Leonard Breen, who has recently been director of the Criteria of Aging Project, University of Chicago, will have the responsibility of development and direction in the field of gerontology, his first responsibility being to direct research in the Columbus, Indiana, Project, a community-University study of the aging. Eugene Kanin is joining the department as instructor in marriage and family relationships, while at the same time completing his research for his dissertation at the University of Indiana. Janice Partridge has become a part-time instructor to assist with the marriage course.

Graduate assistantships and fellowship appointments for the academic year 1957-58 have been awarded to the following persons: Donald Riedel, Arthur Richardson, Paul Sites, Stanley S. Robin, David L. Brewer, Bernice Goldstein, Marilyn Kerster, George Carpenter, and Eugene Sherman.

Social Science Research Council.—The Washington office of the Council was permanently closed at the end of July, and the staff—Elbridge Sibley, Bryce Wood, and Joseph B. Casagrande—was transferred to the main office at 230 Park Avenue, New York 17, New York.

Two new programs—senior research awards in American governmental affairs, federal, state, or local, and grants for research on the Near and Middle East—will be initiated. (See *SSRC Items*, June, 1957.)

The Council again offers during the coming year predoctoral and postdoctoral research training fellowships, faculty research fellowships, and research grants-in-aid to qualified applicants in all fields of social science. In addition, several continuing special programs

of fellowships and research grants will support work relating to political theory and legal philosophy, national defense problems, American governmental processes, state politics, political groups in foreign areas, and Slavic and East European studies. With the exception of research training fellowships which become available after fulfillment of all requirements prior to the thesis for the Ph.D. degree, awards are offered only to scholars of postdoctoral or equivalent status.

About October 1, application forms will be available on request. Most types of awards will be made only once annually, and applications should be filed no later than the closing dates for the respective categories. The earliest closing date will be November 1. After August 1, inquiries concerning fellowships and grants should be directed to the New York address given above. When pertinent, brief indication of the inquirer's age, position, academic status, and the type of support desired will be helpful.

Society for the Study of Social Problems.—The seventh annual meeting of the Society, which will be devoted to current aspects of law enforcement, will be held jointly with the American Sociological Society, August 26–30, 1957, in Washington, D.C., at the Shoreham Hotel. The first day of these meetings, August 26, is one day before the American Sociological Society's meetings begin. Two special features of the program will be an address on August 26 by the Honorable Emanuel Celler, chairman, Judiciary Committee, United States House of Representatives, on the subject "Further Legislation Needed To Implement Desegregation: State and Local," and the joint session with the National Association of Social Work on August 27. All sociologists and other persons interested in research on social problems are cordially invited to attend.

Southwestern Sociological Society.—Officers of the society for the year 1957–58 are as follows: president, Hiram Friedsam, North Texas State College; vice-president, Franz Adler, University of Arkansas; secretary-treasurer, Leonard Benson, North Texas State College; sociology editor of *The Southwestern Social Science Quarterly*, Donald Stewart, University of Arkansas; Executive Committee Members, Sandor Kovacs, University of Tulsa, Walter Firey, University of Texas, Franz Adler, University of Arkansas, and Austin Porterfield, Texas Chris-

tian University. Marion Smith, Louisiana State University, will serve as representative to the Council of the American Sociological Society.

The Department of Sociology at North Texas State College is sponsoring the publication of the 1957 *Proceedings of the Southern Sociological Society*, of which Leonard Benson and Byron Munson are coeditors. Copies may be purchased from the secretary-treasurer, Leonard Benson, Box 5524NTS, Denton, Texas. Members of the American Sociological Society are encouraged to request their libraries to subscribe.

University of Texas Medical Branch, Galveston.—A Division of Medical Sociology, the first organization of its kind in any medical school, has been established, with E. Gartly Jaco as director.

Norman G. Hawkins, formerly of the University of Washington, and Sam Schulman, of the Boston University School of Nursing, have been appointed assistant professors of medical sociology in the division.

Wayne State University.—The department has become an active part of the newly created joint Institute of Labor and Industrial Relations of Wayne State University and the University of Michigan, with offices in Detroit and Ann Arbor.

Edgar A. Schuler, chairman of the department, will be on sabbatical leave in 1957–58 as a Fulbright Scholar attached to Thammasat University in Bangkok. Donald C. Marsh has been appointed acting chairman in his absence.

Harold L. Sheppard is on sabbatical leave in France as a Fulbright Scholar. He has been appointed an American member of the subcommittee on industrial sociology of the International Sociological Association.

Robert L. Fulton, a Graduate Fellow, has received an appointment at the University of Wisconsin.

William A. Faunce, a Graduate Fellow, has received a joint appointment in the Department of Sociology and Labor and Industrial Relations Center, Michigan State University.

Alvin Rose, a visiting lecturer for 1956–57, has received an appointment as associate professor in the department.

Laurene Wallace comes from a position at Alabama Polytechnic Institute to join the department as instructor.

H. Warren Dunham, now in the Nether-

lands, and Edward C. Jandy, now in Pakistan, are returning after spending the past year abroad as Fulbright Scholars.

Stephen Cappannari is returning from a visiting lectureship in anthropology at the University of California at Berkeley.

Frank Hartung, who has been teaching at the summer session of Washington University, participated in a seminar at the Frederick H. Moran Institute, St. Lawrence University, in early August.

Leonard W. Moss is collaborating with Dr. Eugenio del Monte, University of Rome, in a study of the village of Fumone.

A Spanish-language edition of *Modern Society*, by John and Mavis Biesanz, is now in preparation.

Mel Ravitz was director of the University's Summer Workshop in Human Relations.

Richard A. Waterman taught in the 1957 summer session at the School of Music, University of Michigan, conducting a seminar in ethnomusicology.

Helen Hause and Arnold Pilling, who have joined the staff, are offering courses in anthropology.

Stephen W. Mamchur is serving as president for the third year of the Ukrainian-American Cultural Foundation, Inc.

Albert J. Mayer has been appointed director of the Detroit Area Continuing Traffic Study, the purpose of which is to develop methods for predicting the future structure of the urban community. He has also received a Faculty Research Grant from the Social Science Research Council to continue a study of race relations in private housing.

Department members instrumental in forming the Wayne State University Federation of Teachers (AF of L-CIO), probably the largest

university union in the United States, are Richard Waterman, Donald Marsh, Mel Ravitz, Leonard Moss, and Harold L. Sheppard; they are among the officers and charter members.

Yale University.—New appointments include Donald B. Trow as assistant professor and Richard F. Curtis as instructor. Dr. Trow comes from Carnegie Institute of Technology, where he has been Senior Research Fellow in the Graduate School of Industrial Administration. He will give courses primarily in methods of social research and will participate in the medical sociology program. Mr. Curtis will assist in the introductory courses and supervise theses.

Omar K. Moore and Jerome K. Myers have been promoted to associate professor.

Jack V. Buerkle has been advanced to an assistant professorship.

Commonwealth Fellowships paying \$2,100 the first year and \$2,700 the second year are available to students in the training program in medical sociology. Applications should be made before February 1, 1958, to Jerome K. Myers, 306 Linsly-Chittenden Hall, Yale University.

At the Yale Center of Alcohol Studies during the past year Chandler Washburne has been engaged in a study of drinking in preliterate societies on a United States Public Health Service Postdoctoral Research Fellowship, and Kettil Brun, of the Alcohol Foundation of Finland, has been studying American viewpoints and techniques in research on alcohol and drinking. Milton A. Maxwell, of the Sociology Department of the State College of Washington, will be at the Center during 1957-58 as Senior Research Fellow, to analyze the costs of alcohol problems, especially as they affect business and industry.

BOOK REVIEWS

Sociology in the United States of America: A Trend Report. Edited by HANS L. ZETTERBERG. Paris: UNESCO, 1956. Pp. 156. Paper, \$2.50. (UNESCO Publications Center, 152 West Forty-second Street, New York 36, N.Y.)

This UNESCO publication of twenty chapters and twenty-two authors is a useful but limited report of postwar sociology in the United States. As a helpful directory to research activities and personnel, it reaffirms a familiar emphasis: explicit interplay between theory and empirical research in an effort to establish scientific sociological growth. It underscores the significant role of Columbia's Bureau of Applied Social Research and Harvard's Laboratory of Social Relations in the selection and treatment of several of the "topical" chapters, twelve of which are written by past or present associates of these two universities. And it accents a youthful semiorthodoxy: there are no contributions by seasoned old hands, Becker or Hughes or Blumer, for example, or by sociological social critics, say Lynd or Mills, or even by the more influential spokesmen of current trends. Works of these spokesmen—notably Parsons, Merton, K. Davis, Lazarsfeld, Stouffer—receive their due, of course, and if non-American readers miss comments from the horse's mouth, they will nevertheless be impressed by the dominant position of such scholars as well as by the scientific aspirations, empirical bent, and professionalization of their students and colleagues. They will learn little, however, about the humanistic side of sociology, which persists in this country and affects some of our most "scientific" colleagues, or about the large impact of such marginal contributors as Wiener at one extreme and Harry Stack Sullivan at another.

Though slanted and limited, this booklet has several virtues. The editor's introductory survey of sociological trends from 1945 to 1955 is a packed and informative eleven pages. Peter Rossi, in about the same space, outlines with enviable clarity and knowledge major developments in methodology, research techniques, and the organization of research. The skimpier sketches of sociological studies of law, military

life, education, knowledge and science, industry mass communications, religion, the professions, small groups, ruralism, social stratification, and mental health are carefully executed guides—but little more—to recent and ongoing research; nine chapters are less than four pages in length. In some cases this spatial restriction is met ingeniously: Hill and Simpson forego exposition almost entirely in favor of outlining and tabulation in their rather full depiction of "marriage and family sociology," while Sellin and England confine their first-rate, three-page discussion of criminology to four publications.

In spite of the authors' repeated caveat, they will be charged with sins of omission. I was surprised by Keller's disregard of *White Collar* in her tidy chapter on stratification and, to cite an instance of a fairly common and frustrating feature, by Glock's failure to report any substantive findings in his listing of works in the sociology of religion. Accusations of over-enthusiasm also may be voiced: Strodtbeck writes, "Since 1945, the study of small groups has grown like a healthy child"—altogether healthy? The charge of feeble critical estimate may also be made. But evaluation of course resides in the selective process itself, and a few authors introduce a measure of explicit assessment: Gross in the sociology of education, Reiss in urban sociology, Glazer in ethnic relations, and, markedly, Sellin and England in criminology.

Perceptive evaluation and keen analysis of trends mark the two most rewarding essays in the book, those by Gouldner and Lipset. The former's chapter on systematic theory treats the "important convergence" in recent works by Sorokin, Znaniecki, Homans, and Parsons; pursues difficulties and achievements stemming from attempts to integrate two "strategic empirical generalizations": that human conduct is largely normatively oriented but also determined by social interaction; wrestles with the uses of "values," "meaning," "social action," "social system," and other key concepts; and suggestively explores implications of Parsons' employment—and abandonment at times—of the equilibrium model. Lipset, after an introductory discussion of diverse trends and disci-

plinary overlapping in political sociology, delineates four areas of intense research activity—political participation, voting behavior, extremism, and bureaucracy—in which the principal contributions receive pointed comment and cumulative developments are brought out clearly. Lipset notes that this concentration has precluded much work on the “big” problems of social organization—power and control—but concludes that “ultimately” these problems and therefore policy matters should attract American sociologists. European readers of this essay, of the whole collection, indeed, will await such a change eagerly, if not impatiently. For ourselves, perhaps, this report puts the best foot forward.

CHARLES H. PAGE

Smith College

Education and Sociology. By ÉMILE DURKHEIM.
Translated and with an Introduction by
SHERWOOD D. FOX. Glencoe, Ill.: Free Press,
1956. Pp. 163. \$3.50.

When, some twenty years ago, a distinguished teachers college professor asked, “Dare the school build a new social order?” his naïveté went practically unnoticed. If the availability in English of four of Durkheim’s essays on the sociology of education will assist in disabusing educators of this curious conception of the school as somehow an independent molder of the social milieu in which it operates, the efforts of Fox as translator and editor will be well rewarded. The translation of these essays should stimulate the development in the United States of a veritable scientific sociology of education. For Durkheim’s general perspectives and principles as here set forth, combined with the specific analyses he made of the historical evolution of France’s educational system, provide the empirical materials and fundamental hypotheses for a systematic understanding of educational institutions viewed in the context of the social structures in which they have risen and are functioning.

The four chapters here presented are only a tantalizing sample of Durkheim’s pedagogical works. They include two articles, like encyclopedia pieces, on “Pedagogy” and “Education,” prepared for Buisson’s *New Dictionary of Pedagogy and Primary Education* (1911), the opening lecture on “Pedagogy and Sociology” delivered at the Sorbonne in 1902, when Durkheim substituted for Buisson as professor of

the science of education, and the lecture given in 1905 as part of a course on secondary education. They therefore cover, as Parsons notes in the Foreword, the period of Durkheim’s developing recognition of the process of internalization of sociocultural norms and reveal clearly his preoccupation with socialization. These essays should go far toward destroying the stereotypes of Durkheim as an “agelic realist” essentially unconcerned with psychological problems.

The explanatory materials by Fauconnet, Fox, and Parsons provide excellent interpretations of the significance of Durkheim’s accomplishments as a sociologist of education. Fauconnet’s splendid Introduction to the original edition is a model of precise exposition. Based in part on sources still unpublished, it superbly summarizes Durkheim’s sociological approach to the educational process.

Most modern educators will find Durkheim’s own educational theories far too intellectualistic. In this he shows himself to be a product of his cultural environment. But this does not vitiate the fundamental soundness of his sociological conception of the educational process as essentially a “methodical socialization” of the young.

The translation is, on the whole, accurate and smooth, although in spots overliteral.

HARRY ALPERT

National Science Foundation

Veritas, Justitia, Libertas: Festschrift zur 200-Jahrfeier der Columbia University, New York, ueberreicht von der Freien Universitaet Berlin und der Deutschen Hochschule fuer Politik (“Symposium Dedicated to Columbia University on the Occasion of Its Two Hundredth Anniversary by the Free University of Berlin and the German Institute for Higher Studies in Politics”). By the FREE UNIVERSITY OF BERLIN AND THE GERMAN INSTITUTE FOR HIGHER STUDIES IN POLITICS. Berlin: Colloquium Verlag, 1954. Pp. 347.

In this handsome volume appear fifteen papers, most of them on problems of society and politics, written for the combined occasions of the third anniversary of the Free University of Berlin and the two hundredth anniversary of Columbia. The rector says in his foreword that this tribute is offered to Columbia for its encouragement to those who made the struggle to establish a free university in the western

sector of Berlin after it had become clear that academic freedom was to be a thing of the past in the Soviet-controlled University of Berlin.

A major problem of our time, the fate of universities during revolutions or even under conditions of political stress, is the subject of several of the papers: "The Struggle for Berlin's University," by Georg Kotowski; "The German Academics as a Sociological Problem," by Hans-Joachim Lieber; "Freedom of Investigation and Teaching as a Theological Problem," by Hans Koehler. Others bear upon it less directly, although not less fundamentally: "Biology and Totalitarianism," "The 'New Intellectuals' in the Soviet Zone," and "The Spirit of Science." Some deal with the related problem of the freedom and political stance of other occupations: civil servants, laborers, white-collar employees, and the people of the press.

Hans Herzfeld's "State and Nation in German Historical Writing" is a little classic on the relation of the concepts of race, nation (and folk), and state in the ideologies of German historians, especially since the first World War. Students of national and nationalistic movements will find it a source of lasting value, along with, say, Van Gennep's *Traité des nationalités*. Flechtheim's paper, "From Mass Movement to Managerial Organization: The Evolution of World Bolshevism," is likewise a brilliant and scholarly analysis, in the tradition of much earlier German work on the changes wrought in social and political movements by success, a tradition which the generation of American sociologists who worked with Small, Thomas, and Park shared and applied to the study of religious, as well as to labor and reform, movements.

All together, this is an outstanding symposium of papers all of which have some implicit, when not explicit, reference to fundamental problems of social and political life in times of great stress—problems which the authors have seen at uncomfortably close range.

EVERETT C. HUGHES

University of Chicago

Social Implications of Industrialization and Urbanization in Africa South of the Sahara. Prepared under the auspices of UNESCO by the INTERNATIONAL AFRICAN INSTITUTE, London. ("Tension and Technology" series.) Paris: UNESCO, 1956. Pp. 743. \$9.00.

This work, the latest in UNESCO's "Tension and Technology" series, is brought out under the general editorship of Daryll Forde. It is really three books in one cover. The first, consisting of Parts I and II, contains an introduction by Forde to the whole work and digests of close to three dozen field studies of various African urban and semiurban communities. Many of the studies whose general methodology and results are digested in this part of the book have been published separately, but some exist only in manuscript, and others are available only in mimeographed copies. The second book in this volume is a rather extensive study of Stanleyville, Belgian Congo. The authors are V. Pons, a sociologist, who writes on the sociodemography of Stanleyville; Miss N. Xydias, a psychologist, who writes on labor conditions, training, and aptitudes of workers; and P. Clément, an anthropologist, who writes on the social patterns of urban life in Stanleyville, i.e., mainly on the family and kinship structure and various voluntary associations, as well as other forms of primary and secondary social contacts through mutual participation in neighborhood groups, in schools, and so on.

The third book is made up of a series of papers prepared for UNESCO's Conference on Industrialization and Urbanization in Africa, which was held at Abidjan in September and October, 1954. Like the digests of the community studies in the first section of this volume, these papers cover all parts of Africa south of the Sahara and range from studies on limited demographic aspects, or labor-recruitment problems, to far-reaching community studies on a large scale. These latter are usually also digests of larger studies. For example, the excellent paper by A. W. Southall on Kampala, Uganda, is a shorter version of some points elaborated at greater length in his more extensive later book with P. C. W. Gutkind, *Townsmen in the Making*, and the two stimulating papers of Cyril and Rhoda Sofer cover aspects which are treated at greater length in their more extensive social survey of Jinja, published under the auspices of the East African Institute of Social Research.

The emphasis throughout the work is primarily empirical, and the book contains a vast amount of data on urbanization and industrialization in various regions of Africa which may be used by students of economic and social change as the raw material on which to base generalizations. On the whole, the book itself contains few generalizations, and even

Forde's introductory essay, instead of presenting general conclusions, surveys and stresses the variety and diversity of urbanization and industrialization in Africa.

And yet the reader is impressed by a number of common aspects. African cities are inhabited by persons who are almost all "foreigners," both to one another and to the community. Forde expresses this well when he says that "the component groups are severally associated with distinct external societies, whites and Asians with overseas homelands . . . African migrants with their tribal communities" (p. 48). But, in addition to geographical and ethnic alienation, there prevails a large amount of psychological alienation, which shows itself in deep social cleavages and potential or actual eruptions of social strife. At the same time, the African town is more than almost any other in the present-day world a meeting place of many ethnic groups and subgroups and, apart from the major "racial" divisions, is a most effective melting pot. In the future development of political and social advancement of Africans, the city is, as this book shows, the most important and, indeed, the crucial force. It is not only the gateway through which "modern" Western methods of action and thought penetrate into Africa but also the place in which a new synthesis of predominantly African modes of social action and thought is being developed. This is summed up very well in the words of Georges Balandier: "The African town is a society in course of construction . . . while at the same time it serves as a center to which many traditional social and cultural patterns are brought, there to be transformed or fall into disuse. It exemplifies the state of unrest which, in varying degrees, exists in all African communities today. . . . We are confronted here by a changing situation, demanding an essentially dynamic conception of social and cultural factors" (p. 510).

BERT F. HOSELITZ

University of Chicago

- ✓ *Politics in a Changing Society.* By J. A. BARNES. London: Oxford University Press, 1954. Pp. x+220. 42s.

Bantu Bureaucracy: A Study of Integration and Conflict in the Political Institution of an East African People. By LLOYD A. FALLERS. Cambridge: W. Heffer & Sons, Ltd. [n.d.]. Pp. xiv+283. 30s.

The Kalela Dance: Aspects of Social Relationships among Urban Africans in Northern Rhodesia. By JAMES CLYDE MITCHELL. Manchester, England: Manchester University Press, 1956. Pp. 52. 7s. 6d.

While each of these three works merits the attention of students of social change and social organization in its own right, I put them together to call attention to the growing amount of excellent work being done on these problems in Negro Africa. Barnes and Mitchell worked under the auspices of the Rhodes-Livingstone Institute; Fallers, with the East African Institute of Social Research.

All three deal with certain of the problems which have arisen among peoples upon whose tribal, lineage, or other strongly kin systems of life a European territorial administration with a bureaucracy was superimposed. Barnes's style of presentation is that of a historically minded British social anthropologist who sneaks his theoretical points in well disguised. Fallers, American style, treats us to a theoretical chapter on institutional conflict and change and lets the history come after. Mitchell continues in his small monograph his running analysis of the social organization of urban communities in Rhodesia, doing a very nice job of using both sociological and anthropological concepts and of dealing with the realities of urban communities in their own terms rather than harping on the not-very-useful notions of survivals and detribalization.

Yet all are dealing with the same problems, and all are using that great natural laboratory of social change, Africa. Sociologists who are studying social change these days are very much inclined to stay at home and study the minor changes within an essentially homogeneous society; for instance, the fins on American cars or the popularity of various themes in song hits. They are doing little in places where changes are momentous. We would do well to follow closely the work being done in Africa by the new breed of anthropologist—who gets damned by some of his fraternity brothers for having become a sociologist and gets neglected by sociologists because he continues to study in circumstances where the more popular of current research techniques can hardly be used, because of the very great differences of language and culture among members of a community.

Mr. Fallers starts his book with a very ingenious statement on the degree of consistency

required in an ongoing society. While I think there is some confusion in his statement, he is seriously raising an important point: whether we really know how much consensus and consistency are a necessary condition of society and whether we do not assume much too blandly that every institution of society is a function of all the others, without really having looked to see.

EVERETT C. HUGHES

University of Chicago

The Social Structure of Islam. By REUBEN LEVY.
Cambridge: At the University Press, 1957.
Pp. vii + 536. \$9.50.

The Cambridge University Press has done well to reissue the pioneering work of Reuben Levy, which was originally published in London (1931-33) as a continuation of Herbert Spencer's *Descriptive Sociology* and which has long been out of print. It should be gratefully acknowledged that the new edition greatly excels its predecessor in convenience of arrangement and aesthetic attraction.

The extent to which the work has retained its usefulness is almost surprising. It continues to make good and stimulating reading even for the specialist. Its objective and underlying assumptions are well stated in the Preface: "The Muhammadan communities of the world, possessing certain common characteristics traceable to their religion, are suited for treatment as a unity, and the purpose of this book is an endeavour to investigate the effects of the religious system of Islam on the life and organization of the societies which acknowledge it. . . . [In the present book] the basic principles of Islam are examined and an attempt is made to ascertain how Muhammadans of different periods and climes have adapted their way of life to them" (p. v).

A program of this magnitude cannot, of course, be executed in full, and Professor Levy deserves the reader's admiration for his ability to bring together and organize as much material as he did, especially since he bases his presentation very largely on primary sources. If this reviewer feels unable to suppress a certain uneasiness, it is because of the very slight degree to which the book has actually been revised. In chapters v ("Moral Sentiment in Islam," pp. 192-241) and x ("Islamic Cosmology and Other Sciences," pp. 458-505), on the

basis of a fairly careful comparison, this reviewer noted only very few substantive changes or additions; and throughout the book one cannot help feeling that the advances in Islamic studies of the last twenty-five years have not been sufficiently taken into account. This observation applies to the collection of facts as well as to the questions which those facts are marshaled to elucidate. The Bibliography (pp. 506-22), for all its usefulness, reveals a curious unconcern with recent contributions. The work of W. M. Watt on the beginnings of Islam, of Lévi-Provençal on western Islam, of Gaudefroy-Demombynes and Ayalon on the Mamlûks, of R. Hartmann on the *Scheinkalif* in Cairo, are not even mentioned; studies like those of Schacht and Miss Lambton are listed but hardly exploited. The none-too-numerous attempts at a sociological analysis of one or another aspect of the Islamic development, such as Gibb and Bowen's *Islamic Society and the West* or Von Grunebaum's *Unity and Variety in Muslim Civilization*, have been omitted from consideration altogether. The texts which have come to our attention since 1933 have not been utilized as one would have wished (e.g., Tauhidî's *Imtâ'* and some of Ibn Taimiyya's writings). Chapter x, for all the interesting information it contains, remains the unorganic appendix which it was in the first edition.

Yet the book preserves its value as a storehouse of well-selected data, and even though it is out of touch with the more recently developed approaches and methods of sociology, it is not likely soon to be replaced—a curious sidelight on the uneven growth of Islamic studies, and perhaps of studies in non-Western civilizations in general.

It is only natural that a few errors should have slipped into the gigantic assemblage of facts which is Levy's *Social Structure of Islam*.

G. E. VON GRUNEBaum

University of Chicago

The Absorption of Immigrants: A Comparative Study Based Mainly on the Jewish Community in Palestine and the State of Israel. By S. N. EISENSTADT. Glencoe, Ill.: Free Press, 1955.
Pp. xii + 275. \$6.00.

In this book Eisenstadt sets himself several different tasks. First, he weaves concepts derived from recent sociological theory into a

generalized analytical formulation of the interaction between immigrant groups and their host societies. He directs specific attention to the consequences of such processes for varying degrees and types of integration of immigrant groups within the absorbing society.

The author's analytical framework brings into focus facets of this interaction process, particularly at the sociopsychological level, that have not previously been systematically related to each other. In this respect the book represents a notable contribution. However, Eisenstadt might well have exploited the "break-through" to carry his formulations to a further stage of development than he permitted himself in these pages. Nonetheless, he demonstrates the advance in theoretical equipment of the contemporary student of immigration phenomena over his many predecessors in the years between the two world wars.

Eisenstadt next attempts to apply his analytical framework to the waves of Jewish immigrants who settled their ancestral homeland, both before and after its establishment in 1948 as the state of Israel. Writing only a few years after the latter event, the author found the available data rather less than adequate to his purposes. In this respect, accordingly, the monograph must be regarded as a progress report of a pilot stage in sociological observation and analysis. Although historically incomplete, the data in places bear the marks of recent developments in social research technology.

Also of major interest is the author's application of his conceptual framework to a comparative reanalysis of other modern sites of immigrant groups, in particular the United States. Briefly developed here is a threefold typology of ethnic groups representing different modes of "group transformation and organization." This synthesis strikes the reviewer as an insightful and useful one in ordering the diversity of ethnic-host interaction processes and as one which warrants further elaboration.

All in all, Eisenstadt has produced, despite signs of hasty editing, a readable book which should prove rewarding for its conceptual contributions and for its account of what Israelis, discarding the slow-burning "melting-pot" analogy, refer to as a "cultural pressure cooker."

We can look forward to more definitive reports of further studies on the contemporary absorption of new and diverse types of immigrants in Israel. In the meantime, the present work is a significant extension of recent socio-

logical theory to an important scene of contemporary social change.

Finally, it should be noted that this book signalizes the emergence of the Hebrew University in Jerusalem as a new center of sociological thought and research.

LEO SROLE

Cornell University Medical College

America's Concentration Camps: The Facts about Our Indian Reservations Today. By CARLOS B. EMBRY. New York: David McKay Co., Inc., 1956. Pp. xiv+242. \$3.50.

Whenever a book is dedicated, as this is, to publicizing the plight of a minority group, it can best be evaluated according to these basic aims and not according to the canons of science. Embry has given an excellent description of the many problems that Indians face on the reservation and has performed an admirable service in dumping these problems directly in the lap of the public. For critical readers, however, the author's major contribution will be the program to end the "Indian problem."

A historical prelude leads the reader gently and unobtrusively into Embry's main theme—that we have legislated and bureaucratized the modern Indian into his present state of poverty and personal ineptness. The confident hunter and loyal ally has been incited to a desperate defense and driven finally to look to per capita payments and claims for past raw deals for subsistence. There is no other recourse, for discriminative legislation smothers any initiative the Indian may yet succeed in mustering.

The device employed to hammer out the theme of a pernicious bureaucracy dead set against the red man is a series of vignettes of life on reservations across the United States. In live conversations the Indian's frustration, hopelessness, and longing bring an indictment of vacillating Bureau policies that confuse, imprison, and inexorably numb any Indian who would be an American. When expert advice is required to complete the picture, Embry turns to officials of the Bureau itself.

As the author proceeds, it becomes evident that he conceives the problem simply as that of allowing the Indian to work and save as any free citizen should. He quotes an enterprising Sioux who mused wistfully, "If the Government would give me a tractor and some farming equipment and let me farm my land, I could

live good and have \$1,000 in the bank in two or three years' " (p. 184). The government is his only hope, for "if I went into a bank and asked to borrow some money, it would be just like going into a liquor store and asking for a fifth of whisky. They would tell me to get out and get back down on the reservation where I belonged" (p. 185). But although he had the American vision of a good living, money in the bank, and education for his children, "Burt" was forced to "play Indian" for summer tourists, just to live through the winter.

Embry also has nothing good to say about the Indian Reorganization Act, hailed as the "New Deal" for the Indian during the Roosevelt Administration. The IRA simply pyramids the red tape and perpetuates the consignment of coming generations to wardship and poverty: "The Indian to whom Uncle Sam has given a chance to read English can easily understand . . . that the Secretary of the Interior is boss" (p. 197). Indian testimonials and the very articles of the act dispel the fiction of self-government and a free future.

Embry calls for a "drastic remedy": laws discriminating against the Indian should be repealed immediately, and the Bureau "should be folded up, discontinued, and closed forever within a period of ten years or less" (p. 224). An immediate, intensive program of adult education and an adequate education for the young people should be instituted. If necessary, Indian funds held in trust could be released for these purposes. Tribal lands also held in trust should be handed over to the Indians, and a corporation in which each tribal member would hold stock should be formed for their exploitation (p. 227). The reservations, too, should be incorporated rapidly into local county and state governments. Since Indian lands are tax-free by treaty, the federal government must stand by this commitment until the Indians as individuals decide to become taxpayers. In the meantime the government would contribute tax moneys on the locally incorporated Indian lands, so that county and state services could be provided. With such a plan, Embry feels,

We could . . . close our concentration camps.

The great power of a few bureaucrats over a minority race of some 350,000 people would thus be ended.

The Indian could hope to progress as the people of this great country progressed.

The Indian then could take pride not only in being an Indian but in being an American [p. 229].

Embry's casual, yet informative, sketches of reservation life are the fruit of his earnest wish to get firsthand evidence by talking to literally thousands of Indians. He is accurate enough in what he has described, down to the recipe for "raisin jack." He has allowed his informants to spell out some of the vicious effects of controversial and alternately contradictory economic programs, and he has not spared popular myths such as the rich Indian. Finally, he has raised the issue of forced communism or freedom. However, in much of what he has to say, he follows the popular habit of assembling a complex set of phenomena under a simple explanation and solution. By setting up a monolithic cause in the Indian Bureau, he must of necessity prescribe a monolithic solution—its abolition. True, he also speaks out for Indian access to education and the usual health and welfare agencies, but his main target is the Bureau's malign influence.

His program strikes one as typically American. There is the popular antipathy for bureaucracy and authoritarian control, a proclamation of human rights and economic freedom, and an extended faith in education. This is all to the good, but how can it be made to work for the Indian? If Embry considers that an intensive effort at education can produce in ten years or so an Indian psychologically and experientially prepared to play his part in a voluntary and democratic manner, he must be remarkably optimistic, naïve, or insensitive to the magnitude of the problems. From my experience I would suggest that his program is a prescription for a growing—but still minority—group. He quotes but appears not to have understood those Indians who seek economic freedom and security *on the reservation* and who view any attempt to break up the reservations as a threat to their own and their children's survival.

Had Embry taken the trouble to couple his effort with an investigation into the work of anthropologists, historians, psychologists, and sociologists in this area, he would have gained perspective and might not have aimed his barbs solely at the bureaucratic institution raised by the American people to deal with the Indian problem. He might, instead, have concluded that the Indian problem was not simply one of Americanization but a complex one in culture contact and might have realized that no single program would do for all Indian reservation populations, for Indians differ in their degree

of acculturation from reservation to reservation and also within a reservation. Perhaps a number of programs, taking into account these differences, must be run concurrently. The American whites must be alerted and educated to this end, also; perhaps they, too, require a special Indian program, particularly those in and about the reservations.

FRED VOGET

University of Arkansas

In Search of Utopia: The Mennonites in Manitoba. By E. K. FRANCIS. Glencoe, Ill.: Free Press, 1955. Pp. xv+279. \$6.50.

In Search of Utopia is a comprehensive historical and descriptive field survey of the Mennonite communities of the province of Manitoba in western Canada. Those unfamiliar with World Mennonitism will profit from the first chapter, "From Religious Movement to Ethnic Group." This group, also known as "Evangelical Anabaptists," came to Canada from the Russian Ukraine in two separate migrations. It consists of some 45,000 persons sharing an almost undefinable and possibly "utopian" cultural and religious life, which, as Francis notes, seems to be thriving in the face of limited accommodation with the English-oriented Canadian majority. One gains the strong impression that the history of the group, which cannot be clearly defined as a unified cultural or religious body, which remains tightly knit although surrounded by strangers, and which has known prosperity and persecution over much of Europe, resembles what might have been the history of Europe's Jews, had they not early been socially defined as anti-Christian.

The author, while unfortunately not referring his analysis directly to the mass of modern work on migration and absorption, covers these areas fairly well, particularly at the empirical level. For example, he analyzes the trend toward ecological desegregation, the extent to which English and the dominant Canadian behavior patterns have in varying degrees replaced the traditional German dialect and European folk customs, and the participation of the group (actually two broad groupings distinguished by time of migration) in the developing economic, political, and educational systems of Manitoba. He suggests that in their evolution from a religious to an ethnic group their slow rate of change has spared them some

of the difficulties of marginal men. The essentially agrarian economy of the Mennonites before and after migration, the fluidity of the developing institutions of Manitoba, and tolerant treatment by the dominant surrounding population may be the keys to understanding this relatively painless adjustment-without-assimilation.

Francis' obviously great familiarity with the people he lived among and studied, his informal style, and the sixteen pages of photographs he presents give vivid insight into the past and present of Manitoba's Mennonites. That was his objective, and he has done it well.

ARTHUR JORDAN FIELD

Brown University

Moscow and the Communist Party of India. By JOHN H. KAUTSKY. New York: John Wiley & Sons, Inc., and Technology Press of Massachusetts Institute of Technology, 1956. Pp. xii+220. \$6.00.

The author, a political scientist, has composed what is represented in the subtitle as "A Study in the Postwar Evolution of International Communist Strategy." Working from primary source material, Kautsky has provided an informed and perceptive book.

The work is a case study, concentrating on the roles of the Soviet Union and of the Communist party of China in evolving the strategy of the Indian party. Excluded from the purview of the book is a history of the party or an evaluation of the party line or any prediction of the future of the party in India. Actually, the book is postwar history in so far as the argument is conveyed chronologically. However, it is history with an argument: the author on occasion is moved to denounce the Communist party of India and Moscow.

The book consists of seven chapters. Chapter i deals with a taxonomy of the chief strategies of communism during the last three decades. Chapters ii-vi are a history of the evolution of the doctrine of the Communist party of India during the postwar years 1945-54. Chapter vii summarizes and provides conclusions. The argument of the book is that the Communist party strategy in India and in Asia generally is a novel phenomenon of the postwar years, not continuous with the past, that is, not with the prewar doctrine. By "strategy" Kautsky means the long-range party line as

opposed to its day-to-day application, which is propaganda. What is novel is the vastly greater significance of the Chinese party and of the Maoist doctrine for India.

The new Maoist doctrine comprises an appeal to the bourgeoisie from below, not by infiltrating and taking over their existing organizations. The contrast is with the abortive attempt of the Communist party of China to take over the Kuomintang in the 1920's. The scene is the cold war of the 1950's, in which, presumably, the bourgeoisie has been confronted with the alternative of national liberation to colonial exploitation. The second element of the new Maoism is the thesis that the peasantry and not the proletariat is the mass base for power. Both the new appeal to the bourgeoisie and that to the peasantry are adaptations to Asian (and Latin-American and African) conditions. In his exposition of the Communist party doctrine, Kautsky must further distinguish whether it is colonialism or imperialism that he is talking about; his book is not clear on this point, yet it is important for the delineation of Communist strategy as well as for the exposition of Marxist doctrine, socialist or otherwise.

The struggle for control within the Communist party of India among various leaders, Joshi, Ranadive, and ultimately the Andhra faction, is well depicted. The rise of neo-Maoism to dominance in party strategy is presented as a logical evolution. However, how this evolution of doctrine can be considered a case study, lessons from which are to be applied to other nations which were recently colonies or elements of empires, is not explained. While Kautsky's taxonomy may be generally applicable, the example of India is set forth with emphasis laid upon its particularity. This is history and not nomothetics, which is all very well if we know it.

Of more general interest, and of closed concern to nomothetics, is another problem: Kautsky has adumbrated the study of a power elite with focus on authority and wide ramification. He describes a case of competition for power in the ramified lines, where success depends on being cued and on properly taking the cue from the center. It is perhaps in this domain, rather than in the exposition of a party line, that generalizations may be drawn from the book.

LAWRENCE KRADER

University of Chicago

The Norwegians: A Study in National Culture.

By DAVID RODNICK. Washington, D.C.: Public Affairs Press, 1955. Pp. vi+165. \$3.25.

The author and his wife spent a year in Norway, traveled through much of the country, visited or interviewed over 500 adults, and talked with or observed over 1,700 children and young people. The result is an interesting, although brief and impressionistic, study of national character and national institutions in a small, well-behaved country.

Rodnick believes that Norwegian parents bring up their children to adhere to high standards of behavior through continual direction and criticism. They discourage expression of emotions and self-assertion and rarely praise achievement. The result is that Norwegian adults have strong self-critical feelings, accompanied by rather critical feelings toward one another; their high standards for themselves and others are, they feel, not being lived up to; they fail to give one another much warmth and approval.

Two solutions for this psychological deficit are sought. Most people strive to be free from dependence on the (presumably hostile or critical) others; they avoid self-assertion, seeking co-operative relations with others which will not create envy and hostility. People try to be kindly and co-operative but avoid emotional closeness.

Rodnick tries to relate Norwegian national character to social institutions—the welfare economy with its many controls and social security provisions, class structure, educational system, industrial relations, religious patterns, political life, and communications media. He finds the social attitudes of the Norwegians notable for idealistic perfectionism, obedience to impersonal bureaucratic authority accompanied by dislike of personal authority, insistence upon complete civil liberties, and the emphasis, amid all their use of governmental action, upon improvement of personal character as basic to the perfection of society.

The difficulties of studying personality, national or otherwise, are well known. This study suffers from the lack of an articulate theory of personality which Inkeles reported in most such studies in his excellent chapter in the *Handbook of Social Psychology*. The methods are highly impressionistic, and some familiar with Norwegian society believe that the author's comments are misleading. Much reliance was

placed on observation and on interviews with small groups of adults and children. We are told that, in one such group, ten children believed that "Norwegians are a cold people" and only one did not; that all believed that Norwegians wanted to make friends but that it was difficult to; and so on. It would have been most interesting to compare figures from some larger numbers of Norwegians on questions of this sort with the answers which Americans, Englishmen, Frenchmen, Italians, or Japanese would give.

The attempt to relate personality to institutions likewise lacks theory, and it is not done very systematically, being almost incidental to a hasty descriptive survey of the social institutions of the country. There is even a capsule history of Norway. The study of national character in recent years has focused mainly on big powers which were regarded as world menaces—Germany, Japan, Russia, perhaps the United States—or on remote non-industrial societies like the Balinese or the east European Jews. Western Europe and the British Commonwealth, which are most often pointed out as having successfully adjusted modern economies to a humane and democratic society, have, like the "normal" personality, been the least studied. It is clearly important to examine the ways in which child-raising, adult personality, and social institutions are related in these stable states.

Rodnick's work is a pilot study in that direction. His methods are rather primitive, and his conclusions—particularly as the methods are not supplemented by any long familiarity with the society under study—must be taken as highly tentative. But the study should be useful to anyone, Norwegian or foreign, who is planning a more intensive investigation of Scandinavian society, or even merely a visit there.

ALLEN H. BARTON

University of Chicago

materials used were drawn from the records of juvenile offenders found guilty of indictable offenses for the years 1948, 1949, and 1950. For the purpose of determining the relative incidence of delinquency in the police districts, rates were calculated upon the basis of the total offenders in each district and for four age and sex groupings per 1,000 juveniles in the appropriate age and sex population. The study of treatment entailed an analysis of the variations in the extent in which the court employed absolute discharge, conditional discharge, probation, fines, and committal to institutions.

The analysis of rates of delinquents by districts revealed a number of significant variations. The rates for each of the age and sex groupings of offenders showed a wide range of variation among the 134 districts. Those in some districts were many times larger than those in other districts. Moreover, differences in rates between preponderantly rural and preponderantly urban industrial areas were marked. The disproportionately higher rate of delinquency among males than females in all districts corresponded to the findings of many other studies. More serious forms of delinquency were relatively more numerous in high-rate districts than in those with low rates.

Perhaps the most important conclusion to be drawn from the author's study of treatment is the fact that in England, as in the United States, there is no conclusive evidence to show what procedure is effective in the rehabilitation of the hard-core adolescent delinquent.

This book is a useful addition to the literature on delinquency. It is to be regretted that it does not add greatly to our knowledge of the varying conditions of community life which account for the kinds of variations in the incidence of delinquency which the author demonstrated to exist among the police districts in England and Wales.

CLIFFORD R. SHAW

Chicago, Illinois

Juvenile Offenders before the Courts. By MAX GRÜNERT. Oxford: Clarendon Press; New York: Oxford University Press, 1956. Pp. vi+145. 21s.; \$3.40.

This book is a report of a study of the variations in the rates of delinquency, in patterns of delinquent behavior, and in methods of treatment employed by the courts in the 134 police districts in England and Wales. The

Marriage, Past and Present: A Debate between Robert Briffault and Bronislaw Malinowski. Edited with an Introduction by M. F. ASHLEY MONTAGU. Boston: Porter Sargent, 1956. Pp. 90. \$1.50 (paper); \$2.50 (cloth).

Ashley Montagu has done a service by resurrecting this series of radio debates between Robert Briffault, the last of the great exponents of "mother-right," and Bronislaw Malinowski,

one of the first modern social anthropologists, from the buried files of *The Listener* (London) for 1931. Twenty-five years later the debates are still worth reading, but today any undergraduate can spot the holes in Briffault's presentation of the case for the matriarchal theory of social evolution. Malinowski, on the other hand, has stood the test of time remarkably well: his conclusion that "marriage and the family always have been, are, and will remain the foundations of human society" is generally accepted by anthropologists and others.

The institutions of marriage and the family have not gone through great revolutions from "matriarchal" stages to "patriarchal" stages to monogamy. Throughout human history societies everywhere have struggled with two important problems: the care and feeding of children and the transmission of the cultural heritage. The family has always been central to their solution, or partial solution, since no society has been able to solve completely the contradictions built into these tasks.

It is for this reason that we find so many "experiments" being carried out in different parts of the world with regard to the family and marriage. By examining these experiments and understanding them, we can gain greater insight into the problems that face our own society. Today, as twenty-five years ago, the family and marriage are still in a state of rapid change. But nowadays there are fewer prophets of doom and many more who are concerned with the serious study of these institutions and in making them operate more efficiently.

We are indebted to the editor for this demonstration of progress and for the clear evidence that the views of a professional have held up in this controversial field. Today we can add to Malinowski's analysis in a great many ways, but his prescriptions are already in practice in many instances. It is a sad reflection, however, that we do not yet have radio programs, let alone television programs, of this caliber in this country.

FRED EGGAN

University of Chicago

La Sociologie d'Auguste Comte. By JEAN LACROIX. Paris: Presses Universitaires de France, 1956. Pp. 114. Fr. 240 (paper).

Auguste Comte must have been very much on the mind of the Sorbonne professor who once warned Émile Durkheim that the study of so-

ciology would lead to insanity. Comte has, indeed, been a controversial figure both during his lifetime and in the hundred years since his death in 1857, and it is only in the recent past that he has achieved a certain amount of academic respectability. With St.-Simon, Comte has been denounced as a messianic bohemian, the fashioner of totalitarianism, and an unbalanced coffee-house intellectual; he has also achieved notoriety as the creator of a romantically absurd religion of humanity. His love affair with Clotilde de Vaux and its impact on his philosophical viewpoints and especially his opinions on the role of women in society have provided students of intellectual history with a superb example of the influence of personal experiences on the development of ideological systems. Despite the many vagaries of Comte's life and works, he commands the serious attention of sociologists by virtue of his unquestioned position as godfather and "founder-in-chief," as Hankins once called him, of sociology.

Lacroix's introduction to the sociology of Auguste Comte offers no particularly fresh interpretations, but his succinct formulations and his evident efforts to identify the constructive elements of Comte's work render a valuable service to those who have not explored the original writings. He concentrates on exposition and explanation of Comte's major ideas, with little reference to their influence or impact on later developments. There are only incidental references to pertinent historical or biographical information.

After discussing the interrelations of philosophy, sociology, and political action and identifying Comte's predecessors, Lacroix reviews Comte's criticisms of political economy, his conceptions of social order, progress, and social change, his philosophy of history with its essentially Hegelian overtones, his views on ethics and religion, and his more specific observations on the family as the "social cell" and on the significance of the nation, women, and labor in the newly emerging industrial social order.

With inexcusable parochialism, Lacroix fails to make reference to American interpretations of Comte, especially Gordon Allport's analysis of him as discoverer and founder of social psychology and the efforts at reconstruction of his sociology by the late McQuilkin DeGrange. *The Nature and Elements of Sociology* by DeGrange is perhaps the most serious effort to interpret its positive elements in the light of contemporary developments.

On the occasion of the observance by the American Sociological Society of the centenary of Comte's coining of the term "sociology," in 1938, DeGrange predicted that "when, in 1957, this Society meets to commemorate the centenary of Comte's death, his stature as a creative thinker will loom still larger than it does today." It is a sad commentary on the hazards of prophecy that the announced plans for the 1957 annual meeting of the American Sociological Society include no reference whatsoever to Auguste Comte.

HARRY ALPERT

National Science Foundation

Position and Subject-Matter of Criminology. By H. BIANCHI. Amsterdam: North-Holland Publishing Co., 1956. Pp. viii+216. Guilders 12.

This book might serve a variety of purposes. It could help establish, for instance, the concept of intellectual obscenity and thereby deflect the interest of custom inspectors and censors from more worthy publications; or it could provide a good reason for breaking off diplomatic relations with the Netherlands; but it will hardly accomplish its avowed purpose of establishing criminology as a science. It is a book on the philosophy of crime, if there be such a thing, written in abominable English, which, I suspect, is the translation of a Germanic original. To such sentences of the author's as, "Above, around and opposite to the concept of personality, we find the concept of person" (p. 172), "We pass now, once again, on a comparison of the former polarity in criminology of the concepts of disposition and environment on the one hand and the concepts of genuine and ingenuine adequacy on the other" (p. 182), and "It can be understood that one steals something because it is suitable to one's transcendence unto horizon" (p. 201), the printer, if it was he, adds such words as "Forasmuch" (p. 202) or "withinadeficient" (p. 201). If, overcoming such obstacles, I have nevertheless understood the author's aim, it is to establish criminology as a *Geisteswissenschaft*, so as to elevate it from the lowlands of a "science which searches for . . . laws of causation . . .," a "conception of criminology" which, of all horrors, "even today . . . scholars" are "advocating." He attempts this on the preposterous assumption that a science can be established without fact or theory, merely by metaphysical discourse.

In understandable modesty, not a footnote is given which would reveal the author's identity or background; even his sex is hidden behind an initial. The fact that the publisher forgot to print a copyright clause should cause him no concern.

HANS ZEISEL

University of Chicago

Handbuch der Soziologie. 2 vols. Edited by WERNER ZIEGENFUSS. Stuttgart: Ferdinand Enke, 1955. Vol. I: Pp. 611. DM. 62. Vol. II: Pp. 1,234. DM. 73.

To those familiar with sociology and related studies in Germany before the Nazi seizure of power, the apparently sudden re-emergence of work of a high quality would seem inexplicable if they were in ignorance of what may be called the persisting "sociological underground." The writers represented in the symposium now under review clearly had their attention fixed on the main issues of sociology long before the Nazi collapse. This work was not conjured up overnight.

Heinz Maus leads off with a remarkably compact and yet fairly comprehensive history of sociology in 120 pages. It barely goes back of the nineteenth century, which, from a certain standpoint, is justifiable enough. It might have been well, however, to deal with fewer persons in the main text and to make extensive use of footnotes for the remainder. As it is, there is a certain embarrassment of riches. Of special interest to the readers of this journal is of course the discussion of leading figures in American sociology: it is astonishingly thorough and, with a few exceptions, up to date. In particular, his comments on current trends in American sociology are very shrewd indeed. If there is any shortcoming, it is the *very* sketchy discussion of sociology in countries other than Britain, France, Germany, and the United States. Japan, for example, is dealt with in little more than half a page.

The next chapter, by Werner Ziegenfuss, is a very good topical survey of the various fields of sociology and its conceptual apparatus. The sociologists most heavily drawn upon are almost without exception German. One of the most important parts of Ziegenfuss's interesting presentation is his critique of Max Weber. He shows that Weber never regarded *Verstehen* as more than the basic source of hypotheses and hence as offering *no* validation in its own right. In-

deed, Weber says explicitly that all the customary methods of causal inference, and particularly statistical procedures, should be applied to hypotheses derived from *Verstehen*. Ziegenfuss regards this as a weakness, whereas the reviewer takes it as evidence of Weber's commendable hardheadedness. As long ago as 1932, the present reviewer called attention to this feature of Weber's method (Wiese-Becker, *Systematic Sociology*, p. 57, n. 10).

The chapter on social anthropology written by Friedrich Keiter, who possesses a medical degree in addition to his social science doctorate, is a remarkably competent job. Social anthropology, as it is here defined, coincides with certain aspects of what we would call "physical anthropology" and with some kinds of intelligence-testing.

Alas! the generally high level of the chapters is not sustained in the one on social psychology by Willy Hellpach, recently deceased. It is a disastrously confused mixture—eclectic in the worst sense—of venturesome conjectures, premature conclusions, newly coined terminology, and antiquated references. There is no trace of the fruitful suggestions, stemming from Hegel's *Phenomenology of Mind*, that issued in the works of Mead, Scheler, Litt, and other founders of basic social psychology.

Fortunately, the immediately following chapter on social statistics, by Charlotte Lorenz, swings back to the higher level. It provides a splendid survey of the various fields of social statistics, gives excellent examples in seventeen tables, and makes apparent the wide practical experience of the writer.

The ensuing chapter, on demography, is also by Lorenz; here again there are an excellent survey of the many divisions of the field and helpful illustrations in twenty-seven tables. The chapter comprises close to a hundred pages but is tightly organized and concisely written.

Two chapters then follow dealing with the sociological utilization of historical evidence and at the same time with theories of social change. One, by Karl Muhs, discusses the relevant problems from the standpoint of "historical idealism"; the other, by Leo Kofler, of "historical materialism." Although both chapters are of first-rate quality, it is regrettable that present ideological conflicts should have been so directly reflected in a sociological handbook. Both Muhs and Kofler clearly show that they are by no means wedded to the positions they expound,

and a wise editorial policy would have required co-operative treatment of the fundamental themes.

Otto Stammer provides the chapter on political science. At the outset, he discusses the newly arisen "science of politics" that has been so favored by many American civil affairs and educational officers during the Military Government and High Commission periods. Stammer, however, after an eminently fair presentation, indicates that the "science of politics" is an unstable amalgam of current events of political import, politicized history of traditional type, analysis of administrative procedures, constitutional law, and studies of power elites. He then takes the position that sociology of politics is a more sharply defined field, that its methods are less vague and diffuse, and that it conforms with greater exactness to the requirements of a specialized discipline. Stammer offers an excellent outline of sociology of politics per se. The two or three pages devoted to bureaucracy show familiarity with American as well as German treatments. Moreover, the general bibliography is amazingly comprehensive—a fitting conclusion to the first volume of the handbook.

The basis for the division into two volumes is essentially that between the abstract treatment of the fields of sociology in general and the more concrete presentation of various aspects of what we have come to call social organization in particular.

The social-organizational division then opens with a chapter on the non-human animals by Hans M. Peters which shows that the professional biases of sociologists and social and cultural anthropologists (in the British and American senses) have led to the ignoring of highly significant data on the various forms of association and dissociation among mammals.

Friedrich Keiter now returns with a splendid chapter on the primitive peoples, entirely free from the negative value-judgments sometimes implied by "primitive." What he offers is social anthropology in the British sense; he is not much concerned with culture history. His use of the term "culture," however, entirely coincides with that established long ago by Pufendorf and more recently disseminated by the followers of Klemm, Waitz, and Tylor. Keiter makes much use of diagrammatic exposition, lists of salient characteristics, and so on. Nowhere in the literature familiar to the reviewer is any such concise and informative presentation available, nor does he know of any bibliog-

raphy so carefully selected and covering so wide a range.

The indefatigable Keiter also provides a chapter on "Fundamental Kinds of Societal-Cultural Life Processes." In many respects of psychological and social-psychological character, it also deals with the basic sociological processes of association and dissociation, together with a considerable number of subordinate processes. In addition, a fair amount of attention is given to problems of cultural integration, rhythms and cycles in the cultures of determinable historical periods, and the role of values in conduct. Unity in this diversity is not easy to discover. Some of the topics are handled in strikingly suggestive ways, but there is no space for sustained analysis in the sixty-five pages allotted. The bibliography, although very full, seems a little miscellaneous.

Primary groups are presented by Otto von der Gablentz in a very good although not outstanding chapter. It does not add very much to what we are already familiar with in the United States, although here and there the indications of recent German research are instructive. The bibliography is scanty.

Where major social structures are concerned, the treatment provided by Friedrich von der Heydte is distinctly worthwhile. Much influenced by Leopold von Wiese (although Werner Sombart receives greater prominence), the chapter is tightly organized, clearly written, and full of fruitful ideas for research. The bibliography is reasonably good, but there is insufficient reference to recent American work.

What are called the "Formative Forces of Society" takes up the next main division of the handbook, and the lead-off chapter, on sociology of religion, is provided by Gustave Mensching. Mensching's earlier book on this topic is well known, and the 33 pages here offered represent a good summary—in some places, however, going considerably beyond it. The bibliography has been purposely kept brief by reference to that of Wach's *Sociology of Religion*.

Karl Weiss offers one of the genuinely sociological treatments of education. It is to be regretted, though, that it remains at a somewhat too general level. The bibliography, although not lengthy, is very well selected and comprehensive.

Sociology of law is handled by Hermann Eichler in so few pages that it cannot be other than superficial. Reference is made at several points to Max Weber's work in the field, but

there is little or no evidence of his descriptive scope and analytic power. The bibliography is good but not always relevant.

The difference between the third and fourth main sections of the handbook is hard to see; the title of the latter is "Formative Realms of Society." Friedrich von der Heydte is the author of the chapter on the sociology of the state. He draws heavily on Leopold von Wiese, Alfred von Martin, and others. There seems little that is new or significant, perhaps as a result of the fact that there is too much discussion of theories rather than the development of a substantive theory. The bibliography is fair but lacking in focus.

A startling contrast is evoked by the chapter on sociology of economics, by Gerhard Weisser, who makes the flat assertion that economics is a special branch of sociology and cannot be otherwise handled. The chapter, one of the longest (132 pages) but also one of the most rewarding, compares very favorably with the recent Parsons and Smelser *Economy and Society*. The contrast between the doctrinaire treatment in the latter and Weisser's closely organized but flexible presentation is impressive. Weisser's not only is among the very best chapters in the entire handbook but is also a distinguished contribution in its own right. The bibliography is full, drawn from the relevant literature in several languages, and carefully selected.

The concluding chapter, on culture, by Franz Zwilgmeyer, seems to come at the wrong place. This is partly the result of the fact that, although culture is explicitly defined and in early sections treated along lines standard in the recent social science literature, the effect of the Alfred Weber "culture-civilization" split brings about an implicit change in the essential meaning of the term as applied in the later parts of the chapter. But this is not to speak disparagingly of Zwilgmeyer's excellent presentation; he has centered on pertinent issues and shows considerable erudition. The bibliography, though by no means remarkable, includes important items from several languages.

In sum, it seems safe to say that the contributors to this handbook show a degree of acquaintance with current sociology that would be hard to match in any country and that the speed of Germany's material reconstruction finds a striking parallel in its intellectual life. Here appear several developments that, although showing how much continuity was preserved by the "sociological underground," also

manifest new departures of great significance. If it was ever true that everything of any importance in sociology is now available in English (and it never was!), this handbook introduces a necessary modification.

HOWARD BECKER

University of Wisconsin

Constraint and Variety in American Education.

By DAVID RIESMAN. Lincoln: University of Nebraska Press, 1956. Pp. 160. \$2.75.

This little book consists of three lectures on American education given by David Riesman at the University of Nebraska, the first in a series of University Lectures in the Humanities. He deals with the varieties of American university education, the social relations between the social sciences, and the changing demands on secondary school education.

As description this book has to a high degree the wonderful Riesman qualities: an utter inability to look at anything like the rest of us with eyes fixed in a frame. It is the movement of the head that lets us see things in the dark, and so with Riesman. So, too, a catholic sympathy with any human endeavor setting some excellence as its goal, so long as Riesman does not have to commit *himself* to that excellence. You will not find here any outright damning, of "progressive" education, for instance. Indeed, Riesman's program is countercyclical: to be just enough *en avant* of the *avant-garde* to begin finding something to praise in what they are just beginning to condemn.

This is the policy of a nimble buyer who gets in the market when the rest are out; but are we buyers? Faithfully pursued, the countercyclical program would damp the curves of the academic snake (his own metaphor), but, unless it twists, a snake loses traction. For instance, "there has been a movement in anthropology away from the culture-and-personality school, a renewed preoccupation with special methodologies, an almost pathetic eagerness to show that anthropologists of either sex are as tough-minded, hard-headed scientists as any critic from another discipline could ask." Does Riesman look on this as just another twist of the snake, just another, rather less interesting, reaction against an earlier, rather more interesting, one? The dread hour comes when we stop merely entertaining ideas—and our students—and work, indeed as scientists, by grim old Puritan stand-

ards. Then, we ask if there are propositions and if they are demonstrably true. Few, then, will be the culture-and-personality propositions we shall answer "Yes" to. I do not mean there is nothing "to" culture-and-personality but only that we need not interpret the behavior of these anthropologists by a theory whose present inadequacy they themselves have felt. They are not just trying on for size a new personality style but seriously and rightly remembering that they carry a savage *Veritas* on their shield.

Nor do I mean that Riesman himself is not truthful: he is so detached he finds us the truth in plain sight. But he is working on being "autonomous," and it is tough going: he cannot relax and be rigid. He must see some good in everything, while discounting the rigid standards, like truth, that made people think it good and let them set their teeth in it. Trapped by autonomy, he caresses the curves in the academic snake with the precision of a tired lover. But what's the snake out for? As far as we can tell from Riesman, "not to eat, not for love, but just gliding."

But this has nothing to do with the interest of what he says, which is enormous.

GEORGE C. HOMANS

Harvard University

A Study of Thinking. By J. S. BRUNER, J. J. GOODNOW, and G. A. AUSTIN. New York: John Wiley & Sons, 1956. Pp. xi+330. \$5.50.

The problem of grouping together certain events or objects, labeling them, and distinguishing them from other classes of events or objects makes the core of this book. If its apparent simplicity provokes the reader to think about the problem a bit, he will discover that it is not simple at all. There are usually dozens of ways in which the same set of objects can be categorized—how does one go about choosing one rather than another system? To which distinguishing features of the objects or events does one attend and which does he ignore? Are special problems introduced when the labeling or categorization is going to produce, at best, a probabilistic result? Are successful and unsuccessful instances of categorization equally informative?

The opening chapters of the book impress one with the complexity of the problem which initially looks so simple. The authors' analysis

demonstrates why the problem of human thinking has been so very difficult for psychological research. The book, of course, does not deal with the multitude of problems connected with human thinking: it discusses almost exclusively the attainment of correct concepts under a variety of conditions; but this subject, in itself, is large enough.

The gratifying thing is not only the way the problem is analyzed but, even more, how it is put back together again. In later chapters one begins to see how the problems can be ordered and, in a sense, connected by studying the strategies which can be, and are, employed in choosing the correct concept; by studying the kinds of concepts which people seem to prefer and those they find difficult to entertain; and by studying the characteristics of cues which will command attention and those which tend to be overlooked. Indeed, it would not be unfair to say that this volume's major contribution is to put the problems together systematically in a manner which now makes them amenable to definitive research. As a result of the careful and sometimes remarkably ingenious thinking of the authors, one can now see how to approach the problems and make progress.

The authors, however, do not content themselves with systematization. They have carried out and report a number of experimental studies on categorizing and concept attainment. These, in addition to making their analysis concrete and understandable, sometimes yield unexpected and interesting findings; for example, that negative instances are not used so effectively as positive instances toward achieving the correct concept. As a matter of fact, it is quite a common practice to avoid testing an instance which is likely to turn out negative even though this is by far the most efficient procedure. But, in spite of such occasionally interesting findings, the empirical studies form the weakest portion of the book. They are too often mainly descriptive and illustrative; too rarely are they ingenious enough to cast new light on the problem.

The sixty-five-page appendix, "Language and Categories," by R. W. Brown, is an excellent and readily understandable statement of linguistics and its relevance for a variety of problems in psychology. It is disappointing that it is not tied up with the rest of the material; nevertheless, it is valuable all by itself.

LEON FESTINGER

Stanford University

Psychology, Psychiatry, and the Public Interest.

Edited by MAURICE H. KROUT. Minneapolis: University of Minnesota Press, 1956. Pp. xv+217. \$4.00.

This book has two aims: to make peace between psychiatry and clinical psychology by airing their differences and to inform the public about the two professions. The need for clarification became acute recently when attempts to get states and municipalities to grant licenses for clinical psychologists to practice psychotherapy privately showed that the public did not know the difference between psychiatry and psychology and were not even clear about what psychotherapy was.

The book is divided into four parts. In the first the increasing demand for psychotherapy is stated though not very satisfactorily explained. Nor are we told very clearly what psychotherapy is or does. The historical development of psychiatry and psychology is described. Their difficulties in tolerating each other are attributed to insecurity.

The second part states the view of organized psychiatry and psychoanalysis that emotional problems are an illness and should be treated by medically qualified persons. Psychologists may do research and diagnostic testing, but they should not be licensed to practice psychotherapy privately: the latter work calls for supervision by a psychiatrist. Oddly, although there are two contributions by a psychiatrist, there is none here by a medical psychoanalyst.

The third part presents the views of five clinical psychologists working in collaboration with psychiatrists. In general, they feel that psychiatrists should handle psychotics and physical treatments and that psychologists should handle research and testing. But about psychotherapy they are hesitant and seem not to have clear views. There is a good deal of vague talk about democracy, co-operation, and mutual understanding. They stress that psychologists and psychiatrists work well together as individuals and that it is only on the level of organized professional bodies that conflicts arise.

The last part presents the views of seven psychologists in private practice. They speak out clearly. Psychiatrists should deal with organic and functional psychoses and with psychosomatic disorders. Non-medical psychotherapists should be licensed in private practice with character disorders, neuroses, behavior problems of children, and so forth.

If a clear public statement of conflicting views leads to peace, this book should achieve its first aim. But it seems more likely that the war will continue. The second aim has been achieved: public bodies can now find out what are the differences between psychiatry and clinical psychology. But whether they will thereby be in a better position to know what to do about it is more doubtful. Neither existing formal training in psychiatry nor existing formal training in clinical psychology seems an adequate preparation for responsible psychotherapeutic practice. As several contributors point out, psychotherapy is best taught in some sort of apprenticeship—by practice with actual cases under systematic supervision for a considerable period of time until supervisor and student feel the latter can cope by himself. Perhaps a partial solution might be a program in which both psychiatrists and psychologists go through the same apprenticeship in psychotherapy before being licensed for private practice.

ELIZABETH BOTT

Tavistock Institute of Human Relations
London, England

Competitive Pressure and Democratic Consent: An Interpretation of the 1952 Presidential Election. By MORRIS JANOWITZ and DWAYNE MARVICK. ("University of Michigan Governmental Studies," No. 32.) Ann Arbor, Michigan: University of Michigan, Bureau of Government Institute of Public Administration, 1956. Pp. viii+122. \$2.75.

Political scientists feel that surveys of the voting habits of people with varying social and psychological characteristics do not provide the data necessary to evaluate the democratic process. It is therefore encouraging to find a study which sets out to test empirically some of the classical conceptions of the political process. The data were collected by the Survey Research Center during the 1952 election and were in part reported in *The Voter Decides*; the present study uses the same material but presents data not reported in the monograph. The central question posed is: to what extent was the 1952 election a process of genuine consent, making for effective representative government, and to what extent was it mass manipulation?

The authors claim that five conditions must be met in order to indicate consent: a high level of participation among all social groups; a feel-

ing that one can influence the political process, either for the social welfare or for one's personal interests; the presence of effective political deliberation on the issues and candidates; political competition in the mass media; and the existence of influence of person on person, substantially independent of the media. From the evidence the conclusion is reached that the 1952 election campaign did, indeed, result in political change in the presidential office, as the result of the process of consent. By each of the five criteria, consent is evident (p. 96).

But it is difficult to determine what is meant by consent, why these are the "crucial" criteria, and whether the facts show that the criteria have been met. For example, does the fact that 74.2 per cent of the electorate voted indicate a high or a low level of participation? What would be a "good" turnout? We are given no standard of judgment supported by sound argument, and there is no way of telling. With regard to the question of whether the electorate manifested "high political self-confidence as well as self-interest," the answer must depend on the particular measure used and on some reasoned decision regarding how "high" political self-confidence must be in order to indicate a process of consent. In other words, the difficulty is not simply that the empirical data are inadequate but that these questions are not answerable by empirical data.

This is, of course, a secondary analysis, based upon data collected by other people for other purposes. There are certainly simpler and more direct ways of determining the degree to which an election reflects consent. If this monograph has not satisfactorily answered the questions, it has nevertheless provided information regarding the influence of social class, the mass media, and personal pressures on the voting decision which adds materially to our knowledge of voting.

MORRIS ROSENBERG

Center for Advanced Study
in the Behavioral Sciences

Work and Authority in Industry: Ideologies of Management in the Course of Industrialization. By REINHARD BENDIX. New York: John Wiley & Sons, 1956. Pp. xxv+466. \$7.50.

Continuing his work on bureaucracy and the sociology of business, Bendix develops in this

book a broadly comparative analysis of the ideologies drawn upon by management to justify subordinating large masses of men to the discipline of factory work and to their authority. The purpose is to correct distorted ideas of men in the past who reacted imaginatively to factory conditions with which they were not intimately familiar, exemplified by De Tocqueville and Marx.

Following a preliminary development of the theme in the first chapter, the second and third chapters serve to contrast the earlier development of managerial ideologies in England under free institutions and in Russia under autocracy, emphasizing the eighteenth and nineteenth centuries. The fourth chapter analyzes the bureaucratization of economic institutions associated with the transition from smaller family-oriented enterprises to the large-scale corporate forms of the later nineteenth century and after. The fifth and sixth chapters compare the ideologies of large-scale economic organizations of free and authoritarian regimes in the United States and East Germany, the former from the turn of the century onward and the latter under Russian control following World War II.

The chapter on England draws selectively from the rich literature of economic and social history and demonstrates the value of such materials to sociological analysis of ideological metamorphoses in the area of industrial management. The chapter on Russia draws upon the literature on that country available mainly in German works and not easily accessible to American sociologists. The discussion of America covers ground already analyzed by several economists, notably Clarence Bonnett and Robert A. Brady, but from different perspectives and with additions of substantial new materials and insights. Bringing these sources to the attention of sociologists is a contribution in itself. The analysis of the situation in East Germany under Russian Communist domination is very valuable for its substantive materials and interpretations, the general validity of which seems clearly demonstrated already by subsequent events in the satellite countries.

Bendix' analysis of bureaucratization is an important theoretical contribution. His classification of business leaders as entrepreneurs, bureaucrats, and heirs, derived from his independent analysis of biographical sources, is clearly confirmed by implication in recent findings. Clark Kerr takes issue with this classification in his Introduction on the ground that it would be

better to classify business leaders in terms of their essential approach to problems, admitting, however, that this would involve a phenomenon less easy to measure. Implications of the crucial role of proprietorship—individual, family, corporate, and governmental—in the metamorphosis of economic institutions are clearly apparent in this analysis as well as in the substantive chapters.

The task Bendix has undertaken is most formidable. The materials and methods in the main are those of traditional historiography, ably applied to the broad problem of the sociology of economic organization within the special limits of the theme of managerial ideologies. He exhibits scholarship of a high order of excellence. The analysis throughout contains a large number of new and perceptive insights. The exposition is well organized and unusually lucid. The book presents a type of research particularly needed in American sociology where quantities of documentary sources lie fallow and historical depth is generally absent.

WILLIAM C. LAWTON

Western Kentucky State College

The Strange Career of Jim Crow. By C. VANN WOODWARD. New York: Oxford University Press, 1955. Pp. xi+155. \$2.50.

In *The Strange Career of Jim Crow* a historian re-examines the view held by many social scientists that racial segregation in the American South is a natural outgrowth of the slave system. As shown by Myrdal (*An American Dilemma*), Robert E. Park was one of the outstanding representatives of this point of view. In an effort to demonstrate that the insistence on racial segregation by southern white people was not based upon "deep, instinctive antipathy for the Negro race," Park states: "Anyone who accepts that conception of the matter is likely to be somewhat mystified when he learns that the Negro is quite all right in his place. And that place, like the place of anyone else, is the one to which tradition and custom have assigned him."

To Park, the post bellum pattern of race relations was a caste system so "firmly fixed in the habits and custom—what Sumner calls the mores—of both races in the South that all the social disorganization incident to the Civil War and Reconstruction were not sufficient wholly or suddenly to destroy it." It follows that laws

designed to change social relations between whites and Negroes cannot be effective—"stateways cannot change folkways." Myrdal suggests that Jim Crow legislation provides an excellent test case for this a priori notion.

Vann Woodward, in dealing with this problem, has assembled evidence from a variety of sources that, between the Civil War and 1890, it was rather common for whites to mingle freely with Negroes in public accommodations without hostility or resentment. Segregation and the disfranchisement of the Negro, which are often described as representing the "immutable 'folkways' of the South," impervious alike to legislative reform and armed intervention, are of a more recent origin.

Convincingly, the author argues that segregation and disfranchisement of the Negro did not automatically follow the overthrow of Reconstruction governments in the South. These devices for the subordination of the Negro developed slowly and often against strong resistance from southern whites. Their origin is to be sought in the frustration and aggression which inevitably followed the political and economic disorganization after the Civil War and the end of slavery upon which the social order of the South was based. Here was created "the perfect cultural seedbed for aggression against the minority race."

This work sheds interesting light not merely upon the career of Jim Crow but upon the fate of Jim Crow through the analysis of political conflict and social and economic change. Developments in race relations resulting in the relatively rapid change in the status of the Negro, which has taken place in recent years, are analyzed against the background of economic and political changes. The complex of forces generated by economic expansion and industrialization, the northward migration and urbanization of the Negro population, and the ideological impact of World War II and the present cold war have provided conditions which have made possible the development of a movement to abolish racial segregation.

In challenging the basic assumptions as to the nature of the racial system in the South, this work not only is provocative but should provide a new orientation for those interested in research in race relations and for the practitioner who is concerned with the elimination of racial segregation.

HARRY J. WALKER

Howard University

A Catholic Runs for President: The Campaign of 1928. By EDMUND MOORE. New York: Ronald Press Co., 1956. Pp. xv+220. \$3.50.

This book is a description of the influence of the Catholic affiliation of Al Smith on the 1928 campaign. Moore, a historian, demonstrates that, although other things entered in, Smith's religion played a "large, though not wholly calculable, part" in his defeat.

The author's thesis is documented by material drawn chiefly from contemporary newspapers and periodicals and from the private papers of key figures. A large number of quotations are marshaled to show that many Democrats opposed Smith's nomination and election with anti-Catholic arguments. Many of the symbols of the American nativistic movements of the nineteenth century appeared again, especially in the openly anti-Catholic areas where the Klan was a major political force.

Exclusive concern with the nativistic elements in the Smith campaign has blinded the author to significant aspects of the election. Samuel Lubell, nowhere mentioned by Moore, has shown that Smith's candidacy engendered a great deal of new support for the Democratic party, especially in the northern urban areas. It marked the rising importance of urban forces in the Democratic party—a facet of the election casually treated by Moore. Surely we can expect that an analysis of Smith's Catholicism would also include its vote-getting, as well as its vote-repelling, potency.

Throughout the book there is a distinct naïveté about the relation of cultural characteristics to political symbols. The author appears to assume that the cultural identity of a candidate is either unimportant or unrelated to his religious affiliation. Voters who look at a candidate's religion are dismissed as "irrational." The function of Smith's Catholicism in winning urban voters of all faiths is not discussed.

Moore follows the story of the campaign from the convention of 1924, when Smith failed to gain candidacy, through the 1928 election. There are chapters on anti-Catholic movements before World War I and on the role of Prohibition as an issue. Otherwise the organization is chiefly chronological. The sociologist concerned with cultural relations or with political behavior will not find this book very useful.

JOSEPH R. GUSFIELD

University of Illinois

... *wohnen in Wien: Ergebnisse und Folgerungen aus einer Untersuchung von wiener Wohnverhältnissen, Wohnwünschen und städtischer Umwelt*. ("Der Aufbau," Mono. 8.) By THE VIENNA MUNICIPAL HOUSING AUTHORITY. Vienna: Verlag für Jugend und Volk, Ges. m.b.H., 1956. Pp. 108.

... *wohnen in Wien* represents one of the first studies in empirical sociology to be conducted in Austria since Marie Jahoda and Paul Lazarsfeld's well-known analysis of a suburban community during the depression of the late twenties. In 1953 the Social Science Laboratory of the University of Vienna, under the directorship of Dr. Leopold Rosenmayr in conjunction with the Planning Office of the City Administration, launched a series of investigations on the sociological aspects of housing in Vienna. Despite the considerable resistance to interviewing engendered by eight years of national socialism and nearly ten more of four-power occupation, the study produced an imaginative contribution to comparative urban sociology.

Reported in this monograph are four separate but closely interconnected studies. In the tradition of European sociography, Gustave Krall presents an essay, "Grundlagenforschung für die wiener Stadtplanung" ("Basic Research for the Vienna City Plan") in which he traces the historical development of the social ecology of the city and its main residential quarters. The industrial base of Vienna and its housing requirements, as based on a sample survey, are discussed by Anton Schimka, a planning engineer on the staff of the city administration, in "Wiens städtische Entwicklung und planerische Aufgaben" ("The Development of Vienna and Its Planning Goals"). By a comparison of a group of mentally disturbed patients with a sample of "normals," Dr. Hans Strotska undertook an exploratory investigation into social psychiatry with special emphasis on housing variables: "Spannungen und Lösungsversuche in städtischer Umgebung" ("Tensions and the Search for Solutions in the Urban Environment").

The main sociological contribution is in the report by Rosenmayr, "Wohnverhältnisse und Nachbarschaftsbeziehungen" ("Housing and Social Relations"). The results of a carefully executed survey describe a housing culture markedly different not only from the United States but also from that of postwar Germany; they involved a written questionnaire administered by 100 interviewers to 4,108 family units, cov-

ering topics such as styles of life, desired type and location of housing, community facilities, expectations about housing standards of living, and social relations within neighborhoods.

Following the trends in contemporary social research, Rosenmayr assumes that the residential sphere of life and the stable necessity of living quarters supply a point of entrance in conjunction with the analysis of social relations in the place of work. He concludes from his data that "home culture" in Vienna is relatively low, despite the large percentage of the population who spend their leisure time without relying on mass amusements. He finds little initiative in making the home a vital center of private life—this from the evidence of reported style of life and interest in the home. Neighborliness is seen as extremely meager, although in part this is a function of a rather rigid test in which only 16 per cent reported attachment to the people they lived with as the reason for liking to remain there. The analysis rests on stronger ground when Rosenmayr points out that this negative orientation toward "home culture" rests on a basic desire to avoid the procreation of children. Vienna has the lowest birth rate of all European metropolitan areas. Urban sociologists in the postwar decade have been collecting important data on the interconnection between the family life-cycle, the birth rate, and the development of residential and community cohesion. Here is impressive negative evidence confirming these observations.

Therefore, the marked contrast to attitudes toward suburbanization encountered in the United States and in other European countries becomes more understandable. The Viennese population—true to the foreigners' stereotypes—showed little interest in living outside the densely settled core of the city. Forty-five per cent want to remain within the boundaries of the traditional city, and only 18 per cent are willing to go to the suburbs. This is in contrast to a repeatedly encountered average of 70 per cent desiring suburban styles of life in surveys of western Germany.

While the author stressed that the demand for the suburban way of life could be stimulated by better transportation, Vienna and the Viennese are different for historical and contemporary reasons. The traditional high prestige of the residential districts around the historical nucleus of the city attracts people to the nineteenth-century style of urban living. What seems to be at work is the consequence of the slow and

very halting industrial expansion since the collapse of the Austro-Hungarian empire. The standard of living, the transportation system, the standardization of housing production, and even the credit facilities—all the preconditions for both suburban life and a desire for it—have not yet fully developed. Even the relative persistence of the patriarchal family seems to mitigate against the desire for suburban life, in contrast to the American scene. Women tend to seek outlets outside the domestic milieu, and preoccupation with a suburban home where they would have a relatively independent and creative life is undeveloped.

All this, plus the strong reaction to National Socialistic efforts to enforce a spirit of community, converge to produce what Rosenmayr calls "social negativism." The outlets for social negativism are more immediate personal gratification, food, clothing, and travel, particularly the well-known two-seater scooter—not a concern with housing. Under these circumstances, communal interests center around the small shop "around the corner" and the communal room for laundry purposes in apartment houses.

One cannot read this stimulating study without trying to answer the problems that face the Viennese social planners. To describe the structure of existing wants may be highly relevant for catering to the special needs of Vienna. Perhaps the social planners will be strengthened in their inclination to conserve historical traditions and develop Vienna without extensive suburbs. However, to describe these existing wants may be somewhat misleading. The portrait of "home culture" seems to be in important respects like that of western Europe around 1950, when economic recovery was first paying dividends to the population at large. In the hierarchy of wants, housing was low: first things first. However, if the Austrian standard of living is linking itself to western Europe and if it continues to rise, the desire for housing will grow and the conventional pressures for suburban living will begin to be felt.

MORRIS JANOWITZ

University of Michigan

This book consists of a short introduction by the editor, together with abridgments and and selections from four published American research studies and a reprinting of one. The studies are *Interracial Housing*, by Morton Deutsch and Mary Evans Collins; *The People's Choice*, by Paul F. Lazarsfeld, Bernard R. Berelson, and Henri Gaudet; *Dynamics of Bureaucracy*, by Peter M. Blau; *Union Democracy*, by S. M. Lipsett, Martin A. Trow, and James S. Coleman; and *Biographies in Popular Magazines*, by Leo Lowenthal.

Main guidance to the aims of the book is given by the publisher's statement that it is intended "to present to laymen the work of modern American social scientists at its best, and to show how contemporary social science contributes to an understanding of American social life and its problems. . . . The reader can see just how the modern social scientist goes about his work, tests his theories, and comes to his conclusions."

Are original studies necessary to inform the layman? An introduction to a subject is probably better given by one sustained exposition than by five studies differing widely in theoretical level and empirical focus. The editor has limited himself to writing a six-page Introduction, briefly drawing attention to the variety of work in progress in American social science, telling the reader what the studies included are about, and pointing to their common elements. The Introduction and the five studies are less likely to be of use to the reader making his first acquaintance with the social sciences than to one sufficiently sophisticated to pick his own way through the book.

Inasmuch as copies of the book will filter across the Atlantic, the question arises of how far it will help convince British readers that "sociology . . . is the one [social science discipline] that offers the most promise for the future" (Introduction, p. 2). In England, studies of contemporary society have been developed to a high level at the contrasting extremes of quasi-philosophic reflections on fundamental social issues and demographic-statistical investigations. An obstacle to advancement in the underdeveloped areas between these fields of concentration has been what is considered here to be the indiscriminating empiricism of the American sociologist. The British reader (and surely there are many Americans in the same position) usually has little respect for the results of the work of those social scientists de-

American Social Patterns. Edited by WILLIAM PETERSON. ("Anchor Books.") Garden City, N.Y.: Doubleday & Co., 1956. Pp. 259. \$0.95.

scribed by Peterson as persons who leave the academic cloister armed with significant questions, grapple with the raw data they find, and bring these into some sort of temporary order. Such a reader, picking up Mr. Peterson's book, will probably be unfamiliar with the better American empirical work and with the names of the authors of the studies included in it; his initial reaction may well be to categorize these at the same low level of empirical investigations. This is a pity, because most of the studies chosen by Peterson are of good quality. His book could have been used to remind such readers that among the plethora of American empirical works there are some of very high standard and value. The editor could usefully have taken a more active hand with his material and worked harder with his readers to show them more clearly to what theoretical considerations and practical issues the studies contribute, and how. One must not overrate the capacity or the desire of strangers to make strenuous intellectual efforts of their own accord to identify themselves with us as social science specialists and convince themselves of the usefulness of looking at the world as we do.

CYRIL SOFER

Tavistock Institute of Human Relations
London, England

The British New Towns Policy. By LLOYD RODWIN. Cambridge, Mass: Harvard University Press, 1956. Pp. xiii+252. \$7.50.

The American Bar Association has recently announced that it is planning to raise a monument at Runnymede, England, commemorating the right of private ownership of property—land—granted under Magna Carta: a monument to private ownership of land. And yet, in 1909, the home country of Magna Carta gave statutory recognition to the concept of planned, non-private use of land. The Housing and Town Planning Act of 1909 introduced the idea that the land of individual owners ought to be regarded not simply as isolated plots to be developed entirely according to individual taste but as part of a greater whole to be developed as far as possible with due regard to "amenity and convenience in connection with laying out and use of the land and of any neighboring lands." The Ministry of Town and Country Planning, set up by a coalition government in 1943, was the culmination of a number of legis-

lative measures adopted by a succession of Conservative and Labour governments between 1909 and 1943 and designed primarily as remedial devices to alleviate some of the more flagrant social costs of the "market-determined" land use. In view of the expanded mandate of the Town and Country Planning Ministry to insure "consistency and continuity in the framing and execution of a national policy with respect to the use and development of land throughout England and Wales" (p. 19), the Bar Association's hymn to private landownership must be anachronistically humorous.

Although Rodwin devotes two short chapters to Ebenezer Howard, the father of the Garden City Movement, and to some of the "universal" problems arising out of the process of urbanization, e.g., the journey to work, the location of economic activities, population movements, and costs of cities, he allocates the major part of his book, consistent with the book's title, to the British experience in town planning over the last fifteen years and, even more specifically, in the postwar period. In the remaining chapters he traces the history of the New Town Act of 1946, the variety of problems, most of them unanticipated, faced by the planners and almost everybody else in building the new towns, and the lessons learned, as reflected in the modified Town Development Act of 1952 passed by a Conservative government. The book is a case study of the British experiment, with a long chapter (chap. vii, "New Towns: Personal History") devoted to a more detailed consideration of specific problems of each of the fifteen new towns already built or started.

As an economist, Rodwin is much concerned with alternatives, and he raises many more questions than a traditional planner would. He is careful to imply that the available evidence guarantees no clear-cut judgment as to the success or failure of the experiment. "On the admittedly incomplete evidence before us," he writes, "one may wonder whether the policy on the whole has 'paid off' [it is not clear from his discussion what sort of evidence could enable one to find this out]. Suppose, however, there were no new towns. What would have happened instead in the dismal mining villages? In the boom towns induced by sudden expansion? In the dreary suburban extensions of the big city?" (p. 128).

Judging by the Notes, the book is the result of a scholarly and painstaking study of an extensive variety of available source materials,

plus firsthand knowledge acquired on two trips to Britain. That it is a labor of love is by no means hidden. The copious collection of photographs and diagrammatic sketches from the new towns add greatly to the usefulness of the book.

In the Foreword John M. Gaus suggests that "the essence of his study is of universal importance: how can we all live more safely, less wastefully, more richly, in our urban and rural landscapes?" This reviewer subscribes to Mr. Gaus's glowing praise with qualifications. As a social experiment, the new-town policy in Britain would naturally be of interest to social scientists, administrators, politicians, citizens, etc. But the book is addressed primarily to planners—"planning specialists," in Rodwin's own typology—and it has only peripheral interest for other social scientists. This judgment, however, by no means reduces the timely importance of this kind of study, which is as yet a rarity in the professional kit of planners.

A. KESSEL

Chicago, Illinois

Social Stratification: A Comparative Analysis of Structure and Process. By BERNARD BARBER. New York: Harcourt, Brace & Co., 1957. Pp. xix+540. \$6.50.

It is a revealing commentary on the state of our science as well as on the economics of the publishing industry that many major contributions to systematic sociological theory are still marketed under the guise of textbooks. At the same time, one can only congratulate this publisher and his sociology editor on the outstanding quality of their list, this latest addition to which establishes several important landmarks. It presents, for one thing, the first full-length, truly comprehensive review of a rapidly growing field of knowledge; it also attempts to set forth a theory of social stratification based on and systematically related to a general theory of society; and it consistently uses the comparative method, drawing upon a wealth of historical and contemporary materials from both Western and non-Western societies.

The first four chapters are devoted to a systematic exposition of the theory which is further elaborated throughout the length of the book. It is a sophisticated and detailed statement of the structural-functional approach to social stratification. Barber defines social strat-

ification as "a structure of regularized inequality in which men are ranked higher and lower according to the value accorded their various social roles and activities" and social classes as "primarily sets of family groups that would evaluate each other equally and tend to associate on terms of social intimacy." In keeping with this position, he distinguishes economic stratification from social stratification as two separate kinds of social differentiation and holds that despite their large degree of correlation they are "two independent sets of processes and systems in any society."

The choice of definitions depends, of course, upon the purpose of the analysis, but the present reviewer remains unconvinced that these formulations are adequate to account for the full range of this persistent phenomenon. The essence of social stratification lies, after all, not merely in the existence of social inequality but in the fact that social inequality is hereditary: to a greater or lesser degree it is perpetuated from one generation to another. If the individual's class position is determined by the value set upon his social role, the crucial point is that he has that role in part by virtue of his social inheritance. Here economic stratification enters the picture not merely as a related phenomenon but as a basic determinant of stratification. For, as Barber himself points out, discrepancies between social and economic position will disappear in the long run, "because it is hard to maintain a given stratificational [*sic.*] position without a certain amount of income and, conversely, it is not difficult to raise one's children's stratificational position if the income is available to purchase the appropriate training."

The conceptual defects detract little from the value of the book as a whole, however, because Barber does a superb job of reviewing the vast amount of empirical evidence. Four chapters deal with the evaluation and measurement of class position: verbal evaluation, rating scales, intimate social association, and symbolic activities. This is followed by a chapter on class consciousness and one on influence, authority, and power. Two chapters deal with the relationship between social class, socialization, and personality structure. A detailed examination of the various aspects of social mobility extends over four more, and the final chapter of the book is devoted to an analysis of the relationship between social stratification and social change.

Throughout, the reader is impressed with Barber's excellent command of the extensive literature. His skilful utilization of comparative materials, especially his use of data from the history of France and England since medieval times, greatly enriches and fortifies his discussions. The section on "Class Ideologies: The Problem of 'False Consciousness'" and the section on the anomalous display of social class symbols are particularly enlightening. On the other hand, the analysis of the power structure is weak and inadequate: conceptual shortcomings appear to have interfered with an adequate perception of the connections between stratification and power relationships. Although Barber devotes three full pages to a detailed exposition of Warner's Index of Status Characteristics, he fails to mention the serious doubts cast by critics on the reliability and validity of this instrument.

On the whole, the virtues of this book far outweigh its defects. Its systematic and comprehensive exposition and the wealth of materials adduced greatly enhance our understanding of social stratification.

KURT B. MAYER

Brown University

Location and Space-Economy: A General Theory Relating to Industrial Location, Market Areas, Land Use, Trade, and Urban Structure. By WALTER ISARD. New York: Technology Press of Massachusetts Institute of Technology and John Wiley & Sons, Inc., 1956. Pp. xix+350. \$8.75.

The bulk of this volume will be of interest mainly to economic theorists. But it should interest them mightily, for Isard has proposed an ambitious program of integrating classic and contemporary location theory with the well-developed theories of price, production, and trade. Should this result, as the author hopes, in the explicit introduction of spatial considerations into economic arguments and policy prescriptions, both theory and practice would be improved.

Chapter i is described as an introduction not only to the present theoretical treatise but also to a future work on what the author pleases to call "regional science." From the hints given about its projected content, one can guess that it will contain materials closer to interests of the human ecologist and other empirical social scientists than the elaboration, refinement, and

synthesis of the theories of Thünen, Alfred Weber, Lösch, Hoover, and others presented here. It is disappointing to find, for example, that Isard has chosen only to deal with Lösch's formal theory rather than to follow up his numerous empirical examples. While chapter iii presents "Some Empirical Regularities of the Space-Economy," these are apparently regarded as only of illustrative value in demonstrating that, in fact, distance does make a difference.

There is little suggestion that theories should stand or fall according to their ability to explain or predict the form of such regularities and departures from them or that theory might emerge from the process of investigating empirical relationships as well as from a priori considerations. While there are intriguing statements on the importance of an evolutionary approach to location problems—particularly those related to urbanization—Isard considers it more strategic to work first with formulations of static equilibrium.

Among the discussions that may be valuable to human ecologists are the following: a generalization of the Lösch system of market areas to take account of phenomena of agglomeration and the resulting unevenness of population distribution; a tentative outline of the problem of optimum city size (which, however, ignores previous work on the subject); and some theoretical notes on patterns of urban land use, pointing up the formal similarity to patterns of agricultural location.

Perhaps the greatest value of the book is that it offers a comprehensive review of all major location theories in a comparatively accessible form. It attests to the growing interest in this field on the part of American economists, and, together with its sequel, it should open the way to a closer collaboration among scientists interested broadly in the spatial, morphological, and ecological aspects of society.

OTIS DUDLEY DUNCAN

University of Chicago

Science and Economic Development: New Patterns of Living. By RICHARD L. MEIER. New York: John Wiley & Sons, Inc., and Technology Press of Massachusetts Institute of Technology, 1956. Pp. xviii+266. \$6.00.

It is hard to capture the flavor of this strange but fascinating volume in a brief review. The

author is a chemist turned social scientist, philosopher, and economic planner. The problem is the structure of potential economic development in terms, mainly, of physical needs and resources, especially in the poor countries. Can they simply imitate the developed fifth of the world, or will the impending scarcities of energy and materials compel very different patterns of development, especially in the face of probable explosive spurts in growth of population? This problem is considered in the light of potential developments in scientific knowledge and techniques, not merely in vague abstract terms but in considerable quantitative detail. At times, therefore, the book reads almost like science fiction, at others it seems to hover on the edge of technocracy; nevertheless, the performance is impressive; this kind of intellectual effort must be taken seriously by anyone who is concerned about the future of man.

The book opens with a survey of the present world predicament—population increasing rapidly, food supplies inadequate, basic human needs unfulfilled for a large proportion of the world's people. Carbohydrates might be expanded fairly easily; a critical world shortage of proteins, however, is inevitable with even the best of present conventional techniques. The resources of fossil fuels and many critical minerals would soon be exhausted at rates of consumption appropriate to a "rich" world. If a permanent high-level, world-wide civilization is to be established, it must be with techniques very different from what we have now, but still within fairly easy reach of the scientific imagination.

In the second and third chapters the author considers the new foods, new fuels, and new materials and processes which may extricate man from his predicament. The cultivation of micro-organisms, especially algae, seems to open up a new horizon beyond agriculture: these microcultures may produce not only great quantities of protein but also, as by-products of fermentation, methane or even hydrogen: they may therefore be the best means of concentrating solar energy. The atomic energy prospects are bright as far as exhaustible resources are concerned, and an almost illimitable prospect opens out whereby man will get most of the electrical energy and even minerals he needs by gradually grinding down the mountains, extracting energy and materials from the rock, and filling up the ocean beds with the slurry.

So far, Meier stays fairly close to the safe

shores of material technology; however, facing the question of what all this activity is for, he gingerly approaches man as a consuming machine and discusses his requirements for health and comfort. The author has some qualms about equating the comfortable with the good, but it is at least fun to ask what we have to do to be comfortable. There are some very interesting suggestions about the economizing of consumption (this problem of how to get "more" out of a given level of consumption has been scandalously neglected by whoever it is that should have been dealing with it), and a concept emerges of an MASL, or Minimum Adequate Standard of Life, based on the whole on psychophysics. (At current prices this costs about \$331 per head per year in the United States—in case anyone thinks his professional income is inadequate!) It is suggested that a society eager for economic development should impose a ceiling equivalent to the MASL on incomes and devote all surplus beyond this to development!

There follows a fascinating picture of possible patterns of urbanization, with the people displaced from agriculture building themselves vast aggregations of urban villages, interspersed with algae plants and automatic manufacturing establishments, with atomic power plants and quarries off in the hills. The new societies should skip the stage of crowding people into manufactures—automation will take care of that, and labor can go straight into the delightfully visible benefits of construction and services.

It seems unkind to criticize a book like this: as with Dr. Johnson's walking dog, we are so astonished at having this kind of thing done at all that we should not worry about whether it is done well. Indeed, within its limits, it is done well. It has, however, serious limitations which should be capable of correction. There is no adequate discussion of national defense and international order or of disarmament and world government—surely the major problem in world development, for there is a Malthusian problem in the cancerous proliferation of armaments just as serious as the problem of expanding populations. There is also, for the economist, a serious deficiency in the discussion of the place of the price system and of economic institutions in economic development. Meier does not quite avoid that perennial problem of the "planner"—the failure to include uncertainty, liquidity, and the function of "unplanned" social systems in the total pattern of social dy-

namics. He operates a little too much on the "manifest" level without a wholly adequate sense of the damage which the "latent" forces of social life can impose on the best-laid schemes of mice and men. Nevertheless, this is a book for which to be grateful; a book of magnificent scope and broad vision and a certain delightful brashness; it invites criticism in detail and emulation in the large.

K. E. BOULDING

University of Michigan

Rural Sociology: The Strategy of Change. By CHARLES P. LOOMIS and J. ALLAN BEEGLE. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1957. Pp. xvi+488. \$5.50.

This is a simplification and revision of the earlier text by the same authors, *Rural Social Systems*, but it offers considerably less material and thus is much shorter and the terminology is less technical. The literature since publication of the earlier book is adequately represented. Whereas *Rural Social Systems* appears to have been used widely by advanced students, *Rural Sociology* is obviously designed for lower-classmen.

The present text is even more successful in integrating theory and fact than the earlier book, in half as many words and on a less sophisticated level. The first chapter, entitled "Social Systems and Social Change," presents the theoretical organization of the book and defines the major concepts—elements of a social system, the notion of change and processes involved. The definitions are supplemented by a glossary.

Some may question how well students with no sociological background will comprehend such a sentence as the following: "Thus, social-cultural linkage usually requires that the value orientation and the social structure of the change agent system and the target system are brought together in at least temporary closure." Nevertheless, a commendable job has been done, given the objective of organizing present research findings on rural society according to existing theory. This is especially true in consideration of the limitations of space and a nontechnical audience.

A second distinctive feature of the book is the attempt to organize data in terms of important processes of social change. Thus in the discussion of processes at work in each institu-

tion—e.g., the family or the school—communication, decision-making, boundary maintenance, and social-cultural linkage are treated. The dominant organization of the material is structural, that is, the traditional presentation in terms of institutions and organizations is followed, while concepts focusing on interaction, such as leadership, participation, and planning, have a secondary place.

This book follows the earlier one by these authors in sticking to topics usually treated in texts in rural sociology and presenting them in much the same order. The concluding chapter treats of the direction and strategy of change.

HAROLD F. KAUFMAN

Mississippi State College

Population Theory and Policy: Selected Readings. Edited by JOSEPH J. SPENGLER and OTIS DUDLEY DUNCAN. Glencoe, Ill.: Free Press, 1956. Pp. x+522. \$7.50.

The term "population theory" is ambiguous. Sometimes it refers to generalizations which are normative, such as the prescriptions of eugenicists and neo-Malthusians as to the proper size or characteristics of a population. Most population theories are descriptive and consider the prime determinants of population growth or, as in the case of optimum theory, what size of population will yield maximum per capita income under given conditions of capital investment. Generalizations which deal with the level of fertility, others which are concerned only with mortality or migration, and still others on the sources of variation in population characteristics—all these are labeled "population theories," too. The term refers to statements made in common language as well as to mathematical models, to thoughts so vague that they barely deserve to be called conceptual schemes, and to generalizations so limited that it is difficult to distinguish between them and facts. Some form part of the field of economics, while equally important contributions belong to probability theory, sociology, anthropology, and psychology. It would be hard to find another field conventionally taught by sociologists which is so rich in theory. It is to Spengler's and Duncan's credit that they have allowed their anthology to represent the full range of endeavor.

In their introductions to each section of the

book, the editors comment on the variety of data and viewpoints and attempt to show the connections between the various generalizations. Incidentally, these introductions are often more interesting than the papers which they precede. One hopes that Spengler and Duncan will elaborate them into essays at some future date.

I must express disappointment at the readings on the sociological theory of population. It has become a tradition for demographers and sociologists to claim that while sociology has contributed insight into population phenomena, it has neglected to advance an integrated and systematic theory. This view is put forth by Hauser and by Vance in their papers in this volume and is supported by the editors themselves. For instance, the latter are so wary of suggesting there is a sociological *theory* of population that they entitle the sociology section of the book "Socio-cultural *Context* of Population Dynamics" (my italics). Although it is true that no sociological theory of population is as comprehensive and powerful as some formulations by economists, the situation is not nearly so bad as the readings chosen for this volume would indicate. One might mention the long tradition of inquiry, begun by Spencer in the last century and carried forward in the 1920's by Carr-Saunders and since World War II by Davis and Lorimer, into the structural sources of fertility in preindustrial societies. There are also the studies conducted in the Caribbean area, with an eye to their significance for theoretical problems, by people trained at Columbia and Princeton. And what of the important attempt by Kiser, Mishler, and Whelpton to develop an integrated theory of American fertility? Some of these writings may have appeared too recently to be included in the anthology; they may have omitted others because recent papers often are available in books or collections still in print. As the editors say in their Introduction, they were interested in bringing together fugitive literature. Nevertheless, in order to give a more representative picture of contemporary achievement in sociological demography, it would have been wise to include some of these materials. Any one of them, certainly, would have made a more effective introduction to the sociological theory of population than the banal paper by Hiller which opens that section.

But this gap in the readings is insignificant. All practicing sociologists and demographers, teachers of these subjects, and their students

must be grateful to Spengler and Duncan for this treasury of population theory.

ROBERT GUTMAN

Dartmouth College

Theories of Personality. By CALVIN SPRINGER, HALL and GARDNER LINDZEY. New York: John Wiley & Sons, Inc., 1957. Pp. xi+572. \$6.50.

Hall and Lindzey have written an excellent textbook. Modeling their book upon Hilgard's classic *Theories of Learning*, they have surveyed a widespread and varied terrain with thoroughness and reportorial skill.

Seventeen personality theorists are selected for detailed discussion. Six of them—Freud and his analytic dissenters, Jung, Adler, Fromm, Horney, and Sullivan—exemplify the study of the single case and the method of internal consistency. Murray and Allport, although dedicated to the uniqueness of the individual, represent increased rigor of measurement. The field theories of Lewin and Murphy are seen as broadening the conceptual scope of personality with some decrease in depth and specificity. The tight, narrow empiricism of the factor analysts, Cattell and Eysenck, contrasts with the loose organismic viewpoints of Goldstein and Angyal. The constitutional claims of Sheldon, the learning theories of Miller and Dollard, and the phenomenological conceptions of Rogers add three more dimensions for comparison.

Each system is summarized under four headings: structure, dynamics, theories of development, and characteristic research. The current status of each theory is then evaluated.

As to the book's accuracy and adequacy, Hall and Lindzey have produced a stunning success. They are certainly comprehensive: with the notable exception of Erik Erikson, every American theorist of prominence is discussed. While European conceptions—existentialism, Kleinian and stratification theories—are neglected, their omission could be justified on the grounds that they lack sufficient functional and heuristic relevance for the American student.

The tolerant and politic tenor of the volume is worth comment. The authors succeed in picking their way through territory mined with potential controversy in a manner calculated to make no one angry. The standard critique of each theory is impersonally presented at the end of each chapter. Since most of the theorists

read and revised the chapters which describe their work, no one is hurt and no one is surprised. Hall and Lindzey wear no man's color. They summarize, outline, and report, and the reader takes his choice.

The book is written with faithful accuracy. The authors stumble only slightly on the Freudian theory of identification; but who does not? The book is well written, too. Although the authors stick manfully to the enormous labor of summarizing and listing seventeen lifetimes of creativity, they manage to breathe life into their subjects. The monumental granite of Freud, the ancient mystique of Jung, come through. Adler's social humanism, Sullivan's genius in the interview, Murray's literary and cultural sweep, and Sheldon's idiosyncratic concern with breeding are woven into the more impersonal descriptions of their concepts. One closes the book with the feeling of having spent several hours with seventeen thoughtful and creative minds.

But what of the substance—the current status of personality theory as reflected in this survey? It is depressingly apparent that the classic dilemmas and dichotomies are still present and unsolved. Objectivity versus richness of meaning is the major division. The multi-level, intuitive conceptions of the clinicians still elude objective measurement. The rigorous statisticians still cling to unilevel, surface, or superficial variables, thus forfeiting interpretative significance. Murray stands out as the impressive exception to this generalization.

The separation between functional clinical reference and laboratory isolation still remains. University research tends to be experimental, socially irrelevant, and it maintains its ties with the bureaucratic fetishes of academic psychology. A strong Titchnerian prejudice against field or clinical research still exists, resisting the tough functionalism of John Dewey, William James, and Egon Brunswik. This has discouraged the inevitable and necessary development of an empirical study of the critical performance. Rogers, Lewin, and Goldstein merit praise for their efforts to measure events in the context of real life.

Functional relevance is perhaps the issue which provides the perspective on this book and the theories which it surveys. Hall and Lindzey use structure and dynamics and research as the three major outlining, descriptive categories. These seventeen theorists are strong in presenting variable systems, dynamic operating laws,

or measurement methods, and to this extent they belong to a pioneering and trail-blazing generation. Their attempts to apply their techniques and systems to problems of crucial predictions have, for the most part, signally failed: the Veterans Administration study of clinical psychologists and the OSS assessment of World War II are two evidences of functional inefficiency. The three factors separately emphasized by these theorists might be seen as foundations and preliminaries for a study of the personality in action—functional action. The next major step in personality will be the scientific study of process.

When Rogers tapes the therapeutic interview, when Bales rates the interaction of working committee members, when Barker classifies the life-space and the routine events of his subjects, the living personality is being studied and measured. Using the insight and methods of their predecessors, such investigators seem to be moving toward a new empirical vantage point.

Hall and Lindzey have competently summarized the major personality theories at what may be a critical historical turning point. Unless this corner is turned, there will be a continued period of revision, rehashing, and reworking rather than the development of new insight.

TIMOTHY LEARY

Oakland, California

Interpersonal Diagnosis of Personality: A Functional Theory and Methodology for Personality. By TIMOTHY LEARY. New York: Ronald Press Co., 1957. Pp. xix+518. \$12.00.

This book covers six years of research by Leary and others, under the auspices of the Kaiser Foundation, aimed at devising a comprehensive and sensitive methodology for the evaluation of personality, so simple, standardized, and objective that most of the work could be handled by non-professional technicians. This is a difficult if not an impossible task.

Leary begins with a classificatory scheme which recognizes sixteen "generic interpersonal themes," empirically derived and then analytically described as various combinations of a power factor and an affiliative factor. These last two were used as axes to define a two-dimensional circular grid: the sixteen basic themes were placed around the circle in a continuous sequence, and then adjoining segments

were combined to give eight octants. The essence of the technique of evaluation is to plot the position of an individual on the circular grid. There is ambiguity throughout the book as to whether the system rests on a distinction between sixteen themes, eight themes, or two dimensions.

A number of separate evaluations, or plottings, are made for each individual. One is made for each of five "levels" of personality, for the first three of which there is separate scoring of information about the person himself and information about significant others. The five levels are defined according to the source of data. Level I deals with a person's social-stimulus value; Level II with his conscious knowledge of himself; Level III with projective materials; and Level V with values. Any themes systematically avoided in the first three levels are assigned to Level IV, the "unexpressed unconscious."

The fact that the same grid is used to describe personality each time facilitates inter-level and interperson comparisons. The resulting "variability indices" are an important achievement.

Perhaps the most interesting aspect of the book for sociologists and social psychologists is that personality is defined as an interpersonal phenomenon. Each of the sixteen basic themes is stated in terms of a readiness to act in certain specified ways toward others and simultaneously to provoke or "pull" specified behavior from them. Such terminology gives operational meaning to the concept of "interpersonal system."

Leary proposes that the same thematic terms be used to describe both normal and pathological behavior, with differences in intensity indicating the degree of rigidity or pathology. Actually, most of the work reported in this book was done in a clinical setting, and the results are correspondingly limited. Interpersonal behavior is viewed exclusively as an attempt to avoid anxiety. The major situation about which predictions are made is the interaction of a patient with his therapist. The one attempt to use the system to analyze a "normal" (top-level management) group served only to highlight "misperceptions and rigid destructive symbiotic interactions." The assumption that this system will be useful in analyzing healthy or creative interpersonal relationships is intriguing but as yet unproved.

One other difficulty arises from the very

complexity of the system, with its sixteen themes, four degrees of intensity, five levels of personality, and simultaneous scoring of self and other, distinctions which, although conceptually illuminating and valuable, in practice are unmanageable. Leary meets this problem with mechanical rather than theoretical simplification, proceeding to combine categories, compute averages, and ignore large segments of information. In doing this, he introduces distortion as well as simplification.

Despite its limitations, this system must be recognized as a major advance in the treatment of the interpersonal dimension of personality. It offers both a systematic classification of interpersonal tendencies and some careful thinking about the structure of personality.

JEANNE WATSON

University of Chicago

The Analysis of Fantasy: The Thematic Apperception Technique in the Study of Personality.

By WILLIAM E. HENRY. New York: John Wiley & Sons, Inc., 1956. Pp. ix+305. \$6.00.

The Analysis of Fantasy is a clinical description of the author's method of interpreting Thematic Apperception Test stories. Henry outlines the basic personality principles which he utilizes: internal consistency, emotional movement, psychic energy (constant emotional energy), expectancy sets (roles), and the self—principles which he adequately describes but does not bring into systematical relationship. He also describes the basic characteristics of ego development, which he believes are also characteristics of fantasy: identification, projection (habituated response tendencies), impulse denial and rejection, and role assumption; these, he argues, may most clearly be discovered from Thematic Apperception Test stories. Henry is quite sensitive to the social claims of the stimulus and defined task and repeatedly emphasizes that the stories should be regarded as a resultant of inner states—basic traits, habits, modes of reaction, emotional change, etc.—and a personal definition of a generalized social situation represented by the pictures.

It is not easy to identify Henry's theoretical position. His descriptions both of personality and of his basic assumptions regarding the nature of productions on "projective" tests are not sufficiently systematized to make classifi-

cation easy. Perhaps the best term would be "dynamic-eclectic," with the firm recognition that human response is determined as much by social situation as by internal characteristics.

The specific hypotheses for interpretation derived from the author's extensive experience will serve as valuable sources of suggestions both for the beginner and for the experienced TAT tester. The book is essentially a personal document, crystallizing the author's experience: it does not present research data to support any of the wealth he offers of suggestions, hypotheses, and generalizations. An extensive bibliography is provided, but the text makes only minimal reference to previously published methods of analysis or research findings.

Four illustrative analyses are presented, each providing story-by-story interpretation, overall evaluation, and other data. A story-by-story analysis of the stimulus properties of each of the standard Murray TAT cards, based on the author's experience, is also given. For each picture, Murray's description, the manifest stimulus demand, the form demand, the latent stimulus demand, the most frequent plots, and significant variations are provided.

It is unfortunate in the light of the author's broad experience with other culture groups that he did not include a thorough description of methodological problems of TAT, or of any projective test for that matter, for cross-cultural studies. One of his illustrations, however, is of an eleven-year-old Navajo girl; another is of a fourteen-year-old Sioux boy.

All in all, this book is a valuable addition to the literature, and it will serve as a rich source of hypotheses in the clinical application of test stories or in research.

JULIAN B. ROTTER

Ohio State University

Self-evaluation and Rejection in Groups. By JOACHIM ISRAEL. Stockholm: Almqvist & Wiksell for the University of Stockholm, 1956. Pp. 249. K. 20.

Joachim Israel's book is important for at least two reasons: its intrinsic merit as a scholarly contribution and its significance as an indication of the impact of American social psychology on contemporary sociology in Sweden. The second can be inferred from the fact that the book is the first publication in the new series, "Stockholm Studies in Sociology," under the general editorship of Professor Boalt.

While the effect of the book may be as yet only inferential, the degree to which Joachim Israel has been influenced by American thinking is fully supported by evidence. Not only is the book written, and well written, in English, but, of the 123 references in the Bibliography, only 18 are to authors or works from outside the United States. Furthermore, the range of viewpoints on which the author has drawn is extremely broad, representing every important theoretical position in American social psychology today. Only the symbolic interactionists are missing, unless Cooley be included as one of them, a state of affairs that is probably best explained by the fact that Israel elected hypothetico-deductive method for theory construction and experimental procedure for verification. These are hopeful signs that American sociologists may have to revise their stereotype of their European confreres as data-avoiding, armchair theorists.

As a contribution to social psychology, the book has several noteworthy features. Israel is addressing himself to two general problems, in both their theoretical and empirical aspects: the extension and refinement of Festinger's theory of processes of social comparison in the area of evaluating abilities rather than opinions; and the connection between such processes, on the one hand, and the phenomena of deviation and rejection of the deviant on the other. He begins by summarizing the work of Festinger, Schachter, and their associates, subjecting it to a critical scrutiny and specifying certain reservations, refinements, and developments as well as evaluating criticisms raised by various scholars from outside the "group dynamics" tradition. Israel's summary is thorough, accurate, balanced, and dispassionate. Clearly, while he has been strongly influenced by Festinger, Schachter, *et al.*, he has not uncritically accepted everything they assert.

The second chapter is a more detailed report than has been heretofore available in print of the Swedish version of a group experiment conducted in seven European countries and described in *Human Relations*. An especially interesting part is the section in which Israel tries to tease out of his data some of the reasons why Swedish adolescents behave differently from those in Holland, France, and elsewhere.

Chapter iii forms the scholarly, theoretical heart of the book. Here Israel gives meticulous attention to the examination of such concepts as "group," "group goal," "goal structure" (co-

operative and competitive interdependence), "membership," "norm," "rejection," and "evaluation." With great care and explicitness he considers innumerable ramifications of their meanings and then states precisely how and why he proposes to employ each in what he modestly terms "a conceptual outline" rather than a theory. One section of the chapter is given over to an analysis of the functions of reference groups, to which he contributes a number of fresh ideas about how they are variously employed by persons making "wishful," "informative," "corroborative," and "normative" evaluations of their own behavior, opinions, or situations. This section is one of the most stimulating in the book.

Chapters iv and v conclude on an experimental note and in an abrupt way. The first of these reports an experiment on differential rejection of individuals who appear to be either superior or inferior in respect to an ability when the goal structure of the group varies. Chapter v reports an experiment on the effects of the discrepancy between self-evaluations and the evaluations of self by others. Both experiments are designed according to hypotheses developed in the theoretical chapter preceding them and are interesting in themselves; but they deal with only a small number of the theoretical implications and, unfortunately, seem dwarfed by the previous structure. The fact that the second experiment did not yield conclusive results probably also contributes to the sense of anticlimax.

For graduate students and their teachers, Israel's book, as noted, provides an excellent and critical summary of recent thinking and research on central processes and key concepts in the social psychology of small groups. Anyone concerned with methodology in social psychology will be interested in his use of deductive procedure and formalization in theory construction. Finally, research scholars will find it full of provocative insights and testable hypotheses.

HENRY W. RIECKEN

University of Minnesota

The Psychology of Sexual Emotion: The Basis of Selective Attraction. By VERNON W. GRANT. New York: Longmans, Green & Co., 1957. Pp. viii+270. \$4.75.

The writer's key to the lively market for popular literature on sex, love, mating, and

marriage is somehow to get the magic words "sex" and "psychology" into the title. Grant qualifies easily with a book about the lover's choice, in which he attempts to answer the question: Is there a motive other than sensual impulse which attracts a particular man to a particular woman?

Using the arguments that love is a particularistic choice, that case and observational materials suggest that there may be amorous attraction devoid of sensual impulse, that amorous love tends to quick heat and short duration, and that love is normally heterosexual, Grant disposes of sensual attraction, sublimation, tender protectiveness, altruism, complementary needs, similarity, and social determinants as the primary elements in choosing a lover. He substitutes "amorous attraction," "an urge to possess, in a complete, intimate, and lasting sense, manifesting physically in nongenital contacts" (p. 232), as the motive explaining the specific choice. Amorous attraction is a highly individualized, aesthetic preference governed by cultural standards, differences of aesthetic organization in the personality, and accidents of experience. It may be said in his favor that he disagrees with the idea that love is nothing more than inhibited sexual impulse.

The style is pedantic. There is value in giving credit to prior thinking, but reference material is so prominent in this book as to interfere with the development of the thesis. The author's contribution to ideas about love could have been stated fully in an article—in fact, it is stated in seven pages (pp. 231–37). What precedes is no more than an annotated bibliography on love as a non-sensual experience. Considered only as such, it is inadequate, omitting much of the writing since 1945 on dating and courtship, e.g., the work of Winch, Ktsanes, Turner, Herman, Foote, and Ehrmann. By contrast, Grant draws heavily on Binet (1887), Finck (1899), and Lucka (1915).

Choosing a lover is considered neither as an interaction nor as an example of choosing in general. Rather, it is proposed that choice of an aesthetically attractive other is a unique event determined by something approaching a fetishistic object fixation in the chooser. Grant's argument is an oversimplification, which ignores numerous factors which have been shown to provide predictors of choice of a lover.

ROBERT J. POTTER

Chicago

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The American Journal of Sociology



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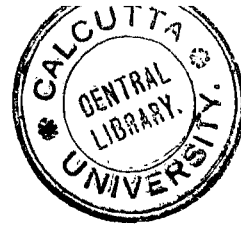
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THE AMERICAN JOURNAL OF SOCIOLOGY

Volume LXIII

NOVEMBER 1957

Number 3

MULTIDIMENSIONAL SCALE ANALYSIS¹

JAMES S. COLEMAN

ABSTRACT

A model for the simultaneous measurement of several attitudes associated with a set of dichotomous items is presented. It provides an extension of the usual scale analysis into higher dimensions, although, to date, the techniques have been worked out only for two dimensions. As an example of the applicability of the model, a set of items previously analyzed as a one-dimensional scale of "particularism-universalism," but with considerable deviation from a perfect scale, are reanalyzed. The analysis allows going beyond the original particularism-universalism continuum into a study of the relative strength of responsibility to one's friend, to a generalized other, and to one's self. This example and the nature of the model itself indicate the possibilities of this model and others based on the same underlying theory for attitude measurement.

In 1951 Stouffer and Toby published a paper of considerable interest concerning the use of four questions which required the respondent to resolve role conflicts of one sort or another.² The questions all involved adherence to a general principle in the face of temptation to help out a friend by violating. For example:

Item 1. You are riding in a car driven by a close friend and he hits a pedestrian. You know he was going at least 35 miles an hour in a 20-mile-an-hour speed zone. There are no other witnesses. His lawyer says that if you testify under oath that the speed was only 20 miles an hour, it may save him from serious consequences. What right has your friend to expect you to protect him?

¹ This paper was written while the author was at the Center for Advanced Study in the Behavioral Sciences. Grateful acknowledgment is made to members of a seminar on measurement and scaling at the Center for their comment and criticism of an earlier draft, in particular to Roy Radner for discussions about mathematical problems.

² Samuel Stouffer and Jackson Toby, "Role Conflict and Personality," *American Journal of Sociology*, LVI (March, 1951), 395-406.

The paper raises an interesting methodological point beyond its substantive interest. The authors constructed a Guttman scale of "universalism-particularism"; but their items did not fit a scale very well, as they themselves pointed out. There were 222 non-scale response patterns out of a total of 648; that is, over a third. Two patterns in particular were frequent, containing 66 and 52 persons. Inspection suggested something which often seems apparent with items which do not quite fit a scale: that, for those who answered in a non-scale fashion, the supposedly relevant dimension "particularism-universalism" was not relevant; they answered on other grounds. The result was patterns which were inconsistent (i.e., non-scale) from the perspective of those who constructed the items yet entirely consistent from the perspective of those who answered in those patterns. Their consistency lay in a different dimension from the "particularism-universalism" dimension.

Such ideas lead to the notion of a multidimensional scale; yet, whenever generalization of a Guttman scale into higher dimen-

sions has been tried, it seems to lead to trivialities, such as logical conjunctions and disjunctions (e.g., "Yes" on the item if high on *both* attitudes; or "Yes," if high on *either* attitude). These difficulties, together with others, such as problems of measuring hierarchies of attitudes and changes in attitude may be due not simply to methodological and formal scaling defects but to an inappropriate conceptualization of attitudes. This paper presents a new approach to the conception and measurement of attitudes, which begins substantively but results in models of formal measurement. With this conception, the notion of hierarchies of attitude has a definite operational meaning, and the formal models not only allow the establishment of such hierarchies but also generalize to higher dimensional scales. In this paper a multidimensional model is developed. (Another model, based on the same conceptional framework but with data on rank order, is now being developed.) As an application, the Stouffer-Toby items are used because they fit a two-dimensional scale rather well.

The conception and the model are not restricted to items of the form of Stouffer and Toby's: they are intended to be applicable to many kinds of questions which allow the respondent to choose from two alternatives. They would *not* be applicable, however, to many agree-disagree items, in which "agree" indicates willingness to take a position as over against *all* other alternatives, while "disagree" indicates unwillingness to commit one's self in the absence of knowledge of other alternatives. Much evidence indicates that the poorer-educated are more likely to take an unqualified position, quite apart from the content, while the better-educated are unwilling to commit themselves. Of course, as one can see in looking through a list of agree-disagree items, not all have this character.

THE CONCEPTUAL FRAMEWORK

G. H. Mead stated a view of the self as an entity which incorporated other people and other objects; identification with others was for Mead "incorporation" into this ever ex-

panding psychological self. The resulting picture of the self is of an inner core with attachments of varying strengths radiating outward to objects of all kinds: persons, ideas, principles, physical objects, ideals, and so on. He suggests further that new objects are incorporated through their (perceived) association with those already incorporated: the latter act, as it were, as bridges or mediating links to the new ones. Depending on whether this perceived association of new with old is reinforcing or antagonistic, the new attachment will be positive or negative. Or, if the new object is associated with more than one object to which the self is already attached, there is the possibility of conflicting "pulls." If, for example, there is a school controversy in his community, a citizen may be pulled by attachment to a friend toward identification with those attacking the school administration; but he may also be pulled away from them by his attachment to the educational principles for which he feels the administration stands.

A slightly different view from Mead's, but one which would be formally similar, is that of students of the process of learning. From the time of Thorndike's "connectionism" to the present, the thread which has run through much of the learning theory is the action of existing elements within the individual—whether called "elements," "primary drives," "secondary drives," or "unconditioned reflexes"—in leading the individual to make the response with which the existing elements are associated. Hilgard says, for example, in his comprehensive survey:

When confronted with a novel problem, how does the learner reach a solution? The stimulus-response psychologist finds the learner assembling his habits from the past appropriate to the new problem, responding either according to the elements that the new problem has in common with familiar ones, or according to aspects of the new situation which are similar to situations met before.³

³ Ernest R. Hilgard, *Theories of Learning* (New York: Appleton-Century-Crofts, 1956), p. 10.

Though learning theorists often occupy themselves with the way a solution is found by trial and error when the elements from past situations do not provide definite guides, this conception of association underlies their work.

This way of looking at the self and its attachments to the outside world leads in a number of fruitful directions. It provides a beginning for a theory of change in attitude; it explains many of the observable phenomena in social controversy and in public response to political campaigns;⁴ and it suggests the conjecture that people, largely as a function of their range of social experience, may differ in the way they relate themselves to the outside world. Some may be "people-oriented," forming attachments to new objects largely through their attachments to certain individuals; others are "idea-oriented," forming attachments more nearly on the basis of their values, ideas, and ideals; still others, perhaps, are "group-oriented," relating themselves to new things through group loyalties. Whether this conjecture is true or not is of course an open question—the point is that this general approach allows the question to be raised. Finally, of relevance to attitude measurement, is this: by viewing items in a questionnaire as "new objects" in the environment which must be responded to, this approach provides a theory about the sources of the responses.

According to this framework of thought, a question which provides an individual with two alternatives sets up a conflict within him—a conflict whose intensity varies in different people. Certain objects (ordinarily values or beliefs) are associated with a positive response; others, with a negative response. Depending upon which set of forces is stronger, he will answer positively or negatively—and, the more nearly equal are the forces, the more likely he will say, "Don't know," or will refuse to respond.

There is also the possibility, of course, that the item may elicit only mutually reinforcing attachments or that it may be a

question which the individual has already faced and decided, so that no conflict exists at all. This situation is familiar to interviewers, who sometimes find that answering a question gives much pleasure to the respondent who has evidently given it much thought. Hyman and Sheatsley report this kind of response to questions on racial segregation:

In the overwhelming majority of cases, the answers, and emphatic comments, fairly burst forth: "We moved last year because some Negroes moved into our block." "I'll go to jail before I let my kids go to school with Negroes." "I don't have to think about *that* question; I'll tell you right now."⁵

These questions were evidently not at all "new situations" which pulled the respondent toward both alternatives—he had long ago been pulled to one or the other. For the purpose of this paper, let us exclude this situation.

One of the most important aspects of this approach is that it attempts to measure *attitudes-in-action*. In contrast, the usual conception behind Guttman scaling or latent structure analysis or other techniques is that attitudes are something "there," resting, so to speak, while we apply our tape line to them and arrive at a measurement. The present strategy "watches them work" by pitting them against one another, as it were, and seeing which wins.

If it pleases us, we can, with Allport, define attitudes as "states of readiness"—but, when we measure them, we had better not conceive of them as *states* of anything, for this will lead us to attempt to measure them by applying the "right" measuring stick to them as they lie there. If we see them as "dynamisms," or "processes," or "forces," then we will measure them by watching them work. The development of theory in other sciences has taught one thing: fruitful measurement of theoretical concepts is measurement of what they do and not what they are: the very definitions, in fact, are definitions not of what these entities are but of what they do.

⁴ James Coleman, *Community Conflict* (Glencoe, Ill.: Free Press, 1957).

⁵ H. H. Hyman and P. B. Sheatsley, "Attitudes on Desegregation," *Scientific American*, CXCV, No. 6 (December, 1956), 35–39.

REINTERPRETATION OF A GUTTMAN SCALE

According to this view, how does a perfect Guttman scale come about? In this way: For everyone in the sample, each question in the scale has associated with it the same two objects (e.g., values), one associated with a positive response, and one associated with a negative.⁶ The individual differences in response will be due to the fact that relative attachment to the two objects differs in different people; the difference in marginals of the items is due to the fact that the two objects are perceived to have a certain degree of association with and relevance to one item and a different association with and relevance to a second item. As a result, the conflict set up within the individual is resolved on the side of a positive response to some items, a negative response to others—except, of course, from individuals whose difference in attachment to the two objects is so great that they respond positively or negatively to *all* the items. Note here that there is no notion of an “attitude continuum” or a “universe of content.” The fundamental quantities are strengths of attachment to objects, and the continuum along which individuals lie is a continuum of relative strength of attachment to the two objects—more precisely, the ratio of strengths of attachment to these objects.⁷

To apply this to the items Stouffer and Toby used to form a scale of “universalism-particularism,” two of the other three items they used may be considered, namely:

⁶ Each item may not have associated with it the *same* two objects; the second item may have associated with it two objects whose attachments are highly correlated with those associated with the first. It is such cases which sometimes make it difficult to name two specific objects with which the items of a scale are associated.

⁷ These statements imply a quantitative concept of “strength of attachment,” having a meaningful metric and a meaningful zero point. The development of the quantitative properties of this concept is too technical and involved to deal with here and will be presented in J. Coleman, *Introduction to Mathematical Sociology* (tentative title) to be published by the Free Press, 1958.

Item 3. You are a doctor for an insurance company. You examine a close friend who needs more insurance. You find that he is in pretty good shape, but you are doubtful on one or two minor points which are difficult to diagnose. What right does your friend have to expect you to shade the doubts in his favor?

Item 4. You have just come from a secret meeting of the board of directors of a company. You have a close friend who will be ruined unless he can get out of the market before the board's decision becomes known. You happen to be having dinner at that friend's home this same evening. What right does your friend have to expect you to tip him off?

Now, rather than to say that there is a continuum of universalism-particularism, we say that each item elicits attachments to two values: responsibility toward one's own friends and responsibility to a generalized other. These two exist in varying strengths in everyone; their relative strength in an individual determines on which side he will respond to each of the three items. Looking at them, we might say a priori that Item 1 would be hardest to answer in favor of the friend, and Item 4 the easiest. That is, the association of the Item 1 with responsibility toward a generalized other, relative to its association with responsibility toward friend, is greatest for Item 1 and least for Item 4. The marginals of these items agree with such an ordering, for Item 1 has the fewest responses favoring the friend, while Item 4 has the most.

For the subsequent analysis, it will be useful to set down the model's assumptions in mathematical terms. The mathematics is quite elementary and will simplify the analysis.

If b_{jk} is the relevance and alignment of object j (e.g., responsibility to friend) with item k , and if x_{ij} is the strength of attachment of individual i to object j , then individual i 's response to item k depends upon whether the quantity $x_{i1}b_{1k} + x_{i2}b_{2k}$ is greater or less than zero. If the response favoring the friend in this example is labeled “+” and if responsibility to the friend is labeled “object 1,” then b_{1k} is positive (that is, object 1 is aligned with a positive response) and b_{2k} is negative (that is, object 2 is

aligned with a negative response). Thus the first term of the summation is positive, and the second term is negative; depending on which is larger, the response will favor the friend or go against him.

Thus, in short, the assumptions of the model are that individuals can be characterized in terms of numbers (the x 's) representing their strengths of attachments to environmental objects and that opinion items can be characterized in terms of numbers (the b 's) representing individuals' perceived association with them of objects in the environment; in the case of two items associated with an item k by an individual i , response to the item will be determined as follows:

If $x_{i1}b_{1k}/x_{i2}b_{2k} > 0$, then the response is positive;

If $x_{i1}b_{1k}/x_{i2}b_{2k} < 0$, then the response is negative.

Finally, when the whole sample of individuals perceives the same two objects to be associated with the item, and this is true for a set of items, then the b 's are constant over individuals, and the x 's are constant over items. It is such a situation, as will be evident below, which generates a perfect Guttman scale.

The inequalities above can be rewritten so that the individual and item are each characterized by a ratio, as follows:

$$\frac{x_{i1}}{x_{i2}} > \frac{-b_{2k}}{b_{1k}} \text{ (positive response), } (1)$$

$$\frac{x_{i1}}{x_{i2}} < \frac{-b_{2k}}{b_{1k}} \text{ (negative response). } (2)$$

If we consider a continuum of x_1/x_2 , then all those individuals whose x_1/x_2 was greater than $-b_{2k}/b_{1k}$ would respond positively to item k , and all those below would respond negatively. The cutting point of the item would be at that point of the continuum at which the b ratio equaled the x ratio. A three-item scale would have a continuum and cutting points something like Figure 1. The individual's response pattern would be determined by the position of his x_1/x_2 ratio on the continuum. Thus the familiar con-

tinuum and cutting points of scale analysis appear, though their conceptual significance is different from that usually given them in scale analysis.

It is important to remember that the single continuum is not a fundamental

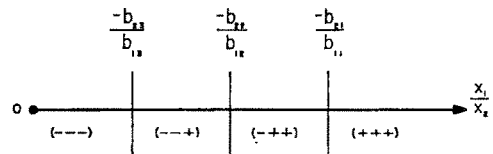


FIG. 1.—A continuum representing the ratio of strengths of attachment to two values, with the "cutting points" of the items shown.

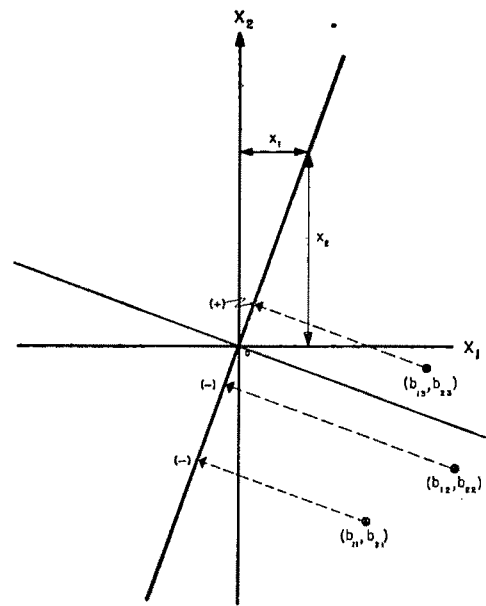


FIG. 2.—The plane of x_1 and x_2 , showing how a perfect scale is generated by a set of three items and a set of persons differing in $x_1:x_2$ ratio.

quantity but is *derived* from two dimensions, upon which can be located the individual's strength of attachments to the two objects as well as the item's association with the two objects. Such a two-dimensional picture is given in Figure 2, where the individual's ratio of attachments is characterized by a line, and the items are characterized by points. In this case, which is fitted to the role-conflict example, the individual's

line must pass through the upper quadrant (positive on both dimensions) because he is assumed to have a positive feeling of responsibility both to friend and to the generalized other. The points representing the items are in the lower-right quadrant, because the responsibility to a friend (labeled "1") is aligned positively with each item, and the responsibility to a generalized other (labeled "2") is aligned negatively.

The response of an individual to an item depends upon whether the normal (perpendicular) to the individual's line passes above or below the item. Every item above the normal is responded to positively; every item below is responded to negatively. It can be shown that if an item is above the line, then $x_1/x_2 > -b_{2k}/b_{1k}$; if an item is below the line, then the inequality is in the other direction. In other words, Figure 2 is simply a geometrical alternative to the algebraic inequalities (1) and (2). And since the normal to the x_1/x_2 line moves in an arc around the zero point, then it generates the four scale patterns and the three "cutting points" which can be placed on a continuum (Fig. 1).

But now what has been gained by this reinterpretation of an ordinary scale? Just this: since the scale is a scale of relative attachments to *two* objects, all sorts of things are possible which were not as long as the scale was viewed as simply a single-attitude continuum. For example, another set of items can be devised pitting responsibility to friend versus some other value, thus indirectly relating, through the two scales, his responsibility to generalized other and this other value. Thus this model conceives of attitudes and values in interaction with one another, not in isolation, as is ordinarily true with attitude measurement models.

As a result, one's attitudes (or "attachments") are always measured *relative* to one another. The absolute strength of an attitude can be only approximated by successively matching the object against numerous others to which the individual is attached. In fact, a Guttman scale seems very often to do this. There is only *one* object of attachment which is the same from item to

item. The attachments which oppose it are in a sense "randomized," differing from item to item. Insofar as a person's strength of attachment to each of the opposed objects is about the same relative to others' strength of attachment, then the items would be expected to form a scale.

A TWO-DIMENSIONAL MODEL

One can also devise a set of items each of which is associated with three rather than two relevant objects. In this way it is possible to go on into higher dimensional scales. In the role-conflict example, one could devise a set of items which pitted responsibility to friend versus a combination of responsibility to a generalized other and responsibility to one's self. Then an individual's response pattern might be far more revealing than to simply locate him along a single continuum; it could tell his relative feeling of responsibility to a generalized other and to himself at the same time it told his feeling of responsibility to a friend relative to these other two. It could in short, begin to show his structure of values or attitudes.

The Stouffer-Toby items, I suggest, do encompass these three attachments, and, when they are ferreted out, the individual's responses will allow locating him in the fashion just mentioned. Perhaps the best way to see this is to start by examining the response patterns for the above three items, considered as a scale. They appear to scale rather well, with relatively few error patterns (Table 1). Thus it seems reasonable that these items be considered to fit fairly well the assumptions of the one-dimensional, or two-object, scale. But now consider the other item:

Item 2. You are a New York drama critic. A close friend of yours has sunk all his savings in a new Broadway play. You really think the play is no good. What right does your friend have to expect you to go easy on his play in your review?

When it is incorporated into the scale to give the best-fitting four-item scale, the frequencies in Table 2 result, with a large

number of error responses. Therefore, rather than to bring this item directly into the three-item scale of the other three, let us introduce first a third value or "object-of-attachment": a feeling of responsibility to one's self. The hypothesis is that Item 2 contains a relatively large component of responsibility to self and a relatively small component of responsibility to a generalized other. Intuitively the notion is that the respondent as a drama critic might hesitate to "go easy on the play" more because of a sense of preserving a professional self-image than because of a regard for his readers. In contrast, Item 1, for example, contains a larger component of responsibility to the other: in a traffic accident, one may feel more responsibility to the injured person and less to one's self.

If this is true, it would appear that those who answered the first item in *favor* of the friend and the second *against* the friend feel more responsibility to self and less responsibility to a generalized other than the person who answered the first against the friend and the second in favor of him. It might of course be the case that some other attachments entered in, but this interpretation is at least reasonable enough to give an idea of how one could begin to analyze differences in the three attachments. For these two items—1 and 2—the four possible response patterns would have roughly these interpretations (where "+" is a pro-friend response):

- + +: strong feeling of responsibility to friend relative to self and others
- + -: Strong feeling of responsibility to self relative to others; medium feeling of responsibility to friend
- +: weak feeling of responsibility to self relative to others; medium feeling of responsibility to friend
- -: weak feeling of responsibility to friend relative to self and others

This heuristic analysis of the possible responses to Items 1 and 2 gives an idea of just how the suggested differences in responsibility to other and to self can affect responses. More precisely, we can write the inequality for the three-object case:

$$x_1b_{1k} + x_2b_{2k} + x_3b_{3k} > 0 (\text{positive response}) \quad (3)$$

$$< 0 (\text{negative response}).$$

TABLE 1

RESPONSE PATTERNS FOR ITEMS 1, 3, AND 4 FROM THE STOUFFER-TOBY EXAMPLE

ITEM				FREQUENCY
1	3	4		
+	+	+		118
-	+	+		175
+	-	+		28 (error)
+	+	-		18 (error)
-	-	+		137
-	+	-		30 (error)
+	-	-		18 (error)
-	-	-		135

TABLE 2

RESPONSE PATTERNS FOR ITEMS 1, 2, 3, 4 FROM THE STOUFFER-TOBY EXAMPLE

ITEM				FREQUENCY
1	2	3	4	
+	+	+	+	66
-	+	+	+	95
+	-	+	+	52 (error)
+	+	-	+	15 (error)
+	+	+	-	8 (error)
-	-	+	+	80
-	+	+	-	16 (error)
+	+	-	-	6 (error)
+	-	+	-	5 (error)
-	+	-	+	66 (error)
+	-	-	+	13 (error)
-	-	-	+	71
-	-	+	-	14 (error)
-	+	-	-	21 (error)
+	-	-	-	6 (error)
-	-	-	-	114

This inequality gives rise to two ratios of alignment for the items, replacing the one ratio in the two-object case:

$$\frac{-b_{2k}}{b_{1k}} = \frac{\text{other}}{\text{friend}} = \alpha_k, \quad (4)$$

$$\frac{-b_{3k}}{b_{1k}} = \frac{\text{self}}{\text{friend}} = \beta_k. \quad (5)$$

The previous assumptions regarding the two items can now be put more precisely:

the assumption is that α is relatively large for Item 1 and that β is relatively small; α is relatively small for Item 2, while β is large. More fully, the a priori assumptions about the order among these ratios for all items is as follows:

$$\alpha_1 > \alpha_3 > \alpha_4 > \alpha_2, \quad (6)$$

$$\beta_2 > \beta_1 > \beta_3 > \beta_4. \quad (7)$$

That is, the other: friend ratio (α) is greatest in connection with the court (Item 1), next with the insurance (Item 3), next in connection with the board of directors (Item 4), and least with the drama (Item 2). In contrast, the self: friend ratio is assumed

analysis to come will show this more explicitly, together with further implications. At this point, however, it is enough to see that, by nothing more than two one-dimensional scale analyses on the same items, it is possible to learn much about the attitudes of the respondents—provided, of course, that the a priori assumptions (6) and (7) above are correct. But note that the possibility of such a “double” scale from the same items depends upon this different way of conceptualizing the one-dimensional scale: a conflict between two values rather than one “attitude continuum” like that of “particularism-universalism.”

The explicit analysis of the three-object

TABLE 3
TWO SCALES FROM THE SAME FOUR ITEMS, INDICATING THE
SCALE PATTERNS IN EACH CASE

(a) other: friend					(b) self: friend				
ITEM				FRE- QUENCY	ITEM				FRE- QUENCY
1	3	4	2		2	1	3	4	
+	+	+	+	66	+	+	+	+	66
-	+	+	+	95	-	+	+	+	52
-	-	+	+	66	-	+	+	+	80
-	-	-	+	21	-	-	-	+	71
-	-	-	-	114	-	-	-	-	114

to be greatest in connection with the drama critic, next with the court, then with the insurance, and least with the board of directors.

If each of these orderings is used separately, disregarding the other, as the order for a single-dimensional scale, then Table 3 gives the scale patterns. Comparing this table with Table 2 shows that these patterns include all those frequencies over 16 in number, without including any frequencies less than 21, and only one less than 52. Thus the three largest deviations from the single-dimensional scale of Table 2 are included here. It would seem that the two-dimensional scale would be an amalgam of the two separate ones, in which Item 2 has quite different positions. And it would seem that persons whose response patterns are found in Table 3a but not in 3b are high in responsibility to others, while those whose response patterns are found in Table 3b but not in 3a are high in responsibility to self. The

case may be carried out either algebraically, using equation (3) together with conditions (6) and (7), or geometrically, using the same conditions but translating them into their geometric counterparts. Though the algebraic method may offer more promise in the long run, it involves complicated manipulations with an unfamiliar algebra of inequalities. The geometric method is an extension of Figure 2 and is much simpler.

As was mentioned before, as the normal to the x_1/x_2 line traverses the quadrant (in Fig. 2), it passes through the three items in a definite order, acting as a “cutting line” which produces a single continuum as in Figure 1. The extension of Figure 2 to a three-dimensional space involves points on the surface of a sphere representing the items and a “cutting plane” normal to the line through the individual's point (x_1, x_2, x_3). Rather than to portray such a three-dimensional figure, a plane representation of a quadrant of a sphere will be used. The

are the error patterns in the two-dimensional scale, are given in Table 5. This table shows that the two-dimensional model fits rather well.

But the major purpose of attitude measurement is not to see whether a model fits the data or to determine just what the allowable scale patterns are under the par-

TABLE 5

SCALE PATTERNS AND ERROR PATTERNS FOR THE TWO-DIMENSIONAL SCALE REPRESENTED IN FIGURES 3 AND 4

ITEM				FREQUENCY
1	2	3	4	
+	+	+	+	66
-	+	+	+	95
+	-	+	+	52
+	+	-	+	15 (error)
+	+	+	-	8 (error)
-	-	+	+	80
-	+	+	+	16 (error)
+	+	-	-	6
+	-	+	-	5
-	+	-	+	66
+	-	-	+	13 (error)
-	-	-	+	71
-	-	+	-	14 (error)
-	+	-	-	21
+	-	-	-	6 (error)
-	-	-	-	114

ticular assumptions used; it is to determine something about the attitudes of persons relative to those of other persons. What can be said about this from the above analysis?

In the first place, it is evident that, if one could precisely characterize the position of the cutting line which generates a given response pattern, then it would be possible to characterize precisely the relative strength of the three feelings of responsibility. If the individual's cutting line is in position 4, then his responsibility to the other is strong relative to his responsibility to himself (i.e., the individual's line which corresponds to this cutting line is characterized by a low x_3 value relative to the x_2 value). If it is in position 1, then his responsibility to himself is stronger. But it is not generally possible to say just which position the cutting line was in to generate a given response pattern.

For example, the pattern ++++ could have been generated from any of four positions; all this pattern tells is that the feeling of responsibility to his friend is high relative to the other two. Some of the other patterns, however, do tell somewhat more. Figure 5 summarizes the information given by these items with respect to the three feelings of responsibility in question. It shows, for example, that persons with pattern 3 are higher in the ratio of self: other responsibility than anyone with patterns 2, 5, or 7; and they are higher in friend: (other, self) responsibility ratio than anyone with patterns 4, 5, 6, 7, or 8. Yet these items allow only a glimpse into the individuals' relative feelings of responsibility in these three directions. If more questions

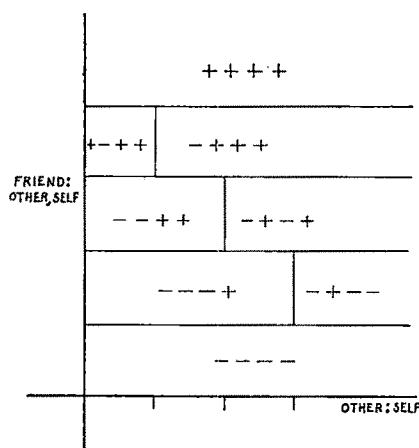


FIG. 5.—The configuration of attitudes along two dimensions and the position of each response pattern in this configuration.

were asked, and particularly if it were possible to devise questions which would pit different combinations of the three values against one another, then it would be possible to chart the individual's position much more precisely.

FURTHER WORK

It is obvious that the model as it stands, though usable in practice, still is unfinished. There is no measure of degree of fit, as there is in ordinary scale analysis (though the

measures of fit in ordinary scale analysis are *ad hoc*); there is no solution as to how to find the best fit efficiently; and the various possible configurations which might occur when the values are aligned on different sides of different questions remain unexplored. Altogether, only a beginning has been made, even with the two-dimensional case. Nevertheless, as is obvious, the model is practical and usable as it stands.

Beyond the two-dimensional case nothing has been done except for initial explorations. It would be fortunate, for example, if the model could be solved by algebraic methods rather than geometric ones; this would make the extension to higher dimensions simpler. However, the algebraic methods are hampered by the lack of powerful methods in the algebra of inequalities such as exist in ordinary algebra. In any case, it may not be fruitful to go into much higher dimensions, since one then gets into a difficulty which hampers factor analysis: several correlated dimensions or factors will reduce to fewer statistically independent dimensions, so that the resulting "dimensions" are composites of severally logically independent ones.

There are several related directions in which work appears promising. One is the use of one-dimensional scales which are "tied together" by having one of their two values in common. For example, in a current research project on high-school children, it is desired to obtain their relative orientation to parents, to the school, to friends of their own sex, to the opposite sex, and to teachers. A circumscribed range of situations can be posed in which they are forced to choose between going with parents or peers, between going to a school-related activity and going with their friends, between going with parents or to a school-related activity, and so on. It is true, of course, their orientation to one rather than another group will be dependent upon the situation with which they

are confronted, but fairly similar situations can be devised. In some ways, a model based on these interlocking scales is simpler to handle and gives information similar to that obtained from items which incorporate all three objects of attachment in each item.

A second direction of work has to do with the underlying model upon which the numerical properties of the "strengths of attachment" are based. The development of this model will at one time give⁸ (a) a justification for the assumed quantitative properties of the "strengths of attachment" and (b) a probabilistic generalization of the model, which as it stands is deterministic. This automatically gives a criterion for goodness of fit of the model. (c) Since this underlying model relates the *strength* of attachment to the expected length of *time* necessary to make a response, then a technique can be developed which will utilize both direction of response and length of time required to make the response, thus extracting much more information from each item. (d) This underlying model implies that, the nearer to zero the basic inequality (3) is, the harder it is to answer the question. If respondents were asked, after answering the above set of role-conflict items, to rank them in order of difficulty of answering, then these answers could be checked with predictions from the model. For example, Figure 4 shows that persons giving the response pattern $+ - + +$ should give either 2 1 3 4 or 1 2 3 4 as the rank order of decreasing difficulty of answering. Similar predictions for the other response patterns may be made by inspecting Figure 4. Such questions will be included in future applications of the model, so that the assumptions of the underlying theory can be better tested.

UNIVERSITY OF CHICAGO

⁸ Work on these developments will be reported in the monograph referred to in the preceding footnote.

COLLECTIVE PROTEST IN RELOCATION CENTERS¹

NORMAN R. JACKMAN

ABSTRACT

Collective protest by people of Japanese ancestry in relocation centers was related to the communicative process among groups mobilized about issues of mutual interest. One hundred and fifteen incidents were classified according to degree of collective protest and compared with the communication channels open in each. In every case there was a relationship between the degree of collective protest and the utilization of channels of communication by contending groups. In centers marked by little collective protest communication remained open and contending parties achieved consensus, whereas in centers marked by violent protest contending groups ceased to communicate and developed conflicting perspectives.

Following the Japanese attack on Pearl Harbor, President Roosevelt issued proclamations which subjected enemy aliens to arrest and detention on suspicion of espionage or sabotage. Certain military installations and defense plants and a limited area around them were designated military security zones, and enemy aliens were banned from them. They were expected to leave voluntarily. But on February 19, 1942, at the request of the War Department, which judged earlier security measures of the Department of Justice inadequate, the President authorized the army to establish military zones in the United States whose limits were to be determined at the discretion of military staff officers. The order that any group, citizen or alien, could be excluded from them was aimed primarily at Japanese aliens and Japanese-Americans from the Pacific Coast.²

During the summer and fall of 1942 a mass evacuation of men, women, and children of Japanese ancestry took place. Nearly 120,000 individuals and families were moved to assembly centers and then distributed among ten relocation centers. Their release was subject to many restrictions, but most

evacuees could leave and take up residence in areas outside of restricted military zones if they so wished.

The majority of evacuees stayed in the centers for the greater part of the war,³ primarily through fear of the hostility of Caucasian soldiers and civilians. Furthermore, since the evacuees had lost nearly all their possessions, many felt that the federal government was responsible for their welfare.⁴

Physical conditions in the centers were roughly comparable, and their formal administrative organization was identical. The frame buildings, covered with black tar paper, were in blocks designed to house from 250 to 300 persons. Each block consisted of fourteen single-story barracks, divided into four or six apartments, a mess hall, a recreation hall, latrines, and a laundry. There were also administrative, utility, and various auxiliary buildings.

Administrative responsibility was in the hands of a non-Japanese civilian staff: a project director, with assistants in operations, administrative management, and community management, a reports officer, and an attorney.

¹ The material for this article has been taken from the writer's unpublished doctoral dissertation, "Collective Protest in Relocation Centers" (Berkeley: University of California, 1955).

² See Dorothy Thomas and Richard Nishimoto, *The Spoilage* (Berkeley: University of California Press, 1946); Morton Grodzins, *Americans Betrayed: Politics and the Japanese Evacuation* (Chicago: University of Chicago Press, 1949).

³ At the end of 1944 less than 40,000 people, of nearly 120,000, had left the centers. During 1945 nearly all were relocated, though much of this movement was involuntary, owing to the closing of the centers (War Relocation Authority, *The Evacuated People: A Quantitative Description* [Washington, D.C.: Government Printing Office, 1946], Table 10, p. 30).

⁴ See L. Bloom and R. Riemer, *Removal and Return* (Berkeley: University of California Press, 1949).

Formal authority was vested in government administrators, many of whom, at least initially, held opinions that differed from the evacuees' on such matters as the need for relocation, the oriental minority in America, and the proper implementation of government regulations and directives. Nevertheless, the administrators were largely united in the belief that, since the centers were a part of a government order, they must be made to function in a reasonably orderly manner.

The long-range government policy concerning evacuation was threefold: to allow the residents to govern themselves, within the limitations of War Relocation Authority regulations, to make them economically self-sufficient, and to settle them as quickly as possible in the nation outside of military zones.

Interaction between the evacuees and the administration centered about the communicating of the rules and regulations to the residents and the executing of them. Channels of communication within the centers followed a hierarchical pattern from the top administrator, the project director, to the individual evacuee who held no appointive or elective post but who was linked to the camp structure through block meetings, the camp newspaper, information bureaus, bulletin boards, occasional mass meetings, and so on.

The functioning of the system depended mainly upon the administrative staff's interpretation of self-government. Self-government, as enacted by the War Relocation Authority from its headquarters in Washington, was centered in a community council composed of evacuee representatives who were American citizens. Over one-third of the evacuees were excluded from formal community government, though the regulation was changed to allow aliens to participate in self-government beginning in 1943. Many aliens feared that the American citizens among them would not represent them properly. They therefore sought representation through informal organizations which had no standing with the administration.

Self-government was impeded by the fact that the community council was subservient, in the main, to the administration. It was thus a self-governing body only in the narrowest sense of the term. The center was, at best, a benevolent authoritarianism. Nevertheless, self-government bodies conducted negotiation and mediation when they had the support of most administrators and evacuees.

The administration attempted to establish enterprises by which the residents could become economically self-sufficient, including production for the war effort and food for the center and recruitment as seasonal farm labor.

The failure of community enterprises was a part of the larger failure of self-government, the main reason for which was the prison-camp perspective of the majority of evacuees: since the government had abrogated their civil rights, they thought it should provide completely for their welfare. On this ground, too, they resisted resettlement, though the principal objection to resettlement was fear of adverse public opinion. Nevertheless, whereas community enterprises were a complete failure (except in the case of community consumer co-operatives), the policy of relocation was partially successful. From a quarter to a third of the evacuees were resettled throughout the nation or were serving in the armed forces by the beginning of 1945.

Resettlement was preceded early in 1943 by a leave-clearance program established to determine the "loyalty" of center residents.⁵ Concomitantly, army teams entered each center to register all male citizens over the age of seventeen for possible induction into the armed services. All registration forms, whether for draft classes or draft-exempt classes, contained the question: "Will you swear unqualified allegiance to the United States of America and faithfully defend the United States from any or all attack by foreign or domestic forces, and forswear any form of allegiance or obedience

⁵ Morton Grodzins, "Making Un-Americans," *American Journal of Sociology*, LX (May, 1955), 570-82.

to the Japanese emperor, or any other foreign government, power or organization?"

Many evacuees refused to register. Others registered but answered the question in the negative. American citizens among them resented having their loyalty questioned, while Japanese aliens, ineligible for American citizenship, feared that an affirmative answer would denationalize them and leave them without a country. However, the response to registration varied widely from center to center. At one extreme, 36.4 per cent of the population of Tule Lake refused to register or gave negative responses to the question; at the other, only 2.6 per cent of the residents of Granada responded negatively.⁶ In an effort to segregate the "loyals" from the "disloyals," Tule Lake was designated a segregation center, and all who had not answered the question affirmatively were sent there, while those in Tule Lake who had answered affirmatively were sent to other camps. Many subsequently registered affirmatively or changed from a negative answer and were thus granted leave clearance or declared subject to the draft.

Other issues arose in every center but were interpreted differently in different centers and were therefore resolved differently. Labor problems, the administration's attempt to weed out agitators, the food situation, and the shooting of evacuees by army sentries were other issues which mobilized interest.

Many evacuees did not question the legitimacy of relocation, but some were hostile to the government's efforts to make the centers function. Others felt that they had been made victims of coercive decrees which violated American democratic values. Data on how the majority of evacuees in any center came to accept the situation—whether they made some form of non-violent adjustment to it or found it unbearable and

reacted violently to it—led to the formulation of the hypothesis presented below.

The data were drawn from the descriptive accounts of relocation centers collected while they were still in operation.⁷ They consist of the reports of government officials, minutes of administrative and evacuee meetings, orders, proclamations and issues of center newspapers, and the like, the most important being reports by participant-observers.

The relationship of communication⁸ to conflict may be summarized as follows: the development and sharing of common symbols, which is necessary for reciprocal role-taking, depends upon situations which allow for the freest association. When free association is blocked, communication is impaired and divergent definitions are imputed to others. The development of collective protest is a function of the inability of contending parties to comprehend one another. In the absence of arbitration and negotiation, groups develop divergent definitions; and conflict ensues.

If groups are consulted on issues affecting their interests, rational discussion becomes possible. But, if they are excluded, there is little opening for controversy, for advocacy, and for defense—which introduce

⁷ This material, collected by the Japanese Evacuation and Resettlement Study of the University of California, plus the official publications of the United States War Relocation Authority, is held by the Documents Division of the University of California Library, Berkeley. It consists of the reports of government officials, minutes of administrative and evacuee meetings, orders, proclamations, issues of center newspapers, and reports of participant-observers employed by the University of California through funds allotted for the Evacuation and Resettlement Study. The footnotes to this article include citations to unpublished field documents, which, though inadequate, since they are in process of being recatalogued and reclassified, provide a lead to the documents held by the University of California Library.

⁶ War Relocation Authority, *Administrative Instruction, No. 22 (Revised), Suppl. No. 3* (Washington, D.C.: Government Printing Office, 1942), pp. 166 and 171.

⁸ See Charles Cooley, *Human Nature and the Social Order* (New York: Charles Scribner's Sons, 1922).

judgment and discrimination—and so their behavior is irrational.⁹

Since the formal structure of relocation centers was identical, attention was focused upon the manner in which administrative policy was implemented, communicated to evacuees, and sanctioned. Hypotheses were developed for the purpose of testing the relationship between formal and informal agencies of communication, as follows:

Administrative orientation.—Free communication or the blocking of communication depended upon administrative policy regarding the participation of evacuees in the settlement of disputes. As long as intermediate bodies functioned between the administration and the evacuees, collective protest never reached the stage of riot.

Channels of communication.—The greater the isolation of interest groups of the evacuees from officially sanctioned meetings, the greater was the divergency of meaning between them and the administration. Conversely, maximum communication reduced misunderstanding to a minimum.

Definition of the situation.—Group isolation increased the possibility of violent action stemming from divergent definitions of the situation. Conversely, the more negotiation between contending groups, the less the divergency of meanings and, therefore, the greater the probability of reasonable, non-violent behavior.

In order to test the hypotheses, the data were classified according to the degree of protest expressed in each incident. The basic unit of analysis here, the *incident*, is an issue in which the interests of the administration and those of a portion or all of the evacuees were in conflict. Any issue about which any kind of collective protest was mobilized has been considered, wherever reflected in the documents. An issue which aroused the interest of evacuees led to the formation of groups oriented toward

a resolution. These groups initiated certain actions which also led to further interaction within and among the groups. Incidents were terminated by a withdrawal of interest, because either the issue had been resolved satisfactorily or another issue was interesting contending groups. In either case the resolution of the issue often had an effect on subsequent issues.

One hundred and fifteen incidents were analyzed as to degree of collective protest and classified into the following categories arranged in descending order of protest (Tables 1, 2, and 3):¹⁰

Small-group protest.—These included verbal and written communications from the members of a block, a church congregation, or other special-interest group. The protests were directed against various administrative officials and concerned living conditions, minor incidents, or administrative regulations. Specific objects of protests included sanitary conditions in a mess hall or block latrine, safety and fire regulations in living quarters, and fire regulations for the entire center.

Petitions and proclamations.—These involved a relatively large proportion of a center's population and therefore concerned issues of a more general and more important nature from the standpoint of the evacuees. They were directed against top administrative

¹⁰ Qualitative content analysis was employed to organize the data. Qualitative content analysis is "the selection and rational organization of such categories as condense the substantive meanings of the given text, with a view to testing pertinent assumptions and hypotheses. These categories *may* or *may not* invite frequency counts" (Siegfried Krauer, "The Challenge of Qualitative Content Analysis," *Public Opinion Quarterly*, XVI, No. 4 [1952], 638). Though the data have been ordered, the criteria for ordering rest on an analysis of content which is qualitative and therefore highly subjective. "One and the same topic may invite different qualitative appraisals of almost equal plausibility . . . [but] communications which are sufficiently outspoken to canalize the imagination usually prove a powerful factor in bringing about a convergence of viewpoints and approaches. It is therefore a reasonable guess that different analysts will arrive at similar conclusions with regard to many texts" (*ibid.*, p. 641).

⁹ Herbert Blumer, "The Mass, the Public and Public Opinion," in B. Berelson and M. Janowitz (eds.), *Reader in Public Opinion and Communication* (Glencoe, Ill.: Free Press, 1953), p. 49.

tive officials, usually including the project director, and concerned such issues as the shooting of evacuees by army sentries, co-operative enterprises, labor recruitment, the loyalty oath, and Selective Service.

Strikes and labor disputes.—These involved bodies of various sizes in direct conflict with the administration. Specific incidents were over farm labor, motor transport, boiler-tender and carpenter strikes, disputes, and slow-downs. A limited number of persons were involved.

Mass resignations.—These protests on the part of elected and appointed evacuees were in direct defiance of the administration and affected the functioning of the entire center. They included general strikes or the resignation of community councilmen or block leaders and of various administrative committees over such issues as the loyalty oath and Selective Service.

Mass meetings.—These involved the largest proportion of a center's population and were characterized by strongly expressed opposition to the administration. They occurred over incidents such as the arrest of evacuees by the Federal Bureau of Investigation for alleged subversion and over the issue of registration.

Gang beatings.—These were extreme forms of protest by small, well-organized undercover groups directed against individual evacuees believed to be spying for the administration. They occurred during periods of extreme tension and served to solidify opposition to the administration.

Crowd behavior.—These protests were culminations of a series of less intensive forms of collective protest, with an intense concentration on the issue and a suspension of rational behavior.

Similar incidents involved varying degrees of collective protest as between one center and another. The type of incident has less bearing on the degree of collective protest than the already-established pattern of interaction and the actual interaction in the new situation: initially, frames of reference from old situations tended to structure the new situation, but the inter-

action which followed either changed or reinforced them.

Since the principal arena of communication was the meetings of self-governing bodies, most of the data was drawn from their minutes and from the reports of participant-observers. Various interest groups met to debate an event significant to them, thereby evolving definitions of formerly indefinite situations. Confused, misinformed, and varying definitions were held by the publics which mobilized immediately after a significant incident. The nature and functioning of agencies of communication (block meetings, council meetings, administrative staff meetings, mass meetings) determined the eventual definitions of the situation. Each incident was checked against the communication channels involved.

The next step was the coding of the data. Each event in the incident was entered on a summary sheet which contained space for the date and a general description in which the behavior of participating groups was classified under three categories: evacuee groups, mediation groups (if any), and administration groups. Further space was marked off for indicators of the actions from the primary sources: statements by participant-observers, minutes of meetings, formal rules and regulations passed, etc. All such statements pertaining to each incident were noted.

The entire incident was summarized on a coding sheet marked off for the listing of information pertinent to the propositions being tested. The incident was categorized according to the degree of utilization of channels of communication. Three categories comprised this rating scheme: "None"—avoidance or withdrawal; "Partial"—conflicts; and "Full"—consensus. In other words, if contending groups ceased to communicate during the process of the incident or never utilized the channels of communication, the incident was scored "None." If they communicated to some extent but tended to withdraw and return to intermittent communication, the incidents were scored "Partial." If communication

was full and consistent, the incidents were scored "Full."

Another rating system was employed to categorize degree of consensus. Whenever available, statements by representatives of each group concerning various stages of the incident were compared. They were classified in order of decreasing divergency as *a*, *b*, and *c*.

After the incidents in all centers were classified according to degree of collective protest, they were distributed by center. Centers were then grouped in terms of three principal types of collective protest in increasing degree of intensity: "successful negotiation of differences," "passive resistance and insubordination," and "overt rebellion." Inspection of the types and frequencies of collective protest gave the classification for each center (Tables 1, 2, and 3).

Incidents were further divided by date—whether before or after segregation. (Segregation refers to the movement of individuals who refused to attest loyalty to the United States government from their respective centers to Tule Lake, and the movement of evacuees who signed the loyalty oath from Tule Lake to other centers.) This temporal distinction reveals the effect on collective protest of the changed composition of center populations as to "loyals" and "disloyals." Many observers have stated that collective protest was directly related to the percentage of "disloyals" in a given center. There is such a relationship, as an inspection of the tables indicates. However, "loyalty" is not an intrinsic personal quality: refusal to sign the loyalty oath was related to specific situations. This is shown by the percentage of the total population of each center which was segregated in Tule Lake for refusing to sign the oath. It varied from 2.6 per cent of the population of Granada to 36.4 per cent of the population of Tule Lake. Interaction between the administration and the evacuees decided the evacuees' attitudes, whether of accommodation or of hostility, and

the attitudes found expression in the response to the loyalty oath. In the case of Mimidoka, a much higher percentage of incidents of collective protest occurred after segregation than prior to it, which indicated that the loyalty oath was not one of the issues which mobilized protest.

Collective protest was thus seen to be related to the formal administrative structure and informal evacuee groups. Communication was channeled through block meetings, council meetings, administrative staff meetings, and mass meetings. At these meetings various interest groups met to debate an event significant to them and to evolve definitions of situations that initially were indeterminate in that there were many different interpretations of them. The severity of subsequent collective protest depended upon the definitions of situations evolved at the meetings.

The incidents now to be described are over issues which aroused interest and provoked conflict between evacuees and administration.

TOPAZ KITCHEN STRIKE

Evacuees from the Pacific Coast began arriving at Central Utah Relocation Center, Topaz, Utah, early in September, 1942. On September 14 the kitchen work crew struck. It was an advance crew from the Tanforan Assembly Center in California which had come ahead of the first large group of evacuees to arrange mess facilities. The workers refused to work for the two Caucasian chief stewards, whom they characterized as arbitrary, dictatorial, and given to issuing conflicting orders, but, at the same time, they were very conciliatory, stating that they wished to co-operate with the director but could not work harmoniously with the chief stewards.

The project director called a meeting of members of the administrative staff, house captains, and strikers. The house captains, who were elected by the evacuees, were there in the capacity of a mediation committee. They promptly presented a plan: the evacuee cooks would run the dining halls, and the chief stewards would promise to stop their abusive and dictatorial ways. The project director backed this plan strongly, rebuked the stewards, and appointed an evacuee steward as liaison between

PROTESTS CHARACTERIZED BY OVERT REBELLION, BY CENTER

[illegible]

PROTESTS CHARACTERIZED BY PASSIVE RESISTANCE AND INSUBORDINATION, BY CENTER

<i>Colorado River:</i>							
Before segregation..	4	2	1	2	11
After segregation...	2	1	4	..	1	..	8
<i>Jerome:</i>							
Before segregation..	1	..	2	..	1	2	7
After segregation...	3	2	1	6
<i>Minidoka:</i>							
Before segregation..	2	1	3
After segregation...	2	2	8	12

PROTESTS CHARACTERIZED BY SUCCESSFUL NEGOTIATION, BY CENTER

<i>Gila:</i>	Before segregation..	2	2
	After segregation...	1	1	2	..	3	..	7
<i>Central Utah:</i>	Before segregation..	2	2	1	5
	After segregation...	2	3	5
<i>Heart Mountain:</i>	Before segregation..	1	2	3
	After segregation...	2	1	3
<i>Rohrer:</i>	Before segregation..	1	1	1	3
	After segregation...	1	1
<i>Granada:</i>	Before segregation..	1	1	2
	After segregation...

the administration and the chief cooks on matters pertaining to the kitchen.

Both sides accepted these conditions, and the strike ended. One evacuee observer wrote that, "from the point of view of the Japanese, their objectives were gained. It seems to me that the Caucasian bent over backward to win the cooperation of the Japanese cooks."¹¹

The following series of incidents illustrates the buildup of collective protest in a center where negotiating bodies were weak but never completely disappeared.

CROWD BEHAVIOR IN POSTON

On November 14, 1942, a thirty-year-old evacuee in Colorado River Relocation Center, near Poston, Arizona, was beaten by a group which he estimated to consist of eight or ten men. The next day the Internal Security Office, the evacuee police force in the center, arrested two men on charges of taking part in the beating. That night a married couple was beaten by a gang of about eight men.

On November 18 a "committee of seven," which claimed to represent all the evacuees in Poston, visited the project director and requested the release of the two prisoners. The committee was referred to the office of the Federal Bureau of Investigation, but its request was denied.

A crowd began to form before the center's jail. The project director addressed the assembled evacuees and urged them to disperse. They refused, and the crowd continued to grow throughout the day while various individuals urged a general strike. In the meantime the project director met with the community council and the Issei (Japanese immigrants) advisory board and urged them to exert their influence in disbanding the crowd. The council members replied by demanding the release of the two arrested men. When the project director refused, the council and the board resigned. Later that day the block managers resigned, and all formally recognized evacuee representation disappeared in Unit One of Poston, the section of the center where the trouble began.

In the meantime the evacuee leaders had sent two representatives from each block to meet as a governing body. This "committee of seventy-two" contained members of the three formally

recognized groups which had resigned that day—the council, the Issei advisory board, and the block managers' group. This group called a general strike of Unit One, and the crowd settled down to picket the jail in shifts.

The next day, November 19, the administration met with twelve representatives from Units Two and Three (the remaining sections), who had been appointed by evacuees as a mediating body between the administration and the committee of seventy-two from Unit One. Negotiations dragged on for several more days. The administration released one of the arrested men but refused to release the other. Emboldened by this concession and by their interpretation of evacuee sentiment, the negotiating committee from Units Two and Three demanded the unconditional release of the second man.

On November 23 the project director again met with the negotiating committee, which demanded that the arrested man be unconditionally released and all charges against him dropped. The director countered by proposing to give him a trial within the center. The meeting ended in a deadlock, but the following day the committee met with the project director and accepted his terms. The arrested man was released to the custody of the evacuee police for subsequent trial in the center by legally constituted authority. This was satisfactory to most evacuees, since the main point of contention was the administration's original intention to try the arrested men outside the center, and they feared that no evacuee would receive a fair trial on the outside because of hostility toward Japanese during the war years. The strike ended the following day, the pickets withdrew from the jail, and the evacuees returned to work.¹²

These incidents illustrate the interaction which occurred when the evacuees' interest was aroused. In every incident investigated in this study, evacuee groups withdrew from communication whenever they interpreted actions of the administration as dic-

¹¹ Fred Hoshiyama, "Social Disorganization" (MS [n.d.]), p. 3.

¹² This descriptive account of several incidents at Poston was drawn principally from the following sources: "Project Director's Weekly Reports" (MS, November, 1942); Norris James, "Final Report on the Disturbances and General Strike at the Colorado River War Relocation Center" (MS, December 11, 1942); Alexander Leighton, *The Governing of Men* (Princeton, N.J.: Princeton University Press, 1946); Tsuchiyama, "Aftermath of the Strike" (MS [n.d.]).

tatorial, as taking decisions about their welfare out of their hands. This occurred when there was no established procedure for arbitration and negotiation or when the administration discouraged negotiation or negotiated in an arbitrary or dictatorial manner (hypothesis 1).

In such instances evacuee groups met mostly among themselves, and, in the absence of opposed interests, consensus developed only within the parties in opposition and not among all the interacting groups (hypothesis 2).

The definitions of the situation, initially formulated by the members of interested groups, tended to diverge greatly from the definitions of other interested groups and were reinforced. When communication broke down completely, no reinterpretation of the situation was reached by either the administration or the evacuees, and riotous conduct ensued. When there were frequent meetings of a parliamentary nature, there was sufficient modification of initially divergent viewpoints, and rioting was averted (hypothesis 3).

In every case there was a relationship between the degree of collective protest and the channels of communication open at the time. In centers marked by successful negotiation of differences, channels of com-

munication had remained open during the incident, and a high degree of consensus had been achieved among contending parties by the time the incident terminated. The degree of consensus in each center varied from incident to incident, but the pattern of communication in early incidents tended to affect subsequent incidents. In centers marked by passive resistance and insubordination, channels of communication were weak, and a growing divergence of perspectives developed. In cases marked by overt rebellion, situations became defined variously by contending groups as they refused to arbitrate and negotiate.

Collective protest in the relocation centers was related to failure in communication. Without an arena for the rational consideration of issues which were common or complementary, there was no way in which perspectives could become shared. In Cooley's terms, without a high degree of communication, there was no possibility of reciprocal role-taking, since the latter rests on the sharing of definitions, and the sharing of definitions rests on the accessibility of people to one another.¹³

UNIVERSITY OF OKLAHOMA

¹³ *Op. cit.*, p. 288.

THE COUNTERACTANCE MODEL¹

STUART CARTER DODD

ABSTRACT

The proposed model operationally defines "counteractance" in three tenses by formulas specifying: how to identify it in fitting harmonic curves (i.e., $y = k/x^n$); how to produce it from preconditions of two measurable and opposed interactions where everyone has equal opportunity to interact; and how to predict in such situations. In four controlled experiments in series of towns an interaction in the form of the diffusion of a message was found with excellent fits to vary inversely with powers of four counteracting factors when isolated in turn, namely: the distance between interactors; the time sequence of interacting; the population size of interactors; and increments in stimulation.

The four pieces of research reviewed here were undertaken in order to learn more about how to describe and predict some mass activity with a resistive factor within it. The counteractance model was explored as part of a program of systematic exploration of dimensional formulas. The partly a priori and partly *ex post facto* analyses reported here aimed to find out more exactly what forms and amounts of resistance would fit models with negative exponents.

This model grew from at least three roots. An empirical root lay in two groups of studies,² those on interaction showing an

inverse relation to the distance intervening between interactors, and the studies of the size-rank rule in its diverse manifestations.

A second root, partly empirical, partly theoretical, came from the desire to test our hunch³ that the logarithmic Weber-Fechner formula⁴ in individual sensory psychology would apply to the stimulation and responding of whole communities. This logarithmic formula can be re-expressed in uncumulative form as the product of two variables equaling a constant, k , i.e.,

$$X^e Y = k \quad (1)$$

(the generalized hyperbola or harmonic series [in algebraic form]).

¹ This research was supported in part by the United States Air Force under Contract AF 33(038)-27522, monitored by the Human Resources Research Institute (now Officer Education Research Laboratory, Air Force Personnel and Training Research Center), Air Research and Development Command, Maxwell Air Force Base, Alabama. Permission is granted for reproduction, translation, publication, and distribution in part and in whole by or for the United States government.

² Joseph A. Cavanaugh, "Formulation, Analysis, and Testing of the Interactance Hypothesis" (unpublished Ph.D. thesis, University of Washington, 1950); Peter C. Dodd, "Social Physics: Theory and Applications of Demographic Gravitation" (unpublished thesis, Princeton University, 1950); Stuart C. Dodd, *Dimensions of Society* (New York: Macmillan Co., 1942), pp. 944; W. J. Reilly, *Method for the Study of Retail Relationships* (University of Texas Bull. 2944 [Austin, 1929]); J. Q. Stewart, "Demographic Gravitation: Evidence and Applications," *Sociometry*, Vol. XI, Nos. 1 and 2 (February-May, 1948); S. A. Stouffer, "Intervening Opportunities: A Theory Relating Mobility and Distance," *American Sociological Review*, V (December, 1940), 677-86; G. K. Zipf, *Human Behavior and the Principle of Least Effort* (Cambridge, Mass.: Addison-Wesley Press, 1949), chap. ix.

This is the simple hyperbola when the weighting exponent, e , is 1 and becomes the generalized hyperbola for other positive values of e . This formula for the constant product of two variables fitted a preliminary set of data when four communities were stimulated by airborne leaflet drops. So this harmonic series, equation (1), in its cumulated or log form was hypothesized as a model for such stimulus-response situations in communities.

A third and theoretical root was the dimensional analysis or S -system which was being developed into one of its special and predictive submodels called the "transact

³ Stuart C. Dodd, "Testing Message Diffusion from Person to Person," *Public Opinion Quarterly*, Vol. XVI (Spring, 1952).

⁴ J. P. Guilford, *Psychometric Methods* (New York: McGraw-Hill Book Co., 1936), p. 139.

mode."⁵ This six-dimensional *S*-formula for any "transaction" (defined as any action-in-explicit-context) factored any human action and its context into six factors capable of being standardized, or classes, of variables. It analyzed transactions as an algebraic product (which means socially "a joint occurrence") of *acts* (*A*) of *people* (*P*) in *time* (*T*) and *space* (*L*) for ends or *values* (*V*) and with means or residual *circumstances* (*C*).

These reliably observable and always present factors of human behavior answer the universal questions: "Who does what?" "When and where?" "Why?" and "How?" When this formula, capable of describing any behavioral situation whatever, was formally expressed, as a product of these six major dimensions, each weighted or developed by an exponent, it became the formula for a hypothesized behavior ($=B_h$), namely:

$$B_h \stackrel{\text{def.}}{=} A^a P^p T^t L^l V^v C^c \quad (2)$$

(the "transactance" model [in dimensional form]).⁶

Whenever any one of the six factors is isolated (making the exponents of the other five factors zero), and whenever the exponent on the isolated factor, *X*, is negative (expressing a resistive factor), this transact formula simplifies to the special case of the hyperbola which is our counteractance model, namely:

⁵ Stuart C. Dodd, "The Transact Model—A Predictive and Testable Theory of Social Action," *Sociometry*, December, 1955.

⁶ This set of six "factors" developed, as explicitly expected by us (*Dimensions of Society*), out of the four "sectors" of "time," "space," "people," and "all else" (*I*) by subclassifying the indexes of the residual sector (*I*) into the compound or non-elemental categories of "acts," "values" (i.e., desiderata), and a reduced residual category called "circumstances" (*C*). The smaller non-overlapping set of four categories, the sectors, is best for rigorous logical analyses. The six factors of the larger set have more overlap between them but are very useful for practical purposes in describing social phenomena.

$$B_h \stackrel{\text{def.}}{=} X^{-e} \quad (3)$$

(the "one-factor" counteractance model [in dimensional form])⁷

and

$$B_o = ? X^{-e} \text{ or } r_{ho} \stackrel{\text{hyp.}}{=} 1.0 \quad (3A)$$

(the counteractance hypothesis of high correlation between the behavior, as hypothesized, B_h , and as observed, B_o).

These three roots of the counteractance model grew vigorously when nourished by an Air Force contract to develop general principles (expressed in mathematical models) for diffusing messages by means of dropping airborne leaflets: "Project Revere." It proved admirably adapted for basic experimental research on human interaction, since it gave the research worker a high degree of control of his stimulation of both individuals and whole communities and of his measurement of their responses and the factors of their context. We selected the diffusion achieved as the criterion variable to be predicted and maximized. We defined diffusion most simply (among many indices tried) as the percentage of persons knowing the leaflet message, and this was determined either by interviewing a probability sample of residents in the locality three days after the leaflets were dropped or by tracer stubs torn off the leaflets and mailed back.

Project Revere contrived situations as experiments with varying cleanness of controls. These tests were designed to measure the form, amount, and statistical significance of the dependence of the observed diffusion transact, B_o , upon its six factors, each isolated in turn. Of the six, the four reported below were observed under certain conditions to vary inversely with the diffusion. Consequently, the four factors of distance, time, population, and stimulation strength are brought together here in order to illus-

⁷ For the differences between dimensional and statistical forms of formulas see Dodd, *Dimensions of Society*; Dodd, "The Transact Model . . .," *op. cit.*; and works cited in nn. 9 and 10 below.

trate and contribute to delimiting, and in some cases to testing, a general counteractance model.

Algebraically, the counteractance model signifies: The product of two variables (X , Y in eq. [1] or X , B in eq. [3]) is constant, or each varies inversely with the other (when their exponents are 1). Exponents on X and Y other than 1 say further that the two variables may have any relative weighting as specified by the ratio of their exponents. If for convenience' sake we take any exponent on Y as unity and let the exponent on X vary accordingly, we have an exponential ratio of $e/1$ or just e .

Socially, the formula states: These symbols represent a closed system in which there are just two relevant variables related as follows: (1) they interact with equal opportunity; (2) they counteract each other; (3) they may act unequally; and (4) their result or end action is constant throughout the situation studied. These relations which are assumed and expressed in equation (1) are the social preconditions that are hypothesized to exist whenever the model is used or tested. Insofar as this model fits given data, it means that these four preconditions tend to exist, fully and solely, in those data.

WHAT FACTORS FIT THE COUNTERACTANCE MODEL

Among the twenty-seven series of tests in Project Revere the counteractance model was observed to fit four of the six factors of a transact.⁸ (A "transact" here denotes any transaction when observed in all-or-none form.) When all factors but one were controlled to a high degree, the diffusion of a leaflet message was found to tend to vary inversely in turn with (1) the distance between interactors; (2) the sequence of interacting; (3) the number of interactors; and (4) the increments in the stimulation to

interact.⁹ To review the four experiments briefly:

Distance.—The dependence of the diffusion transact or one-way interact in a population upon the distance across which the message diffuses was isolated and measured in one community as follows: We launched a six-word message—a coffee company's new slogan—in a town of 905 inhabitants one Monday morning by ringing the doorbells of a 20 per cent probability sample of the 303 dwelling units. The interviewers in "Operation Coffee" told the slogan to each housewife in the sample, suggesting that she could do her friends a good turn by repeating it to them. The interviewers promised to deliver a free pound of coffee to every housewife who would know the slogan when they would call at every household on Wednesday. The slogan spread to 82 per cent of the housewives. The distribution of the distance between the homes of each teller and her hearers was a close fit to the waning curve hypothesized by the hyperbola or "inverse distance" formula as shown in Figure 1L. The small discrepancies representing the deviations of the observed data from the model are attributable to sampling fluctuations, as they were not significant by the chi-square test: $p(\chi^2) = .85$. In these data, then, isolating and measuring the distance factor showed almost perfect dependence of the transact upon it. The correlation of the amount of diffusion with the inverse intervening distance was as near perfect ($r = .997$) as behavioral scientists may hope for. This curve, defined by equation (1) or (3), was later christened a spatial subcase of the "counteractance" model.

This extremely high correlation evidences and was made possible by a high degree of control over the other transact factors in "Operation Coffee." The act of retelling the slogan and the react of hearing it were suffi-

⁸ Stuart C. Dodd, "Diffusion Is Predictable—Testing Probability Models for Laws of Interaction," *American Sociological Review*, Vol. XX, No. 4 (August, 1955).

⁹ For fuller account cf. Stuart C. Dodd, E. D. Rainboth, and J. Nehnevajsa, "Revere Studies on Interaction" (to be published). For a report on "Operation Town Size" see S. C. Dodd, "A Power of Town Size Predicts Its Internal Interacting," *Social Forces*, 1957 (in press).

ciently similar for all tellers to have introduced not even 1 per cent of attenuation in the correlation. The time available to the starters was about the same for all of them. The motivation was identical—a pound of coffee to each friend—however much individuals varied in appreciating it. The size of population was constant at 303 housewives throughout the two days of the experiment. The usual galaxy of other known and unknown circumstances in a community were present, of course, but they are proved

not to be correlated with the diffusion and the distance, or at least to cause no disturbing effects upon the diffusion-distance relation by the following observed fact: *The combination of all variables other than distance here produced only six-tenths of 1 per cent of error of estimate (i.e., $k^2 = 1 - r^2 = 1.0 - .997^2 = .006$), when estimating the transact from the distance factor via their regression equation.*

The distance was only an easily observed index of the counteraction, which was here

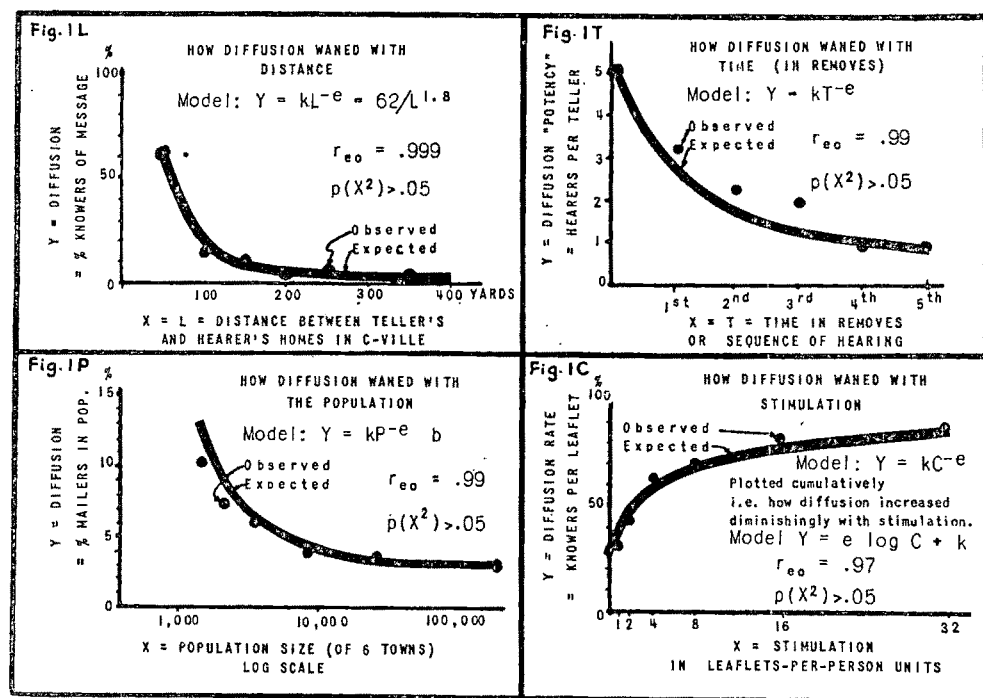


FIG. 1.—The counteractance model-fitting message diffusion data to four factors of its context. The generalized harmonic series or hyperbola defined by $Y = kX^{-e}$ was observed in Project Revere to fit as specified below, the dependence of the diffusion ($= Y$) of a leaflet message upon each of its four factors ($= X$) in turn. The factors were: in Fig. 1L the *distance* (i.e., $X = L$) between interactors; in Fig. 1T the *time* (i.e., $X = T$) between interactions; in Fig. 1P the *population* (i.e., $X = P$) of interactors; and in Fig. 1C the *circumstance* (i.e., $X = C$) of amount of *stimulation* to interact. The excellent fits here show that diffusion waned with distance and time, with the log of the population, and with increments of stimulation.

This counteractance model is a special case of the dimensional transactance formula (see S. C. Dodd, "The Transact Model," *Sociometry*, December, 1955), viz.:

$$B_s \text{ def } A^a P^b T^c L^d V^e C^f,$$

where the behavior-in-context, B_s , as expected or predicted, is defined as an exponentially weighted product of six factors, i.e., the acts (A) of people (P) in time (T) and space (L) for ends or values (V) and with means or residual circumstances (C). The counteractance submodel isolates any transact factor (X) in a closed system where that factor counters or resists the transacting, as shown by a negative exponent.

the resistive behavior or effort required as "a cost" in telling the slogan to a friend. Of the total effort to tell evoked in those housewives by the interviewers' promise, part of the effect was manifest in the amount of telling achieved, while another unmanifest part of the total effort was absorbed or canceled out or counteracted by the distance each

dict, for further test, that if distance measures resistance or countering action (here presumably effort) to the mass interaction studied; and if the amount of the total interaction at issue (defined as of one kind in one population in one period and area for constant motivation and residual circumstances) is constant (or can be treated so by

TABLE 1

DIFFUSION RELATED TO FOUR OF ITS FACTORS

TABLE 1L

DIFFUSION AND DISTANCE
IN A TOWN OF 905 POPULATION

Distance (50-Yard Units)	Diffusion ("Social") (= % Pop. Hearing)	
	Expected (<i>h</i>)	Observed (<i>o</i>)
1.....	61.9	61.9
2.....	17.8	14.2
3.....	8.6	9.5
4.....	5.1	4.8
5.....	3.4	4.8
6.....	2.5	2.4
7.....	1.9	2.4

$r_{ho} = .997 \quad p(\chi^2) > .85$

TABLE 1T

DIFFUSION AND TIME

Removes	Diffusion Potency (= Mean Hearers per Teller)	
	Expected	Observed
0.....	5.12	5.12
1.....	2.56	3.20
2.....	1.71	2.36
3.....	1.28	2.00
4.....	1.02	1.00
5.....	.85	1.00

$r_{ho} = .99 \quad p(\chi^2) > .05$

TABLE 1P

DIFFUSION AND POPULATION IN SIX TOWNS

Town Population	Diffusion ("Social and Physical") (= % Pop. Mailing Back)	
	Expected	Observed
1,304.....	13.1	10.3
2,013.....	9.58	7.3
4,150.....	6.24	6.1
8,430.....	4.65	4.0
34,600.....	3.48	3.9
325,994.....	3.14	3.1

$r_{ho} = .99 \quad p(\chi^2) > .05$

TABLE 1C

DIFFUSION AND STIMULATION IN
EIGHT MATCHED TOWNS

Leaflet Ratio (= Stim.)	Diffusion ("Social and Physical") (= % Pop. Know- ing Message)	
	Expected	Observed
1/4.....	25.2	25.2
1/2.....	34.0	37.4
1/1.....	42.9	30.1
2/1.....	51.7	44.1
4/1.....	60.6	63.4
8/1.....	69.4	71.8
16/1.....	78.3	82.4
32/1.....	87.1	87.9

$r_{ho} = .97 \quad p(\chi^2) > .05$

teller had to walk. (There was no driving or bicycling in this little town of a few hundred yards' diameter. The 5 per cent who telephoned the slogan are not included in the data.) Since the ergs of physical energy spent in walking are fairly proportional to the distance walked, that distance measured well the unmanifest and consumed effort which "counteracted" some of the diffusing.

This experiment adds strong evidence toward confirming the inverse distance sub-form of the counteractance hypothesis or model. By this model or hypothesis we pre-

measuring it as a base for percentages); and if the interaction and the countering action are isolated so as not to be correlated or "overlaid" by other variables, then the amount of the interaction will tend to vary inversely with a power of the intervening distances, i.e., equation (3) will fit the facts.

A social "law of inverse distance," when it becomes eventually more exactly delimited and confirmed, will probably say in effect: *If other factors are constant, interacting wanes with a power of the distance between the interactors.* This is the distance-with-a-

negative exponent L^{-1} part of our transact model equation (2) and of our all-inclusive dimensional model.¹⁰

Time.—The dependence of the diffusion transact upon the time during which it transpires was isolated and measured in the same community as follows: On Wednesday each housewife was asked to identify who told her the coffee slogan, where, and when. From these replies the growth of the diffusion, or percentage in the town who knew the message, was determined in both ordinal and cardinal units of time. In cardinal units of clock minutes and hours the daily rhythms of eating, working, recreating, and sleeping interfered with observing any regular curve of growth for the person-to-person diffusing of the message. But in ordinal units of the order or sequence of passing it on (at one, two, three, etc., removes from the starter), the data showed more regularity. The removes were uncorrelated with the daily rhythm and so canceled it out or controlled its effect. The regression of the ordinal time index upon the cardinal time was a straight line.

When the potency rate of diffusion, defined as the "first hearers" per teller per remove, was computed, we discovered a previously unsuspected effect. As shown in Figure 1T, the potency rate tended to wane harmonically, varying inversely with the time in remove units. Again the fit of the hyperbola or counteractance model to these time data was good (Fig. 1T). The correlation coefficient of observed and theoretic points was .99, and the discrepancies were not significant by the chi-square test at the 5 per cent level.

Time in ordinal units was well isolated from the other factors or uncorrelated with them. The removes were observed to be uncorrelated with distances. As above, the interact, the stimulation and motivation of it, and the population of the town were all con-

stant or alike for everyone as far as observable during the forty-eight-hour experiment. A correlation of .99 means that other factors could account at most for 2 per cent of the variance as an error of estimate in the regression equation (i.e., $k^2 = 1 - r^2 = 1 - .99^2 = .02$).

The finding that the potency rate or activity rate in diffusing tended to vary inversely with time, though unexpected in the first exploratory test, was supported in a dozen other tests in Project Revere. The harmonic logistic model—which includes the factor of a harmonically waning activity rate—proved by far the model best fitting the growth of diffusion throughout a dozen diverse testings.¹¹ Consequently, we now expect the potency rate for an interaction, when set off by stimulation at one point in time, to wane in general, and we have developed three mechanisms or hypotheses to explain the harmonic form of such waning and are now testing them in further experiments.

The hypotheses to explain the harmonic waning potency are the overlaying, the drop-out, and the rebuffing effect. The overlaying effect means that an average event in one period will in general be overlaid by n other average events after n periods and so likely to get only $1/n$ th of the public's attention in the n th period. The public's activity relative to a past event will then probably wane, i.e., will vary inversely, with the elapsed time. The relative strength or interest of the succeeding events compared with the initial stimulus is measurable by the exponent in equation (1) or (3) when the factor X represents time ($X = T$).

The drop-out effect hypothesizes that individual differences of interest in retelling the message result in the less-interested persons dropping out of the telling group first. This means a waning number of tellers as time goes on and so a waning total rate of telling. The persons least interested may be thought of as having more "overlaying" in-

¹⁰ Stuart C. Dodd, *Systematic Social Science* ("American University of Beirut, Social Science Series," No. 6 [Beirut, 1947]), pp. 785 (University Bookstore, Seattle, Agents). Cf. Dodd, *Dimensions of Society*.

¹¹ Stuart C. Dodd, "Testing Message Diffusion in Harmonic Logistic Curves," *Psychometrika*, Vol. XXI, No. 2 (June, 1956).

terests in other matters, so that their progressive ceasing to retell can also be seen as due to competition with other activities.

The rebuffing effect hypothesizes the same result but by a more social mechanism. It proposes that people do less and less telling in proportion as they are rebuffed by hearers who interrupt, saying they have already heard the message. Such rebuffs will increase with an increasing proportion of message-knowers, and this, in turn, increases with time. So rebuffs may be a counteraction which makes diffusing wane with time. And, of course, all three mechanisms, which counteract the diffusing with time, may operate jointly. Further experiments, testing models which isolate the overlaying, individual differences, or rebuffing effects, can delimit the shares of each mechanism in turn under its special preconditions.

Whatever the more specific nature of the separate or joint counteracting mechanisms may prove to be, we hypothesize that if time since an event measures some counteraction to the interaction of people relative to that event, and if that total interaction is constant in amount or can be treated so, and if, as usual, other factors are either constant or uncorrelated so that the time factor can be observed in isolation, then the interaction at issue will tend to vary inversely with a power of the elapsed time. Then equation (3)—with time as the counteractant index—will fit the facts, i.e., $B_o = T^{-e}$. More simply, we expect a social “law of inverse time”—to be confirmed through research by others—as follows: *If other factors are constant, interacting of people about a past event tends to wane in proportion to a power of the time elapsed.*¹² We believe that this model will fit data better and better according as all the factors are better isolated, measured, and controlled.

Population.—This hypothesis had its in-

ception during Project Revere in the test on six towns called “Operation Town Size.” The results described here grew out of this exploratory treatment of data gathered in other experiments rather than from a model designed to test the hypothesis rigorously in a new situation (as was the case with the inverse distance hypothesis above, which had had empirical roots in previous studies, already referred to, and the inverse time hypothesis where the unexpected finding in the exploratory test situation was supported by later tests in Project Revere itself). Consequently, we present this hypothesis with only that degree of assurance which cleanliness of results in an initial study permits.

The dependence of the observed diffusion transact, B_o , upon the size of the interacting population was isolated and measured in Project Revere as follows: One leaflet per person was dropped comparably by planes on six communities whose population varied from 1,300 to 326,000. The total response mailed back as shown in Figure 1P fitted the counteractance model excellently. The correlation coefficient between the observed data and the harmonic curve (with an exponent of -1) was .99. The chi-square test of the discrepancies was not significant at the 5 per cent level.

For control of the five transact factors in equation (2) other than population, we note that the act of mailing back, its stimulation and motivation, and the time allowed for it were the same for all six communities. Distance was well controlled, since the plane spread the leaflets as evenly as possible over the whole area of each community so all the people could pick up leaflets at about the same distances from their doorsteps.

To explain the observed fact that the percentage diffusion varied inversely with the population, we propose a “counteractive interstimulation” subhypothesis (a hypothesis as to mechanism). The explanation by interstimulation proposes that, the larger the towns, the more the increase in average daily stimulation of all sorts playing on each person will outstrip the increase of population. If the interstimulation of people were

¹² We expect this harmonic rule to hold also for future or anticipated events where preparatory interaction tends to increase as the event gets near. Thus, in general, this hypothesis is: Interaction about a past or future event tends to vary inversely with the intervening time, when other factors are constant.

in pairs only, it would increase up to a theoretic ceiling proportional not simply to the population but to its square. This square is a theoretic upper limit for pair interaction, since everyone cannot interact daily and directly with everyone as cities get large.¹³ Now the stimulation of the leaflet was constant in per capita size for all towns. Consequently, the ratio of stimulation by the leaflet to total stimulation on each person would shrink as size of city was greater. The stimulation of the leaflet varied among towns exactly as the first power of the population; the total interstimulation of people in pairs, we argue, would increase at more than the population rate of growth and perhaps approach the square of the population; their ratio indicating the share of the public attention commanded by the leaflets should vary at most as the inverse first power of the population, i.e., $P/P^2 = P^{-1}$. The fit was tried for each tenth of a point of the exponent and found to be about equally good from $P^{-.7}$ to $P^{-1.0}$.

The larger populations generate more interstimulation than the smaller, and the enlarged total stimulation competes with the stimulation of the leaflet and counteracts some of the potential responses mailed back. The relative response to the leaflets in small and large towns is reflected in the small townner's excited narrative when planes rained leaflets on his town—as compared with blasé metropolitan comment, "Uh huh, just another advertising stunt."

Consequently, though more tentatively than for previous factors, the third factor's relation to a transact is formulated as follows: If population size measures some counteracting factor—perhaps total interstimulation—and if other factors are constant or uncorrelated, then the partial interaction per capita at issue will tend to vary inversely with a power of the population. Then equation (3), rewritten for the population factor, $B_o = P^{-e}$, will fit the facts. This hypothesis if confirmed would

become the social "law of inverse population." It might be roughly: *If other factors are constant, the interaction per person about some stimulus, constant per person from town to town, wanes with larger populations.*

Increments of stimulation.—For the fourth set of experimental data, the dependence of the diffusion transact upon the increases in stimulation was demonstrated in the test called "Operation Leaflet Ratios." Leaflets were comparably dropped on eight matched small towns. The number of leaflets was doubled from town to town in the series, rising from one leaflet to every four persons to thirty-two leaflets per person in the eighth town. As the stimulation of these communities rose in geometric ratio, their responses were observed to rise in arithmetic ratio, as had been hypothesized. Each doubling of strength of stimulation yielded about nine percentage points of increase in the community response measured by the percentage of knowers of the message. This logarithmic relation of stimulation to response fitted the observed data well. The correlation coefficient between the log model and the observed diffusion was .97, with non-significant discrepancies by chi-square test at the 5 per cent level. The log curve is the cumulative or integrated form of the hyperbola (for continuous data which are approximated by the discrete harmonic series or inverse natural numbers: $1/1, 1/2, 1/3, 1/4, \dots, 1/N$). It is the counteractance model again as graphed in log form in Figure 1C.¹⁴

Again, in this test one transact factor—a circumstance—was isolated, varied under control, and its effect measured. Again, the other five transact factors were held fairly constant by selecting matched towns and by giving them identical treatment except for controlled varying of the one factor at issue.

¹⁴ In harmonic form the fit is looser than in the cumulated or log form reported above. As figure 1C pictures, one town showed a negative instead of the expected positive increment compared with the previous town in the series. The Weber-Fechner principle is erratic at the extremes such as below one leaflet per person, apparently. But, aside from the extremes, the fit of the harmonic curve is excellent.

¹³ Further increased interacting in larger groups than pairs, owing to more roles and more symbolic mediation in cities, would increase the interstimulation by amounts at present unknown.

For interpretation, one asks how is it possible that increasing the stimulation can counteract the response? The answer is that the absolute response increased but increased diminishingly (Fig. 1C). As the number of leaflets increases, each evokes a smaller *share* of the total response of a community. Proliferating leaflets appear to compete with each other, reducing the share of each in the diffusion produced. Thus, the more leaflets, the more some of the effect of each is counteracted.

Consequently, the relation of this fourth factor constituting a circumstance in a transact, much as in the cases of the other three, is stated as: If increasing stimulation evokes a decreased rate of interactive response, if that total responsive interaction is measurable, and if other factors are constant or uncorrelated, then increments of the interaction at issue will tend to vary inversely with some power of the stimulation.

As far then as we can now state it, the upshot seems to be an eventual social "law of inverse stimulation": *Insofar as other factors are constant, per capita increments of reaction between lower and upper bounds of reaction wane with the amount of stimulation of one kind.*

The hypotheses.—With these four sets of well-fitted data before us (Fig. 1), let us examine more closely the set of counteractance hypotheses as candidate laws for sociology. The counteractance model may be thought of as an operationally defined theory comprising five aspects, or a main hypothesis with five subhypotheses. Each of the five asserts one precondition or assumption; taken together, they specify the model, which is a statement of a theory in explicit and testable terms.

The main "counteractance hypothesis" proposes that the model, equation (1), will fit closely and surely the observed data (insofar as the observations were made under the preconditions specified in the model, of course). This summary hypothesis is tested by the degree of closeness and sureness of the fit. How closely and with what probability does the theory agree with the facts? A descriptive index such as an appropriate

form of correlation index, r_{ho} , can measure the closeness of fit of the observed and expected variables (i.e., the data in hand and the model). A probability index from sampling statistics, such as the chi-square test for the significance of the observed discrepancies from the model, can measure the sureness of the fit. To apply these tests, the "counteractance" index, B_h , defines the behavior as expected by the product of the six factors as in equation (2). Then the "counteract" index, B_o , denotes the behavior as observed which serves to validate or invalidate the hypothesis. One may then say: The formula describes and may be expected thereafter to predict such behavior under those preconditions.

The five hypotheses specify the five assumptions implicit in the counteractance formula, equation (1). These are:

1. *Countering.*—The two variables, X and Y , are inversely related to each other ($X = 1/Y$) and so have perfect negative, curvilinear correlation. Socially, this means the two indexes are completely opposite or in full conflict as in representing an act (Y) and a counter to it or resistance (X). The countereffect, whatever its cause, may be very indirectly measured by some observable index such as in our data where distance presumably measured effort to overcome it, time measured perhaps competing events displacing an action, population measured (we hypothesize) distracting stimulation and the response to it, and increments in stimulation may have measured detraction from the share of response produced by each unit of stimulation.

2. *Interacting.*—The two variables algebraically are multiplied together, forming a product (XY), not a sum ($X + Y$). Socially, this represents interaction or joint action where each variable modifies the other and not independent or alternative actions (which may be represented by algebraic sums or logical sums). This means that the product or joint result of the two variables may be a pattern or emergent new whole which is more than the sum of its parts. The effect of the two variables is one unified act—such as telling-a-message-

in-spite-of-resistance—not an aggregate of acts, such as telling several messages.

An a priori test or "precondition" for this precondition of multiplicative combining of the two variables in equation (1) is the "vanishing-result" test. If any factor in a product vanishes (i.e., = 0), the product vanishes; but an addend in a sum may vanish without wiping out the sum. The four factors of distance, time, people, and stimulation (like all the factors of every transact) *always* satisfy this vanishing-result test. For every transact, every human act, must become non-existent if there is no doer (P), no time (T) or space (L) whatever to behave in, no values (V) or verbalizable wants, and no context (C) whatever, material or symbolic. These factors may vary as four of them did in turn in our data above, or they may be constant at some non-zero amount, of course; but they cannot become absolutely zero if the transact exists at all.

3. *Unequal acting*.—The two variables are algebraically specified by the counteractance model, equation (1) or (3), to have any relative weighting depending on the value of the exponent, e . Socially, this means the act (Y) and its counteract (X) may be of equal or unequal importance and effect. At one extreme, when the exponent is zero, the counteract has no effect on communicating (as in the case of phoning within one exchange area). At the other extreme, when the exponent is infinite, the counteract is all-important and completely prevents the act, as when the Atlantic Ocean, before it was ever crossed, completely prevented any communication across it. In between, when the exponent e is 1, the counteract equals the act in importance—each is half the total activity. This is the case of the simple harmonic series or equilateral hyperbola.¹⁵

¹⁵ It is also the case of unit elasticity in supply and demand curves in economics. It is the case of a constant demand (= k in eq. [1]) which makes price and supply inverse to each other. The relation of the counteractance model to demand-supply-prices is explored in some detail in Dodd, *Systematic Social Science*, chap. xii.

4. *Definite total action*.—The algebraic product of the two variables is a constant ($XY = k$ in equation [1]). Socially, this seems to mean that there is a definite total amount of relevant behavior or human energy in the situation. This total is (a) either constant over all values of the independent or counteracting variable, X , or else (b) taken as a base for percentage expression of X and of Y as representing proportions of that variable but definite total of energy. (The proportions must be such that the sum of the logs which represent the two proportions, p, q , is a constant, i.e., $\ln X + \ln Y = \ln k$ or $p + q = 1$).¹⁶ By this percentage device one can deal with a manifest part, Y , of the total and variable activity, and a second unmanifest and counteracted part, indirectly indicated by X .

5. *Equal opportunity*.—The fifth assumption or social precondition underlying the counteractance model relates the dimensions of actors to the acts. This relation can be expressed statistically as a rectangular distribution and socially means an equal opportunity or equal probability for each actor to act. Thus it is assumed that, in walking X units of distance, each unit consumes an equal share of the actor's energy. Again, in reacting to stimuli on X successive days in the second set of data, we assume the daily reactions to be about equal, so that after X days each day probably received $1/X$ of the total reactions. Similarly, we assume the reaction per leaflet to be equal for all leaflets dropped in a given operation, i.e., each leaflet has a social opportunity or mathematical probability of being picked up that is equal for all leaflets dropped on the one occasion. This

¹⁶ We speculate here on possible relations between this equation of log proportions and information theory which defines information in terms of logs and probabilities. "Information" (= H) is being increasingly linked to the concepts of "organization" and of "negative entropy." Entropy and organization are counteractive tendencies for the universe to run down and to build up. Is it possible that the counteraction equation expresses in primitively simple form the cosmic equilibrium of the organizing forces and the counteractive disorganizing forces within a constant cosmos?

amounts to taking the average share of opportunity, $1/X$, as the statistical probability, p , that is associated with or represents each leaflet. In the last set of data the equal opportunity was assumed in taking the average or per capita interstimulation in each town as representing each person—for purposes of mass comparisons.

The assumptions are not independent. They have some logical redundancy as in presenting the two factors (X , Y) either as reciprocals or as forming a constant product (i.e., $X = 1/Y$ implies $XY = 1$). But the redundancy may be useful as when, in examining a new situation, certain of the five preconditions turn out to be more observable than others of them, and this helps in deciding on the applicability of the general model and the form in which to observe it.

The five explanatory preconditions seem to depend on form more than content, on the symbolic analysis chosen more than on the phenomena or their "intrinsic nature"—a major tenet underlying all dimensional analysis of which this paper is but another example. The counteractance model brings together behavior of any institutional or other content, provided its symbols are related together as specified by the formula. Re-expressing phenomena by the symbols gives sociologists observable, verifiable, and predictable ways of dealing with phenomena. In fact, all laws in science as such are seen as a search for formal regularities and unification, provided the forms or formulas in turn have one-to-one correspondence to observables.

The five assumptions or social preconditions can be viewed as a "shrinking share" explanation of the simple harmonic series, $1/1$, $1/2$, $1/3$, $1/4$, ..., $1/N$. Shrinking shares are easily understood by the instance of slicing a pie: any whole pie when sliced into N equal slices will necessarily give slices of size $1/N$; the more the slices, the smaller they are. The number and size of slices must vary inversely to each other, if the slices are equal. If unequal, one approximates the situation by saying "the number and average size of slice are mutu-

ally reciprocal." This way of looking at, or "explaining," all cases of data which fit the counteractance model, we have thus far found to be more satisfactory than Zipf's explanation by "least effort"¹⁷ or Simons' by probability.¹⁸ On more vigorous logical analysis the three explanations may prove to be largely differing language statements of the same referent; meantime, the slices of pie offer a fruitful concept. If total distance walked is the "pie," the more the units of distance, the smaller is each unit's slice of total energy. If total reactions in a given time are the "pie," the more days, the smaller will be each day's slice of the total reactions. If total reaction to leaflets is the "pie," the more leaflets, the smaller each slice of reaction per leaflet. If total reaction to all kinds of interstimulation in a town is the "pie," then the more numerous the people and their units of interstimulation, the smaller, relatively, will be each unit slice such as one leaflet per person.

Thus the counteractance model describes and should predict new situations in the future wherever a single total human activity is the joint result, in some ratio, of a manifested part or action (Y) and a resistive part or counteraction (X). This last sentence, summarizing the assumptions in the counteractance model, is more exactly and universally stated in formal algebraic terms by $Y = kX^{-e}$ (eq. [1]).

WHY THE COUNTERACTANCE MODEL MAY BE USEFUL

Model-testing should yield validated models which then become formulas or rules telling social engineers how to pursue their ends by the most efficient means. Thus the model of inverse distance tells

¹⁷ *Op. cit.*

¹⁸ Herbert A. Simons, "On a Class of Skew Distribution Functions," *Biometrika*, Vol. LXII, Parts III and IV (December, 1955). Simons, whose article came to our attention after the original writing of this paper, reports a stochastic and probabilistic analysis which seems compatible with ours. His model seems to us to delineate a mechanism underlying our terms of "counteracting," "interacting," etc.

the operator how to distribute leaflets to the greatest number of people by plane. Many points, approaching even distribution over a territory, will reach more people than one or a few peaks. The rule of inverse time tells how to schedule dropping leaflets so as to maximize the desired interaction at the desired times. The rule of inverse population tells him how to choose the towns and missions for his planes so as to reach the greatest number of people. The rule of inverse stimulus tells him how many leaflets to drop on a given population with largest probability of reaching most of them with least waste.

All these rules, if confirmed by tests on other sorts of mass reaction, can help build up a body of laws of social mechanics as dependable as those of physical mechanics in its early years.¹⁹ For the counteractance model tested here was not dependent on the particular kind of behavior used in testing it, that is, on the reaction to leaflet messages. Nor is it dependent on the situation, such as that involving civil defense interest in the United States in 1952-53.

¹⁹ Stuart C. Dodd, "Formulas for Spreading Opinions—a Report of Controlled Experiments on Leaflet Messages in Project Revere," *Public Opinion Quarterly* (in press).

Nor is it dependent on the American culture or industrial civilization. With local exponents, coefficients, and units, the same *shape* of curve defined by equations (1) and (3), we predict, will recur wherever a definite mass action with a counteracting factor within it is measured in isolation from other factors. It is chiefly, we believe, a matter of sufficient resources for such social research being provided, for such formulas to be extended to more than one counteracting variable, to diverse classes of acts and counteracts, and in diverse contexts of people and purpose, period and place, and attendant circumstances.²⁰

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²⁰ In the long run the immediate and practical uses of the counteractance model, as of any highly general or dimensional model, will be outweighed, we believe, by its methodological and systematizing uses. The counteractance model is a subcase of our powers model, which is a subcase of our transact model, for all social behavior, and this, in turn, is a subcase of our dimensional formula for all symbolizable knowledge. As the counteractance model is verified by other investigators, it will contribute to verifying the dimensional system of sociology. The bit of that system here tested simply isolated the relation of an action to a counteraction as expressed by a negative exponent, eq. (3).

PSYCHIATRY IN MEDICINE: INTRA- OR INTER- PROFESSIONAL RELATIONSHIPS?¹

HARVEY L. SMITH

ABSTRACT

The role of psychiatry in medicine demonstrates the dissociative forces within a profession and the range of differences tolerated. Divisive forces include technical skills, conceptual systems, scientific orientation, historical background, and problems of confidentiality. Integrative forces are the professional politics of psychiatry and medicine, changing concepts in medicine, high social rewards to physicians, and, most important, the continuing identification provided by the shared early medical training. The result is the conflict-filled role-reciprocal of psychiatrist-physician. The responses of individual psychiatrists to the conflicting strains of differentiation and integration provide an area for collaborative research by psychiatry and sociology.

In the study of occupations and professions little attention has been paid to the range of differences in behavior which may be encompassed within a single profession. Rather, there has been concentration upon establishing the norms, the central tendencies, of professional behavior. Erroneous ideas have thus emerged concerning the monolithic unity of some professions. It is the purpose here to explore the range and limits of toleration of difference within a profession, with a view to considering the questions of how such differentiation develops and is maintained and how occupational (professional) integration and identification are effected. For this purpose the role of psychiatry as a marginal man of the medical profession is analyzed.

When one observes psychiatry in medicine in terms of the organization and integration of a profession, it becomes clear that in many important aspects it is subject to quite deviant contingencies of survival. The technical skills peculiar to psychiatry are important in accounting for its marginal status in medicine. As T. H. Marshall, writ-

ing of the development of professional organization, has stated, "the foundation of the whole structure is the specialized technique."² The specialized technique of psychiatry has moved with time from a medically respected base in neurology to a psychodynamic orientation with emphasis upon psychotherapy. The peculiar techniques of psychotherapy, which might be called the unique skill of psychiatry in medicine, were developed outside the body of organized medicine and are, in fact, broadly shared with members of other, non-medical occupations and professions.

Psychotherapy is of central importance in the emergence of psychiatry as a distinctive subspecialty in medicine, and yet it has had a major share in assigning psychiatry to the margins. Psychotherapy is, so to speak, "conceptual medicine," with the competing theoretical systems of psychiatry providing a series of "as-if" systems for understanding and changing human behavior through a relationship. This is fairly different from the rest of medicine, which usually manages to keep at least one hand firmly on the body. The difference is a source of strain for many psychiatrists, and within the specialty there is much seeking after "organic" proofs of psychotherapeutic in-

¹ This is a revision of a paper presented at the annual meeting of the American Sociological Society, Washington, D.C., September, 1955. Data were secured through several years of research, teaching, and administration in medical and psychiatric situations and also from a study of the profession of psychiatry undertaken with the Russell Sage Foundation.

² T. H. Marshall, "The Recent History of Professionalism in Relation to Social Structure and Social Policy," *Canadian Journal of Economics and Political Science*, V (1939), 338.

tervention. The psychosomatic "cure" is widely hailed by psychiatrists.

Some physicians consider the non-neurological aspects of psychiatry to be akin to social science, and they esteem them accordingly. It is an added problem for psychiatry that, while it is striving to consolidate its place in medicine, it is also assuming for medicine the role of repository and translator of material from social science. It faces the dilemma of becoming at the same time both more "medical" and more "social." While it is busily justifying itself as a matter of conventional medical concern, it is, at the same time, becoming more special in its interests.

The marginality of psychiatry's place in medicine is demonstrated, too, by the fact that its very place in the medical-training curriculum is a source of conflict. Psychiatry comes as a late development in an already overcrowded schedule: the time it requires must be taken away from other interests. In many places hours sacred to anatomy are being diverted to psychiatry, with ensuing bitterness on the part of the displaced.

Other resistance on the part of medical-school staff members stems from the lack of understanding of psychiatry, from the precedence each man accords to his own work, and from the threat posed by psychiatry.³ Antagonism is often associated with age, and the responses of students may be quite different from those of the staff. New and disturbing material, drastically altering the outlines of conventional medicine, may threaten the systems of prestige and authority; criteria of competence may be altered. Psychiatry is often accused of having the imperialistic intention of making medicine its adjunct. The occasional psychiatrist has his specialty boards in internal medicine, but few internists are likely to have such certification in psychiatry. Many psychiatrists feel that it is their mission to educate or re-educate other men. The psychiatrists push, and are often pushed back

hard by, other medical staff members. Psychiatrists are pressed for "proof" of the adequacy of their ideas and techniques and are rapidly placed in a defensive position. Students, however, are newly recruited from a public which is growing increasingly aware of psychiatry. They find psychiatry already disseminated throughout their curriculum and are much less likely to be outraged by it, although considerable resistance may also be found among them.

Profound differences in scientific orientation may underlie medicine's resistance to psychiatry. Romano has noted certain cardinal orientations of traditional medicine which have been upset or deflected by psychiatry. One is causality—thinking in terms of unitary cause and effect rather than multiple determinants of behavior. Second is the concern of medicine with parts rather than with wholes. A third point is medicine's concern with what and how rather than why. And, finally, medicine has distrusted the validity of subjective data.⁴ These differences have contributed to the "outsider" status of psychiatry in medicine. Psychiatry "wants in," but medicine's acceptance is grudging.

The historical background of psychiatry is also involved in its marginal status. It has been said that psychiatry, born out of jails and almshouses, had a more ignoble birth than other branches of medicine.⁵ Although perhaps the first specialty to develop out of medicine, the field soon became lost to scientific medicine in the morass of the concerns of its custodial institutions. Only recently in its development did it rejoin the movement of scientific and therapeutic medicine, and the psychiatrist may still remain somewhat isolated in his special concerns. The retarding weight of its history has been noted by Romano: "In the past it has been concerned with one small segment of its operational field, namely, the care of the chronically ill, which for the most part has

⁴ *Epidemiology of Mental Disorder* (New York: Milbank Memorial Fund, 1950), p. 125.

³ Report of the Group for the Advancement of Psychiatry, *Report on Medical Education*, No. 3 (March, 1948), p. 6.

⁵ C. C. Burlingame, "Psychiatric Sense and Non-sense," *Journal of the American Medical Association*, CXXXIII (April 5, 1947), 971.

been distant and foreign to the central theme of medicine."⁶

The marginality of the psychiatrist may also be experienced in general hospitals where he is likely, as a widely ranging consultant, to be an outsider on another physician's or service's, ward. Also, his frequent insistence upon the individualized treatment of his patients may bring him into sharp conflict with nursing and other agents of ward administration. Similarly, he may find difficulty in having his instructions understood and executed on a ward where his special language is not spoken.

In general, the colleague relationships of psychiatrists with other physicians highlight the specialty's marginal status. The problems concerning medical referrals of patients to psychiatrists indicate hostility or ignorance. Kubie has stated it "as a basic fact [that] the ordinary physician is as untrained in psychotherapy as is the ordinary layman."⁷ In an interview, a psychiatrist reported that in his experience "the average doctors are quite hostile to psychiatry and many of their patients almost have to force themselves into the hands of a psychiatrist—their doctors won't send them." Another claimed that most of his patients came for psychotherapy on their own initiative but that they remained in the care of their family physician for medical treatment. If that physician were hostile to psychotherapy, the patient would be caught in a bad situation. One psychiatrist reported a patient as having been an "absolute football" between himself and the referring physician. Another indicated that referred cases were often hard to handle. The patients had, in his view, already been mishandled and were afraid of and punished by the threat of psychiatry. He cited, as an important problem, the general physician's misunderstanding of the nature of those mental and emotional disorders

which were accompanied by physical symptoms. Doctors, he said, might treat such patients a long time before realizing that they were emotionally ill. In the experience of some psychiatrists, referred patients had been "deceived" by being sent for neurological examinations, often following the administration of "nerve tonics." It was reported that referred patients had often been discouraged by their physicians' deprecating or pessimistic remarks concerning psychiatry. Some doctors often asked their patients to report to them about the psychiatrist and his work, which has been called "trespassing on the therapeutic situation."⁸

Matters of secrecy and confidence often intrude between the psychiatrist and his medical colleagues. In fact, they may even tend to isolate psychiatrists from their fellows. Psychoanalysis, in particular, has been called "this peculiarly confidential field of medical practice," and it has been noted that, more than in any other branch of medicine, psychotherapy has to be conducted in privacy,⁹ a fact which may come between the psychiatrist and referring colleagues.

The "outsider" status of psychiatry in medicine has given the field an air of defensiveness. It is and it behaves like a minority group, being sensitive to attack, constantly analyzing the sources of hostility, and preoccupied with "selling" itself. Psychiatry is the butt of medical jokes, and its special language is a source of innocent merriment to other physicians. It has been described as a rejected sibling in the medical specialties which has developed habits of self-defense and constantly attempts to prove its legitimacy.¹⁰ Psychiatry responds by defensively comparing its "cures" with those of other medical specialties, by hailing

⁶ John Romano, "Basic Orientation and Education of the Medical Student," *Journal of the American Medical Association*, CXLIII (June 3, 1950), 411.

⁷ Lawrence S. Kubie, *Practical and Theoretical Aspects of Psychoanalysis* (New York: International Universities Press, 1950), pp. 218-19.

⁸ T. A. Watters, "Certain Pitfalls and Perils of Psychiatric Referral," a paper presented at the 1950 annual meeting of the American Psychiatric Association.

⁹ Kubie, *op. cit.*, pp. 211-12.

¹⁰ "Comment: Education versus Indoctrination in Psychiatric Teaching," *American Journal of Psychiatry*, CVII (February, 1951), 633-34.

new "proofs" of its effectiveness, and by carefully noting new "firsts" for the specialty—appointments, departments, courses, clinics, and so on. In a sense the pioneer psychiatrist in a new medical area has burdens paralleling those of a "race man."

In sum, then, if one looks at psychiatry in terms of professional organization and integration, its marginal status within the medical profession becomes quite clear. This is further documented by the repeated observation that many psychiatrists look for intellectual collegueship not to physicians of other specialties but to anthropologists, psychologists, and sociologists.

However, although the psychiatrist is an embattled physician, he is a physician nonetheless. Because of this, a complex set of factors operates to integrate his specialty within the profession of medicine. Medicine itself supports such integration. Many physicians sincerely believe that the psychiatrist's intrinsic skills in psychotherapy are outside the scope of medicine. But medicine will fight fiercely, and has done so, to prevent their exercise by "laymen" and to guarantee them to psychiatry or to control by psychiatrists. The growing influence of the psychosomatic point of view in medicine is also an integrating circumstance. Certainly, in recent years the hours of teaching it in schools of medicine have expanded greatly. The professional politics of American psychiatry have stressed the need for integration with medicine. There are also rewards of prestige and money—these are high for physicians. The public image is a satisfying one.

Surely, for most psychiatrists the battles entailed by their specialty do not shake their conception of themselves as physicians. There are others, however, for whom the stresses of marginality produce a professional "break point." These are men with an unclear professional self-image or those who reject identification of themselves as physicians. One such psychiatrist said: "My medical training taught me absolutely nothing about psychotherapy. I call myself a psychotherapist rather than a doctor. What I do

has no bearing on medicine." In such rare cases the therapist may never have desired to become a physician but felt forced to survive the medical-training system as the means of securing the most sanctioned background and vantage point for the practice of psychotherapy.

Certainly, the entire medical profession has a common base in early training. Perhaps what is shared by all and denied to the non-physician is some particular kind of "charisma." It is an awful thing and a vital part of the conception of one's self to be trained in responsibility for the life and death of patients, and sharing that training provides a common core of deep emotional identification among physicians that establishes a collegueship, however embattled, which is usually more deeply meaningful than the intellectual relationships of psychiatrists with members of other disciplines. Growing up together professionally provides a continuing base of identification, however deflected by later specializations.

The core of the professional dilemma and the key to this area of professional strain is, perhaps, that, for the psychiatrist, being a physician is a reciprocal role, an inseparable correlative. Only physicians may become psychiatrists. This reciprocity unites psychiatry as a professional substructure to other parts of the medical profession with quite different survival contingencies and often incompatible orientation. Crucial aspects of the reciprocals lack congruence and may be in conflict; nevertheless, there are critical components of a medically shared value system which unite them, and the conflicting strains of differentiation and integration pose the expected problems for the marginal men of medicine. To the question "Who am I?" no clear and simple answer is forthcoming.

As psychiatry becomes more firmly entrenched in the system of medical training, such problems of identification may have to be faced earlier. Until the recent past the problem of confronting psychiatry as an alternative career was left to the graduate physician. Students must now begin to

wrestle earlier in the system of rewards and punishments with the problem of dealing with psychiatry's marginal prestige. The effect upon professional identification of this earlier exposure to a marginal career line will need future observation.

An important focus for further investigation is the response of individual psychiatrists to their medical marginality. The final adaptation to the problem will be subjectively determined by each psychiatrist. Clearly, at the social-psychological level of professional identification of the self there is wide variation in the individual's subjective responses to the situation. Different master-role components, as of doctor or

psychotherapist, are internalized. For the more adequate understanding of such individual motivations and responses, we need to call upon our colleagues in psychiatry itself for further help.

Perhaps this could be a shared frontier of future research in the professions. It is a frontier of the greatest complexity. As one psychiatrist stated in an interview, "It would take me three hundred hours on the couch to tell you why I became a psychiatrist." Yet just such data are needed to understand the seeming paradox of psychiatry's tempestuous collegueship with medicine.

UNIVERSITY OF NORTH CAROLINA

THE DETERMINATION OF LOCAL POWER ELITES¹

ROBERT O. SCHULZE AND LEONARD U. BLUMBERG

ABSTRACT

The commonest techniques for identifying a community's power elites are focused either upon position, delineating elites on the basis of formal status in the local economic or political-civic structures, or upon reputation, determining elites through nominations by "juries" of presumably knowledgeable local informants. Utilizing both techniques, research undertaken in a middle-sized midwestern community indicates that the two approaches produced sharply disparate results. Considering only the technique based on reputation, however, use of variously derived nominating panels did not yield significantly different results.

We have witnessed lately a significant reawakening of interest in American sociologists and political scientists in the study of community power structures and decision-making. Although an earlier impetus was provided by the work of the Lynds² and Mills,³ the fund of relevant research remained meager until the publication of Hunter's study of Regional City in 1953.⁴ Since that date a growing number of empirical studies have appeared.⁵ In fact, sufficient research has now been completed to warrant some initial stocktaking—as evidenced by the recent appearance of Peter Rossi's suggestive summing-up and critique of the relevant literature.⁶ Utilizing findings from two recent studies of a midwestern community,⁷ we propose, in this paper, to take a further second look at two of the central methodological problems which confront students of community power: the techniques for determining com-

munity power elites and some consequences of different operations in doing so.

Most students concerned with the struc-

⁵ See, e.g., Robert E. Agger, "Power Attributions in the Local Community," *Social Forces*, XXXIV (May, 1956), 322-31; Robert E. Agger and Vincent Ostrom, "The Political Structure of a Small Community," *Public Opinion Quarterly*, XX (Spring, 1956), 81-89; George Belknap and Ralph Smuckler, "Political Power Relations in a Mid-west City," *Public Opinion Quarterly*, XX (Spring, 1956), 73-81; A. Alexander Fanelli, "A Typology of Community Leadership Based on Influence within the Leader Subsystem," *Social Forces*, XXXIV (May, 1956), 332-38; John L. Haer, "Social Stratification in Relation to Attitude toward Sources of Power in a Community," *Social Forces*, XXXV (December, 1956), 137-42; Martin Meyerson and Edward C. Banfield, *Politics, Planning, and the Public Interest* (Glencoe: Free Press, 1955); V. J. Parenton and Roland J. Pellegrin, "Social Structure and the Leadership Factor in a Negro Community," *Phylon*, XVII (1956), 74-78; Roland J. Pellegrin and Charles H. Coates, "Absentee-owned Corporations and Community Power Structure," *American Journal of Sociology*, LXI (March, 1956), 413-19; Peter H. Rossi, "Historical Trends in the Politics of an Industrial Community," paper presented at the annual meeting of the American Sociological Society, September, 1956; Roland L. Warren, "Toward a Typology of Extra-community Controls Limiting Local Community Autonomy," *Social Forces*, XXXIV (May, 1956), 338-41.

⁶ Peter H. Rossi, "Community Decision-making," *Administrative Science Quarterly*, I (March, 1957), 415-43.

⁷ Leonard U. Blumberg, "Community Leaders: The Social Bases and Social-psychological Concomitants of Community Power" (microfilmed Ph.D. dissertation, University of Michigan, 1955); Robert O. Schulze, "Economic Dominance and Public Leadership: A Study of the Structure and Process of Power in an Urban Community" (microfilmed Ph.D. dissertation, University of Michigan, 1956).

¹ Revision of a paper read at the annual meeting of the Eastern Sociological Society, April, 1956, in New York City. We wish to thank Morris Janowitz for helpful comments.

² Robert S. Lynd and Helen M. Lynd, *Middletown in Transition* (New York: Harcourt, Brace & Co., 1937), esp. chap. iii.

³ C. Wright Mills, "The Middle Classes in Middle-sized Cities," *American Sociological Review*, XI (October, 1946), 520-29.

⁴ Floyd Hunter, *Community Power Structure* (Chapel Hill: University of North Carolina Press, 1953). For a provocative review of Hunter's research by two unconvinced political scientists see Herbert Kaufman and Victor Jones, "The Mystery of Power," *Public Administration Review*, XIV (Summer, 1954), 205-12.

ture and dynamics of community decision-making have initially assumed that a theoretically fruitful and empirically tenable distinction can be made between the most powerful persons and units in a community and those having lesser degrees of power. To make this assumption is not to deny that power relations in the modern urban community are "unequal," nor is it to argue that community power can necessarily be conceived in the form of either a single or a simple pyramid; neither is it inconsistent with Simmel's long-recognized thesis that dominance is always a two-way street. One can acknowledge that all persons and units in the community exercise certain measures of influence and control without rejecting the proposition that some can mobilize such considerable resources—organizational, economic, psychological—that they have relatively most power over crucial community decisions and actions. Even C. Wright Mills, who in 1951 asserted that in contemporary America the "engineering of consent to authority has moved into the realm of manipulation where the powerful are anonymous,"⁸ has since been moved to write a detailed and stimulating volume which suggests that the powerful may not, in fact, have become so very anonymous and that power relations may not have grown so nebulous, ill-defined, mercurial, and diffuse that they cannot be charted at all.⁹ So we begin by assuming that communities and societies contain power elites which are somehow delineable.

The next and crucial question is this: How do we proceed to determine "the most powerful and influential" in American communities?¹⁰ In general, sociologists

and political scientists have employed one or the other of two techniques: one based on position and the other on reputation.

The method based on position involves selecting certain persons as most powerful and influential on the basis of their official status in the community's institutionalized economic, political, and/or civic structures. Thus both Lynd and Mills—following Marx—have contended that crucial power decisions are the province of individuals holding the top positions in the major industrial, credit, and business units in the community. And although Stouffer utilized top political and civic (rather than economic) status, he likewise employed this in identifying community elites.¹¹

A more prevalent technique has been to allow certain members of the community under study to do the determining. Based on local reputation and derived from theoretical formulations of Weber and Lasswell, with a debt to Warner, this method has been used most notably by Hunter¹² and Angell.¹³ However, most students who have relied on local nominations have made use of a technique based on position as an intervening step. Thus the persons whose perceptions of community influence were utilized by both Hunter and Angell did not consist of randomly selected cross-sections of the local populations; they were, rather, individuals presumed to be knowledgeable because of their formal local social positions.

In this paper we ask the following questions:

1. To what extent do the methods based on reputation and on position yield similar or compatible answers to the question Who are the most powerful and influential in the community?

2. Considering only the approach based

⁸ C. Wright Mills, *White Collar* (New York: Oxford University Press, 1951), p. 110.

⁹ C. Wright Mills, *The Power Elite* (New York: Oxford University Press, 1956), esp. chaps. ii and iii.

¹⁰ There are certain possibly significant conceptual differences between the terms "power" and "influence," which, however, have been of much greater concern to theoreticians than to those doing actual field research. For present purposes, therefore, we shall use the concepts as roughly synonymous (cf. Agger, *op. cit.*, p. 323).

¹¹ Samuel A. Stouffer, *Communism, Conformity, and Civil Liberties* (Garden City: Doubleday, 1955).

¹² Hunter, *op. cit.*

¹³ Robert Cooley Angell, "The Moral Integration of American Cities," *American Journal of Sociology*, Vol. LXI (July, 1951), Part 2.

on reputation, what is the effect of using different panels of presumably knowledgeable persons?

The subject community—which we shall call Cibola—is a midwestern industrial city of some 20,000 inhabitants, located approximately 30 miles from one of the largest metropolitan centers in the United States. Over the past several decades and especially since World War II, Cibola has become increasingly involved in the industrial and social complex of this giant neighbor, Metro City.¹⁴

In attempting to determine Cibola's power elite(s), we used both techniques. With regard to that centered on reputation, we initially designated as our "nominating panel" the formal heads of the local (white) voluntary associations.¹⁵ They were selected on the assumption that they were most representative of the broad, "grass-roots" base of local organized power. Each was asked five questions.¹⁶ Although a total of 271 persons was named by at least one association head in response to at least one of the "perception of influence" questions, the 18 persons most frequently named accounted for the majority of all nominations. Accordingly, these 18 persons were designated as the public leaders of Cibola.

How did the composition of the public leadership category, as thus defined by reputation, compare with that of community elites determined on the basis of their positions in the economic or the political and civic organizations in Cibola?

Persons occupying the top formal status in the major local industrial and credit units were designated as the "economic dominants." They included the heads of all industries employing 75 or more workers, the

heads of all banks with total assets in excess of one million dollars, and, in addition, persons who were members of the boards of directors of two or more of these industries and/or banks and who thus served in the formal "interlocking" of the dominant economic units. By this definition, a total of 17 persons was named as the economic dominants of Cibola.

There was almost no overlap between the public leaders and the economic dominants; specifically, the 17 economic dominants included but 2 of the 18 public leaders in the community.

A second method of employing criteria of position involves selecting persons filling the top political and civic status in the community. In his study, Stouffer selected as civic leaders fourteen objectively defined public statuses: mayor, president of the

¹⁶ These questions were:

1. "Suppose a major project were before the community, one that required decision by a group of leaders whom nearly everyone would accept. Which people would you choose to make up this group—regardless of whether or not you know them personally?"

2. "In most cities certain persons are said to be influential 'behind the scenes' and to have a lot to say about programs that are planned, projects, and issues that come up around town. What persons in [Cibola] are influential in this way?"

3. "If a decision were to be made in [the state capital] that affected [Cibola], who would be the best contact man to get in touch with state officials [besides local members of the legislature]?"

4. "Who [besides local members of Congress] would be the best people to get in touch with federal officials in [Metro City], Chicago, or Washington?"

5. "Are there any other people whom these leaders work with and who have not been named so far, but who should be included in a list of community leaders?" Since our object was to delineate a *general* category of community leadership, the individual responses to the five questions were combined in the final tabulation.

Previously, the association heads' responses had been totaled separately. It was found that all but 1 of the 18 persons subsequently defined as public leaders ranked among the top 19 in *both* the "major-project" and the "behind-the-scenes" questions. It was further noted that eliminating nominations in response to the remaining three questions changed somewhat the total scores and the rank order of the 18 most frequently named persons but had almost no effect on the over-all composition of the top 18.

¹⁴ Cibola is not encompassed, however, within the Standard Metropolitan Area of Metro City.

¹⁵ Eighty per cent of the heads of the 180 voluntary associations in the community were interviewed. None of the association heads whom we were unable to locate and interview was perceived as influential by any of the 143 interviewed, nor were their organizations regarded as influential.

chamber of commerce, chairman of the Community Chest, president of the largest labor union, county chairmen of the Republican and Democratic parties, commander of the largest American Legion post, regent of the DAR, president of the women's club, chairmen of the library and the school boards, the parent-teachers' association, the bar association, and the publisher of the largest locally owned newspaper. Applying this definition of civic leaders in Cibola, *only 4* of the 18 public leaders were found to occupy any of the fourteen top civic statuses in Stouffer's list. Various other objective criteria for selecting the political-civic elite of Cibola were considered (e.g., selecting the members of the city council and the school board and the top lay officers in the largest churches and in the businessmen's luncheon clubs), and the results were compared with the leaders as defined on the basis of reputation. In no instance did the persons categorized as top political-civic leaders by any of these definitions include more than 4 of the 18 public leaders.

We may say, therefore, that the heads of voluntary associations definitely make a distinction between those persons who occupied the top formal political and civic offices and those who, in their opinion, wielded the most influence and exerted the greatest public leadership in the community.¹⁷ The fact that the association heads selected so few of the nominal leaders of Cibola as the "real" influential public leaders attests to a considerable degree of sophistication on their part.

On the other hand, that the heads of voluntary associations perceived so few of Cibola's economic dominants as community influentials may be due to the considerable social distance which perhaps separated most of them from the "real" centers of local power and influence. Our research de-

sign made it possible to test this possibility, for we asked the public leaders and the economic dominants the same "perception of influence" questions which we had previously addressed to the heads of associations and on the basis of which we had made our initial designation of public leaders. The over-all selections of the three panels—the heads of voluntary associations, the public leaders, and the economic dominants—are compared in Table 1.

While it is apparent that a nice similarity in rank orders did not obtain, Table 1 nonetheless reveals a high degree of consensus among all three categories interviewed as to the over-all composition of the local elite of power and influence. Thus the public leaders themselves specified 72 per cent (13 out of 18) of those defined as public leaders on the basis of nomination by the heads of associations. Likewise, the top group in influence as perceived by the economic dominants included 72 per cent of the men already categorized as public leaders. Only 3 of the 18 classified as public leaders failed to be included in the group selected by either the economic dominants or the public leaders.

Even more significant was the degree of agreement among the three panels with regard to the uppermost range of community power: of the 10 persons most frequently considered influential by the public leaders, 9 had, in fact, been operationally defined as public leaders; likewise, of the 10 most frequently regarded as influential by the economic dominants, 9 had been categorized as public leaders. Furthermore, 7 persons (Houston, Drew, Milan, Moseley III, Rush, Ford, and Chapman) are included among the top 9 in all three lists. And, finally, it is apparent that none of the panels discerned those of dominant economic status as an appreciable segment of public leadership. Like the heads of associations, the public leaders themselves included but two economic dominants in the top group, while the economic dominants themselves regarded an only somewhat greater number of their fellow dominants

¹⁷ Cf. F. A. Stewart, "A Sociometric Study of Influence in Southtown," *Sociometry*, X (1947), 11-31, 273-86. Although his research design was somewhat dissimilar, Stewart's findings were consonant with those reported here.

TABLE 1

RANK ORDERS OF PUBLIC LEADERS AS PERCEIVED BY ASSOCIATION HEADS,
PUBLIC LEADERS, AND ECONOMIC DOMINANTS*

Association Heads' Selections	Public Leaders' Selections	Economic Dominants' Selections
1. HOUSTON	1. HOUSTON	1. HOUSTON
2. DREW	2. FORME	2. <u>MOSELEY III</u>
3. MILAN	3. <u>MOSELEY III</u>	3. FORD
4. <u>MOSELEY III</u>	4. FORD	4. DREW
5. RUSH	5. DREW	5.5. RUSH
6. FORME	6. RUSH	5.5. PETERS
7. FORD	7. CHAPMAN	7.5. CHAPMAN
8. JOHNS	8.5. MILAN	7.5. MILAN
9. CHAPMAN	8.5. Thomas	9.5. TAYLOR
10. SCHLAFF	10. TAYLOR	9.5. <u>Staley</u>
11. BERNARD	11. PETERS	11. Fielding, Sr.
12. WARNE	12. JOHNS	12. <u>ROGERS</u>
13. TAYLOR	13. <u>Staley</u>	12. FORME
14. MOSELEY II	14. HAAS	12. <u>Fischer</u>
15. PETERS	17. WARNE	15. MOSELEY II
16. GUYER	17. Lee	16. WARNE
17. HAAS	17. Smith	17.5. Kingsley
18. <u>ROGERS</u>	17. Arthur	17.5. Lake
	17. Lake	

* These rank orderings are based on total frequency of "nominations" by the 143 association heads, the 18 public leaders, and the 17 economic dominants, respectively, in answer to the five "perception of influence" questions. Those defined as public leaders (the top 18 in the association heads' list) are indicated in CAPITALS; those defined as economic dominants are underlined. The public leaders' positions in the rank orders are connected by solid lines; the positions of others are connected by broken lines.

as among Cibola's most influential—4 out of the top 18, or 22 per cent.¹⁸

These data show that, despite the fact that the heads of voluntary associations, the public leaders, and the economic dominants occupied different positions and played dissimilar roles in the local social structure, each category perceived substantially the same set of persons as most influential in the affairs of the community.

A question remains: If Cibola's most influential by reputation—its public leaders—were neither the community's economic dominants nor the current occupants of either its top political or civic offices, where did they fit into the local social structure? All were white males. Their median age was fifty-three; their median number of years' residence in Cibola was thirty.¹⁹ In occupation they represented only the business and professional classes in the city, albeit a fairly wide range of positions within these broad categories.²⁰ In general, it may be said that the public leaders were drawn almost wholly from the old middle-class segment. Only one, to be sure, was an official in any of the "big-business" firms. (And he resigned and opened an insurance office in partnership with a local politician during the course of our research.)

It cannot be assumed, however, that the public leaders constituted a representative

cross-section of Cibola's small-business and professional men. They were, rather, persons intimately involved in the community's voluntary associations who, in earlier years, had occupied responsible positions in local civic and political units. For example, the median number of memberships currently held by the public leaders in voluntary associations was 9 (compared with a median of three for the economic dominants); each public leader was a member of at least one of the three most prominent civic luncheon clubs (Rotary, Kiwanis, Lions), and 14 were members of the Chamber of Commerce. Furthermore, 89 per cent of the public leaders (16 out of 18) had, at one time, served as president of at least one local voluntary association, and 61 per cent (11 out of 18) as president of at least one of the five associations regarded by all three basic categories of power figures—the association heads, the economic dominants, and the public leaders themselves—as most influential in the community. Finally, while not more than one of the public leaders could have been regarded as a professional politician,²¹ fully 89 per cent (all but 2) had occupied elective or appointive office in at least one of the local units of government: For example, 3 had served as mayor, 6 as city councilmen, 3 as school-board members, and 9 as appointive members of one or more of the several municipal boards and commissions.

We find, therefore, that, while a considerable majority of the local public leaders were businessmen, they were not the top businessmen, the economic dominants. Likewise, while almost all had held responsible civic and political positions in Cibola, they were not, with minor exceptions, among the community's formal civic and political leaders at the time of our study.

²¹ One individual, although he had lately served as chairman of the state central committee of the Republican party and as an elective state official, indicated that his position as owner-president of a small local business college was his usual occupation and the one from which he derived most of his income. Occupationally, therefore, he was listed in n. 20 as an educator.

¹⁸ Of the few economic dominants regarded as top influentials by *any* of the three panels, *none* represented the five largest industries (all absentee-owned) or the two largest banks in the community.

¹⁹ Although they tended, thus, to be middle-aged men of fairly lengthy local residence, only 28 per cent of the public leaders (5 out of 18) were native-born Cibolans.

²⁰ Specifically, they included 3 industrial executives (only 1 of whom, however, occupied a superordinate position in his firm and could therefore be called an economic dominant); 2 merchants (one of whom, also on the boards of directors of two local banks, was the second and final economic dominant in the public leader ranks); 2 educators; 2 professional Chamber of Commerce officials; 1 (subordinate) bank official; 1 wholesaler; 1 realtor-insurance broker; 1 salesman; 1 attorney; 1 clergyman; and 1 retired person (formerly the owner of one of the relatively small, locally owned manufacturing plants).

Considering what we know about the power structures of other middle-sized American communities, it seems apparent that Cibola's public leaders were of the age, sex, race, length of residence, business and civic experience, and connections appropriate to community influence and leadership. And it became abundantly apparent, subsequently, that the public leaders did in fact constitute a closely knit friendship group which exercised substantial—if not always decisive—control over the community's decisions. Yet this important category of elites could not have been revealed had we relied solely on one of the usual methods based on position.

The two questions raised earlier in this paper may now be answered as follows:

The composition of the community's power elite, as defined by reputation, differs significantly from that defined on the basis of superordinate positions in *either* the local economic *or* the political-civic institutions.

However, the use of different panels of persons who may be assumed to be reasonably (although not similarly) knowledgeable does not produce significantly different results.

Generalizations based on the study of

but a single community, as this is, are obviously provisional. Nevertheless, they strongly suggest the advisability of studying a community's power structures from at least two methodological perspectives—that based on position and that on reputation. It is not a question of whether one or the other is "right." Rather, by using both and by determining the nature and degree of similarity between the two resulting lists, valuable leads are found as to the structure and dynamics of local power. In Cibola, for example, the marked disparity between the categories of public leader and of economic dominant suggested—and further research confirmed²²—a widespread and growing reluctance on the part of the economic dominants to become involved in the initiation and determination of local political decisions. And this, in turn, raised the larger question of the changing role of major economic units—especially absentee-owned corporations—in the local power structures of American communities.

BROWN UNIVERSITY
TEMPLE UNIVERSITY

²² Robert O. Schulze, "The Role of Economic Dominants in the Community Power Structure," a paper read at the 1957 annual meeting of the American Sociological Society in Washington, D.C.

INTERDISCIPLINARY COLLABORATION IN MENTAL ILLNESS RESEARCH¹

OZZIE G. SIMMONS AND JAMES A. DAVIS

ABSTRACT

In undertaking interdisciplinary research, a team of behavioral scientists anticipated that the principal problems of collaboration would be due to their conceptual differences. However, methodological differences, here characterized in terms of "clinical" and "quantitative" points of view, turned out to be the major barriers to collaborative effort and generated issues regarding such operations as choice of research problem, sampling procedures, kinds of data, and techniques for collecting them. Other problems arose due to the adjustments in role required by team research. The conceptual framework and methodological strategy in process of development by the team seeks to take systematic account of these differences.

A number of recent papers have been concerned with the problems of interdisciplinary collaboration of teams including behavioral scientists and psychiatrists.² The present paper chronicles initial phases of the development of interdisciplinary collaboration by a group of behavioral scientists completing their second year of team research.

¹ Revised version of a paper read at the 1956 annual meeting of the American Sociological Society in Detroit. The research reported on here is being undertaken by the Community Health Project of the Harvard School of Public Health, under the direction of Ozzie G. Simmons, and is supported by special grants from the National Institutes of Mental Health. In addition to the authors, the team consists of Katherine Spencer, Stanley H. Cath, Howard E. Freeman, Leota L. Janke, Herbert Naboisek, and Dorothy Mathews, whose suggestions in the preparation of this paper are gratefully acknowledged. We also wish to thank Gerald Caplan, William Caudill, Clyde Kluckhohn, Ralph Notman, Benjamin Paul, John Spiegel, and Alfred Stanton for reading the original manuscript.

² Publications most relevant to the present discussion are Urie Bronfenbrenner and Edward C. Devereux, "Interdisciplinary Planning for Team Research on Constructive Community Behavior," *Human Relations*, V (May, 1952), 187-203; William Caudill and Bertram Roberts, "Pitfalls in the Organization of Interdisciplinary Research," *Human Organization*, X (Winter, 1951), 12-15; Joseph W. Eaton, "Social Processes of Professional Teamwork," *American Sociological Review*, XVI (October, 1951), 707-13; and Frederick C. Redlich and Eugene B. Brody, "Emotional Problems of Interdisciplinary Research in Psychiatry," *Psychiatry*, XVIII (August, 1955), 233-39.

The project was begun three years ago with a general mandate from the funding agency "to develop a framework for systematic research in the community aspects of psychiatric rehabilitation." The original staff consisted of a sociologist and an anthropologist. During the first year these two defined an area of research cutting across disciplinary and conceptual lines in the social sciences and decided to include representatives from several other disciplines. The present team consists of two sociologists, an anthropologist, a psychiatrist, two clinical psychologists, and a social worker.

The team members expected to differ in their conceptual orientation, and accordingly they felt a need for sessions on orientation. At the same time, the realities of scheduling, as well as the paucity of systematic data on the posthospital experience of mental patients, made it desirable to collect some data to serve as a common base for erecting an interdisciplinary framework. The field experience almost immediately raised a series of problems requiring decisions about research operations—problems which brought to light a variety of conceptual and methodological differences.

As it turned out, the major barriers to collaborative effort emerged in the methodological rather than the conceptual sphere. Our original assumption was that different conceptual and theoretical backgrounds would make for difficulties in interdisciplinary research; it seemed reasonable that

sociologists, anthropologists, clinical psychologists, and psychiatrists would often disagree on concepts and theories. There were, of course, difficulties in communication inherent in the fact that different disciplines use the same term to refer to different things. Nevertheless, our impression is that purely conceptual disagreements were minimal, probably because the theories of the various disciplines are not contradictory in the sense that one says "X is true" and another says "X is false." Theoretical formulations among the behavioral sciences are more often statements like "X is important," which is not to deny that Y and Z are also important. Thus, there is no inherent contradiction in saying that a patient's personality, biology, kinship and other roles, and social class are important to his behavior. This does not mean that all team members gave wholehearted indorsement to every concept discussed but that few cared to veto those introduced into the research by others of the team. Moreover, conceptual differences were seen as the rationale for bringing together an interdisciplinary team.

The preliminary field studies led us to adopt a conceptual model that focuses research on the networks of interpersonal relations in which patient, family, and hospital personnel are the principal actors. The networks are examined within the contexts of social structure, culture, and personality. The model views the released patient as located in the center of a web of personal relationships, the most strategic of which are those with the hospital and his immediate family. As such, the model still falls short of constituting a unified interdisciplinary framework, which, indeed, remains a major goal of the behavioral sciences. Nevertheless, by its emphasis on the concepts of role and interaction process and on situational analysis for their investigation, the model requires application of knowledge from all the disciplines represented on the team.

METHODOLOGICAL PROBLEMS

During the initial period of field work, the major divergences often *seemed* to be

conceptual or theoretical, but the basic differences were methodological, not conceptual. For example, one staff member would propose that a certain psychological type of patient be excluded from a sample design; another would express reservations. Although the discussion would seem to center around the psychological concept, it would later become apparent that the reason for the disagreement was in different conceptions of the ends and means of sampling itself. While one person saw sampling as a means of getting a representative portion of some universe, another saw it as a way of getting cases which would yield crucial insights. Again, a staff member would propose the use of a certain attitude scale. Discussion would center on the concept "attitude" as an analytic tool, but it would turn out that the crux of the problem was in differential acceptance of questionnaire material as research data.

Many of the barriers to communication and consensus can be ascribed to opposing points of view that may conveniently be identified as "clinical" and "quantitative." We are not concerned here with favoring one of these as opposed to the other or with systematically defining the clinical and quantitative methods but rather with describing polar types of contrasting views and the consequences of their polarity. Some of the principal notions that characterize each of these idealized points of view can be summarized as follows:

CLINICAL

1. Principal emphasis is on the configurational, and a given segment of behavior is meaningful only as it can be related to a total context. Thus the focus is on the "whole person," and inferences are made primarily from data on the individual case. This orientation is usually associated with a strong preference for a phenomenological or natural history approach.

2. The method of analysis and combination of data substantially involves individual judgment, which always depends in some degree on the knowledge and skill of the investigator. Documentation is evaluated according to standards not explicitly formulated.

3. Major interest is "understanding" the full

complexity of the phenomenon under investigation; specification of the operations is subsidiary.

QUANTITATIVE

1. Principal emphasis is on the relational and comparative, and a given segment of behavior is meaningful if it can be operationally specified and measured in relation to other such elements. Thus the focus is on given dimensions of persons, and inferences are made primarily from (logical) data on class membership. This orientation is usually associated with a strong preference for a controlled or experimental approach.

2. The method of analysis and combination of data substantially involves direct recourse to formal devices, such as equations or tables, which eliminate individual judgments. Documentation is evaluated according to certain formal rules of evidence.

3. Major interest is selecting variables for research that can be "measured," specified, and replicated.

No member of our research group has adhered to the purely clinical or quantitative point of view as described here. Depending on the particular issue, a staff member may have taken a clinically oriented stand on one occasion and a quantitatively oriented stand on another. Where an individual stands has depended not only on his disciplinary orientation but also on his research experience, his knowledge of the other disciplines, and his personal predilections.

Although differences of a conceptual order were anticipated, these conflicting methodological viewpoints were not. Moreover, they often seemed to trigger highly emotional reactions. Individual members made some attempt to understand their colleagues' point of view, but the more pronounced tendency was to retreat into reaffirming one's own point of view, often at the expense of others', and to press for formulation of research operations in ways that would place the greatest premium on the approach most favored.

The situation probably had much to do with evoking these reactions: it was ambiguous, uncertain, and strange. We were working in the field of mental illness, where the basic dimensions are still unknown, and in a sector of the field (community rehabili-

tation) almost completely uncharted for research. We had not yet delimited the areas we wished to focus upon or arrived at a common focus for the research. Moreover, we were confronted with the uneasy task of establishing long-term relationships with informants who were, at best, recovering from a psychotic episode and, at worst, actively psychotic. (A large proportion of our patient informants turned out to be the latter.) Most staff members responded to this threatening situation by falling back on what they knew best, partly to ward off still more unknown procedures.

The ways in which methodological differences affected the team can be illustrated by analyzing issues concerning such research operations as choice of problem, sampling procedures, kinds of data to be collected, and techniques for collecting them.

Clinician and quantifier agreed that the principal objective at the outset was to identify the significant factors in the post-hospital situation. However, once the group went beyond this general statement, it became apparent that there were substantial differences as to what constituted appropriate problems for research as well as the objectives to which the research should be directed. The quantifier was primarily concerned with contributing to systematic knowledge, leaving for others the task of its application, while the clinician, whose previous professional activity had included practice as well as research, was concerned with collecting knowledge directly applicable to problems of treatment and programming.

Although the clinicians, now engaged in scientific endeavor, clearly had no intention of directly helping the patients, they tended to retain elements of their "service" orientation. Thus, their principal concern in formulating research problems was the practical use of the findings. Opposed to this was the quantifiers' orientation to "exact science" and their insistence on rigorous and precise procedures. They felt that selection of problems primarily in terms of their relevance to immediate therapeutic needs would reduce the research to *ad hoc*

trouble-shooting, while the clinicians felt that selection of problems primarily in terms of their amenability to rigorous scientific method would eventuate in research that was neither interesting nor relevant. Moreover, the clinicians were concerned with how practitioners would react to the project, while the quantifiers were more concerned with the impression to be made upon professional research workers.

Sampling procedure was a second source of conflict. In our first field study we undertook to investigate posthospital experience through interviews with patients and their significant associates on a long-term basis after release from the hospital. Since the aim of the longitudinal case-study program was primarily to explore and to arrive at hypotheses, we were not urgently concerned with obtaining a random sample of cases for the purpose of estimating parameters. We set out to take forty consecutive patients to be released from two mental hospitals, all with a formal diagnosis of schizophrenia, to be stratified according to age, sex, and hospital-admission status (first versus multiple admissions).

Quantifiers objected to any criteria that could not be formalized and applied equally to all the cases. Consequently, they were disconcerted when the clinicians proposed additional criteria to be applied only by the use of clinical judgment. Thus the quantifiers opposed attempts to exclude patients who met the standardized criteria because they were still "too psychotic" or because lifelong consistently low levels of "psycho-social functioning" made them "non-rehabilitable," or to include patients who did not meet the standardized criteria because they were "striking instances" or "clinically interesting." The clinicians' relative indifference to specification of sampling criteria can be attributed in part to their notions that generalizations about logical classes had limited value so long as exceptions could be found, whereas "insights" and "discoveries" to be obtained from the study of individual cases were in the long run more fruitful and "valid." Both clinicians and

quantifiers were interested in generalization, but the clinician for the individual case, the quantifier for the group.

Quantifier and clinician apparently approach sampling from temperamentally different points of view. The quantifier is exceedingly pessimistic, has no hope that a good sampling scheme will provide any positive help, but knows all sorts of folklore about studies that were jeopardized because people forgot that corner houses have high assessments, that foreign names are not distributed randomly in the alphabet, and that months contain different numbers of Sundays. Thus the most he can look forward to by manipulation of his statistical rituals is the hope of avoiding disaster. The clinician, however, is more sanguine. To him, sampling is an opportunity to get information, and, the more interesting the information, the better the sampling. Logically, therefore, the way to improve sampling is to increase the number of fruitful cases and to decrease the unfruitful ones. That such procedures affect, among other things, factorial designs and the probability that all arrangements have an equal chance of appearing in the sample seems to him less important than the fact that they might yield "answers."

Quantifiers tended to focus on manifest behavior and more on what was said or done than on how it was said or done. Their ways of collecting data were geared largely to what informants were ready to furnish. The clinicians, on the other hand, tended to focus on the deeper meanings of behavior rather than the behavior itself and to accept little at face value. The quantifier relied on the "facts" he collected; the clinician, on inferences about feelings and affect. As a result, the quantifiers thought that clinicians ignored the data, while the clinicians felt that the quantifiers were naïve in assuming they could get "meaningful" material by operating on the "raw" data.

Consistent with their focus on depth materials, the clinicians favored unstructured research techniques and deprecated questionnaires, rating scales, and other formal

instruments. At this early stage of research, however, all were agreed that primary reliance had to be placed on the interview as the technique for gathering data. This was one skill that all members felt they possessed when they came to the project, yet conflicts arose as to how the interview ought to be conducted and what it was to elicit. In keeping with our exploratory orientation, we aimed initially at "free" or "non-directive" interviewing and were disposed to rely on very broad formulations of relevant content to obtain comparable and adequate coverage by field workers. The clinicians, focused on dynamics, were willing to continue with this type of interview indefinitely. The quantifiers, however, focused on content and eventually began to push for greater standardization in interviewing: for measures for insuring comparability of data, maximizing inter-interviewer reliability, and facilitating subsequent replication. The clinicians feared that standardization of the interview would obscure the possibilities of getting at dynamics and symbolic meanings by reducing the range and variation of responses of both interviewer and informant and, accordingly, resisted the quantifiers' efforts at standardization.

To facilitate collaboration, we had from the beginning accorded equivalent research roles in the longitudinal case studies to all members of the team in planning, collection of data (e.g., concentrating on the same content areas and using roughly the same type of interview), and analysis and interpretation of data. In one sense, this policy insured the emergence of common problems, since a disciplinary division of labor might have permitted each member to probe the area he knew best and to settle down to purely nominal collaboration with his colleagues. However, the members did not share cases, but each was responsible for a group of cases. Thus, preoccupation with comparability across cases remained high, and each team member retained a strong investment in ultimate resolution of our methodological conflicts. Although differ-

ences remain, many conflicts have been partially resolved by the development of a methodological rationale for the project that seeks to take them into account. While we have stressed the differences between the clinical and the quantitative points of view in order to illustrate how they infiltrate into problems which do not seem methodological, it is clear that the picture we have given is of "ideal types." Just as no one person approximates either pole, it is our belief that no one interdisciplinary project can or should do so.

Our present strategy attempts to combine the rich detail and depth of clinical analysis with the relatively greater precision of quantitative research. Special empirical studies, using separate samples and special types of data analysis, are designed to test ideas generated by the longitudinal case studies. This is seen as the basic pattern for future research, which is to include a study of the process of release from a mental hospital.³ Detailed analysis of patterns of release in a few of our longitudinal cases yielded the hypothesis that considerable strain is imposed on the relations of three "actors"—hospital, patient, and family—during the process of release and that the strain is structured according to power differentials among the three. This hypothesis was tested and documented in a special study by quantitative analysis of data collected on a sample of eighty-five cases.

Our explication of procedures has helped allay fears of clinicians and quantifiers that each wanted to push out the other and made for increasing recognition that the two approaches are complementary and necessary for the kinds of research in which we are engaged. Still, the differing points of view are tenaciously held, and they persist as barriers to communication and consensus which will have to be overcome as they arise in continuous attempts at collaboration.

³ Ozzie G. Simmons, James A. Davis, and Katherine Spencer, "Interpersonal Strains in Release from a Mental Hospital," *Social Problems*, IV (July, 1956), 21-28.

SITUATIONAL PROBLEMS

Another order of problem was that of role readjustments inherent in the transition from individual to team research. Few of the members of our group had previously participated to any considerable extent in team research. We were not prepared for the high frequency and intensity of interaction with colleagues in which we were immersed as a consequence of teamwork. Whether the member had come from an academic or a therapeutic milieu, he had spent a considerable amount of his professional time alone or with students or patients. To some extent, he had been able to govern his contacts with colleagues according to his propensity, with formal interaction restricted to the occasional departmental meeting or clinical staff conference. As a member of the team, the nature and amount of his professional interaction was largely set by the group and almost wholly limited to it. Depending on personal inclination, some members were better able to survive this than others. Moreover, this "submarine effect" was enhanced by continuous preoccupation with the one subject of posthospitalized schizophrenics.

A concomitant of this high and restricted interaction was the constant mutual exposure that team members were subjected to with regard to their work. Almost everything the individual team member was called upon to prepare was subjected to scrutiny by his colleagues. With respect to field work, each interviewer held regular consultations with the staff psychiatrist to assess his relationship with particular informants and with the project director to assess research needs. Then there were weekly staff meetings, where particular cases were discussed for training purposes and where analyses of the role of the interviewer in the development of the case sometimes became too candid for comfort.

Intellectual stimulation and opportunities to clarify ideas are advantages to be derived from teamwork and were so regarded by the participants, but they are gained at a

cost one does not ordinarily have to pay in individual research. Members of the team were always under the strain of being unable to take anything for granted, of having to spell out and justify their contributions and interpretations. They often had to fight in defense of a proposal whose "reasonableness" or "rightness" seemed self-evident to them. A certain amount of this can be exhilarating and constructive, but, when it takes the form of sustained and unremitting pressures, it can wear one down. Another cost was that of submitting to the group tempo, inevitably slower than that of any individual, in arriving at major decisions, since consensus was sought for all decisions directly affecting individual team members. The process often seemed tortuous and laborious, requiring patience, tolerance, and faith that the end would justify the sacrifice.

Another set of strains was imposed by the need for co-ordination of the efforts of staff members, as in uniform systems of recording and coding of field data. To meet the need, a variety of standardized procedures had to be complied with by the members, and various forms had to be completed on a regular basis. Almost without exception, they regarded the tasks as dull and irksome, and occasionally either showed resistance by satirizing them or by just "forgetting" to comply. Perhaps the greatest discomfort in this connection was experienced in learning to use a category system, developed along the lines of the Yale Cross-Cultural Survey, for coding of manifest content data collected in the case studies.

Finally, we may mention the members' difficulties with deadlines that were seldom self-imposed. Timing and scheduling, in relation to grant-getting activities, did not always accord with the inherent needs of the research itself, and members had, on occasion, to adjust to disconcerting changes in pace in demands on their productivity. Already strained by their participation in the group effort, they suffered a loss in productivity from accustomed individual levels just at the time when the urgency to produce was greater than ever.

Caudill and Roberts recommend that, if the collaborators do not have some knowledge and understanding of each other's point of view before the research begins, "a long period of time should be spent, preferably at the beginning of the project, in straightforward, reciprocal teaching and learning."⁴ It may be that many of our problems could have been averted if we had been able to follow this procedure, but in our case the realities of short-term subsidies and the paucity of systematic knowledge in our field of interest pressed for early field activity. On the other hand, it may be that abstract discussions without a concrete focus, no matter how long carried on, would have

failed to uncover our conflicting points of view or to have provided sufficient incentive to alter them in the interest of group collaboration even if they had come to light. Our experience with field data not only quickly uncovered our basic differences and made us face the need to do something about them but facilitated this task by giving us a common experience and a base of data to which we could relate our efforts at working out a conceptual and methodological framework that has made considerably more realistic the ultimate achieving of true interdisciplinary collaboration.

HARVARD UNIVERSITY
UNIVERSITY OF CHICAGO

⁴ *Op. cit.*

INTERVIEWING THE BUSINESS ELITE¹

HARRY V. KINCAID AND MARGARET BRIGHT

ABSTRACT

Interviewing top-ranking business executives raises several problems of procedure, among which are: enlisting co-operation, preliminary preparation, qualifications of respondents, and multiple respondents. Practical resolution of these and other problems was achieved largely on a trial-and-error basis. The use of two interviewers, or tandem interviewing, eliminates a major source of bias inherent where there is a single interviewer and contributes to the efficiency with which data are gathered and analyzed. Other decisive conditions are the sponsorship and affiliation of the researchers, the practical emphasis given to the subject, and the competence of the interviewers.

Recently the writers engaged in research requiring personal interviews with high-ranking executives in 37 large business organizations, primarily manufacturers.² We found early in planning the work that we had to proceed on a trial-and-error basis, since little of the literature on research procedures was pertinent. Our purpose here is to discuss the procedures we adopted and some problems encountered in obtaining the co-operation of corporations and in conducting the interviews.³

Perhaps the most interesting procedure reported here is "tandem" interviewing,

that is, the use of two interviewers.⁴ By far the majority of interviews in social research are by one interviewer working with one respondent, and this is implicitly assumed in most of the technical literature. As will be discussed later, we, as it were accidentally, developed a system of employing two interviewers in the exploratory phases of the study, and this system was used thereafter.

In the future large corporations with their far-flung operations—divisions, subsidiaries, plants—may, at least temporarily, have to maintain production and distribution without the co-ordinating function normally performed by a headquarters office. Recent developments in fission and fusion weapons present the possibility that the headquarters of industrial or commercial concerns may be either destroyed or cut off from communication with other parts of the organization.⁵ Thus it is important to ask what functions are performed by headquarters personnel and how their loss may affect the units they control.

For the investigation of this problem, corporations were selected on the basis of

¹ This article may be identified as Publication A-197 of the Bureau of Applied Social Research, Columbia University.

² This was part of a larger program of research on the concentration of administrative talent in urban areas carried out at the Bureau of Applied Social Research under contract with the RAND Corporation; Kingsley Davis was principal investigator.

³ In another phase of the project we studied 180 manufacturing companies with assets of over \$50,000,000. The average company had 11 officers, with a somewhat smaller number at the headquarters office; over one-half had more than 25,000 employees. Of the 1,966 officers in these companies, 42 per cent served as members of the boards of directors. These figures reflect a high concentration of economic power among persons of high corporate status.

The formidable procedural problems involved in these interviews perhaps account for the fact that little such work has been reported. But there is little doubt that research directed to this stratum of our society is of great significance and that many important problems can be approached only through personal interviews with the business elite, e.g., the much discussed problem of the relationship between business expectations and decision-making.

⁴ Tandem interviewing is discussed in greater detail in an article by Kincaid and Bright in the *Public Opinion Quarterly*, XXI, No. 2 (1957), 304-12.

⁵ Of 180 companies studied, about three-fourths had headquarters offices in the 12 standard metropolitan areas, all vulnerable. Of their 1,966 corporate officers, more than one-fourth were working in the New York-N.E. New Jersey standard metropolitan area; nearly one-half in the 4 standard metropolitan areas of New York-N.E. New Jersey, Chicago, Detroit, and Pittsburgh.

their size and potential contribution to the economy in time of national emergency.⁶ We requested interviews with top-level executives in individual form letters to the chief executives in which were stated the study's aims and sponsor and reasons for requesting an interview with a high executive. The aim of the study was not so much representative of executive opinion as informed opinions and information from executives in a position to speak on the entire range of operations of their companies and hence on problems which might arise should the headquarters offices be eliminated. Since, presumably, only a handful of executives in any one company is qualified for such a task, it was especially important to seek interviews at the highest level possible.⁷

ENLISTING CO-OPERATION

In most sample surveys where a cross-section of a population is selected for interviewing, the rate of refusals to be inter-

⁶ Size was defined by total assets. It was assumed that the largest corporations would have the most difficulty if headquarters were neutralized, having more components of organization than the smaller ones. Particularly in the industries whose production would be of prime importance in war, production is heavily concentrated in a relatively few companies. For example, in most industries, 50 per cent of each one's total U.S. employees are in a relatively small number of companies (e.g., rubber, 2; primary metals, 6; electrical machinery, equipment, and supplies, 6; transportation, 2; chemical, 17; professional, scientific, and controlling instruments, etc., 6). The large number of mergers in manufacturing companies in recent years intensifies the situation. The Federal Trade Commission recently reported 1,773 mergers in the manufacturing and mining field from 1948 through 1954; of which nearly two-thirds involved companies with assets of \$10 million or more (*New York Times*, May 20, 1955, p. 12).

⁷ We obtained interviews with executives of generally high status among officers at headquarters as follows: 2 presidents, 3 executive vice-presidents, 14 "other" vice-presidents, 8 secretaries or treasurers, 2 corporate attorneys, 2 comptrollers, 13 directors of departments or assistants to corporate officers, 3 managers of plants (1 vice-president and 2 general managers). The total number of respondents is more than that of corporations either because more than one interview was conducted within a company or because more than one respondent was present at a single interview.

viewed can usually be kept low enough to allow the claim that it does not influence the representativeness of the sample. Obtaining the co-operation of a representative sample of large corporations may be quite a different matter, particularly if it is important to differentiate according to industry. The number of companies at the top by almost any measure (sales, assets, employment) is relatively small, and a few refusals may render the sample unrepresentative.⁸ Even where there is not the problem of sampling error, as was the case in our exploratory study, a refusal may mean the loss of information concerning a company, since follow-up attempts are likely to be unsuccessful. All this shows the extreme importance of careful preparation for enlisting co-operation.

In our initial request a letter was sent to the chief executive of the company asking for his participation in a study of considerable interest to American industry and to national security. Here the names of the sponsor and the institution were given, with the explanation that we required interviews with executives well-qualified to speak on the over-all operations of the company.⁹

In our research, affiliations and sponsorship and the nature of the appeal seemed to be most important in getting co-operation. Executives are not likely to judge a request for assistance in a research project simply on its merits alone. Rather they tend to speculate on what a refusal would cost in public relations. If the affiliation and the sponsor are "reputable," they will, in all likelihood, at least consider the request. Co-operation in projects involving executive time costs a company a considerable amount of money, and if the anticipated return in enhanced public relations does not appear to warrant co-operation, a refusal or a dodge is likely.

⁸ In 1948 there were only 261 manufacturing companies in the United States with assets over \$50 million; there were only 923 with assets in excess of \$10 million.

⁹ While it was known that the president would only rarely grant an interview, this appeal was made in order that care would be given in delegation of the interview to another person.

As will be shown later, there were a number of refusals and dodges, the presumed prestige of the university and the sponsor notwithstanding.

The pressure on big business for co-operation in research emphasizes the importance of legitimate affiliation and sponsorship. The federal government in particular is demanding in its requests for data. Several of our respondents noted this and reminded us that ours was but one of the projects in which the company was participating. Their often hostile attitude to "studies" suggested that the saturation point is rapidly being reached. It follows that in a "sellers' market" co-operation is more likely to be extended, if at all, to those projects with substantial affiliations and sponsors. While this is not a unique problem in social research, it probably reaches its most acute form in the big-business community.

Apparently most research projects are viewed by business as something to be avoided if it can be gracefully managed. We have fragmentary evidence that any "opening" in the correspondence requesting assistance will be used as a basis for refusal. For example, we used two different versions of a form letter sent to the companies in the sample. The first in point of time requested not only an interview but also any literature pertinent to the study.¹⁰ In reply several companies refused co-operation on the ground that since there was no pertinent literature there would be no point in conducting an interview. While such refusals are not sound on logical grounds, they shut the door on the researcher, for refusal at the top level is the most difficult to circumvent.

From our experience we judge that an ap-

¹⁰ Actually, the effectiveness of the two letters is difficult to compare, since one was sent to a group of "smaller" large companies, and the other to the largest companies. Moreover, the appeal for co-operation was made for a somewhat different purpose in the two versions. Possibly the larger companies, because of their greater concern with maintaining a favorable public image, are more likely to co-operate in such ventures than are the smaller.

David Riesman suggests that concern with public relations may be just a rationalization for doing something that is actually interesting.

peal should include but one request. If an interview is the aim, the appeal should be framed around this, making no peripheral requests which could be used as a basis for refusing co-operation. And, in general, it is wise to keep letters requesting co-operation short and concise, if for no other reason than the limited time the reader has for correspondence.¹¹ A request is likely to be successful, too, if it stresses the practical aspects of the research problem and, insofar as possible, relates them to the company's own particular interests. We have abundant evidence that executives are not accustomed to appreciating knowledge for its own sake.¹²

Although we used this technique to only a limited extent late in the project, we found that an excellent means of getting entree to a corporation is through persons in a well-established business school or economics department or through members of a university's administration at the higher echelons who often have personal acquaintances among businessmen. We found this an almost sure means of assuring interviews in large corporations, although a necessarily limited one. However, it may be only one of the methods open in the event that a more direct approach proves unsuccessful.

PRE- AND POSTINTERVIEW WORK

The pre-interview preparations and the writing-up immediately after an interview limited the number of interviews that could be scheduled in a given time period. First,

¹¹ While the two versions of the form letter are not strictly comparable, the first version (requesting information in addition to an interview) was nearly twice as long as the second. From the first we obtained 68 per cent refusals; from the second 27 per cent.

¹² Once the problem has been established as a practical one, businessmen (not unlike informants in general) are frequently willing to supply information and opinion of a seemingly peripheral nature. For example, it was not difficult to obtain information relating to the informant's job history, his daily activities, his opinions as to what characteristics enhanced a man's opportunities for ascent within his company, etc. Frequently these diversions from the more strictly defined practical problem gave us insight for evaluating the information from the interview as a whole.

the respondents were speaking as management experts on a problem directly affecting their company, and we were therefore obliged to learn as much as possible about the company in advance, so that we would not waste time in getting answers to routine questions and to be sure that our questions were cogent. Indeed, our experience proved that the respondents in many cases expected that we would be well-informed about their company, at least to the extent of having read the publicly available literature. This preparation involved a search of the business literature—mainly periodicals, in this case—and the documents—annual reports, prospectuses, organization charts, histories, etc.—prepared by the company, all of which, though simple and straightforward, is time-consuming. Second, the material was written up jointly by both interviewers immediately after an interview, and, since we often obtained extremely large amounts of material, this was also a lengthy process.

We soon found that with two persons we could do no more than three interviews per week, equally spaced, without slighting some phase of the preparation or the write-up. Generally the interviews had to be scheduled considerably in advance because the respondents were away or were too busy. The time between the initial letter to the chief executive and the response ranged from a few days to several weeks, after which it was necessary to arrange for an interview, usually by telephone. In addition, checks were ordinarily made by telephone near the time of the interview to confirm the appointment for the interview, since our experience with a few unfortunate substitute informants had convinced us that it was sometimes advisable to reschedule an interview if our originally designated respondent had to leave town unexpectedly or for other reasons was unable to keep the appointment.

The drain on a research budget is obvious. The costliness of research similar to that reported here has to be faced. As will be shown, other circumstances, too, con-

tributed to the costliness of the interviewing, the most notable of which was the use of two interviewers.

THE INTERVIEW

No attempt will be made to analyze systematically all that influenced the interview. Rather we shall discuss two conditions which seemed most important: the use of two interviewers and the structure of the situation at it influenced the role of the respondent.

"Tandem interviewing."—Our decision to use two interviewers arose during the course of the preliminary interviews. Because we were jointly responsible for the design of the study as a whole, we both felt obliged to take part in the explorations which set the over-all design of the research. It did not occur to us at this time that two interviewers could be used throughout. As the preliminary work progressed, however, the advantages of tandem interviewing became apparent, and all subsequent interviews were so conducted.

One of the major advantages was the smoothness of the interviews. With one person taking notes and the other making queries, the interview was not broken by frantic attempts to keep up with the respondent. It was possible for us to get a great amount of material in a short time, while conducting the interview as a continuing conversation¹³ We feel also that one of us, and then, again, the other, contributed in maintaining a good working relationship with certain respondents, though this is difficult to state explicitly.

Another advantage is the efficiency with which the required information can be elicited. What one interviewer overlooks, the other is likely to remember, and this is particularly true when both are familiar with the work from the beginning, as was the case here. Also, unanticipated lines of thinking on the general research problem were

¹³ We felt a mechanical recorder or a secretary's taking notes would not have been acceptable to most of our respondents. Frequently respondents asked not to be directly quoted, and they assumed some discretion on the part of the person taking notes.

better exploited, because one or the other of the interviewers was likely to pick up immediately any interesting and significant idea and probe for more detail. These advantages are even greater when it is remembered that there was only one interview with a given executive. While executives were not often reluctant to assure further assistance from the company if needed and willingly referred us to other persons for specific information, it was important to make the most of the time allowed with those who had granted us the first interview. This procedure reduced considerably the amount of follow-up work.

In the write-up stage of the results of the interviews the advantages of having had two interviewers is obvious. First, accuracy and comprehensiveness are enhanced by the collaborative account. Second, there is less chance for individual biases or for a personal theory to be perpetuated by unintended selection or emphasis of data. Third, the final analysis of the results as a whole is subject to the cross-checking of two persons who had both been present at all interview sessions.

Perhaps the best reason for the use of two interviewers is that the ability of the majority of our respondents called for it. Our respondents were, as a rule, highly articulate persons capable of thinking through abstract and complex issues and forming and expressing their views quickly and economically. We are certain that one person would not be able to exploit this sort of talent to the fullest, at least not in a short time. Our interviews, which averaged about one and one-half hours each, supplied us with a wealth of information. Since the time of executives is usually at a premium, it is important to get the required data in as short a time as possible. It may be that the added expense of using two interviewers is somewhat illusory. This would certainly be our view, at least in the case of exploratory studies. Moreover, the efficiency with which the data are collected makes it doubtful that the additional expense is really much.

We conclude that in a study similar to

ours it would be a waste of the respondent's talent to rely on one interviewer. At least, it is essential that interviewers be well-trained, whether it be tandem interviewing or not (this is hardly the situation for training the potentially good interviewer). Moreover, unless the data are of the most simple and straightforward type, the interviewers should be active in all stages of the work, including the analysis.

In addition to the presence of two interviewers, two other circumstances proved important in structuring the respondents' role in relation to the interviewers: the interviewers were referred to the respondent by the chief executive of the company, and the subject matter of the research was of national interest, with patriotic overtones. These elements cast the respondent in the role of a person "ordered" to talk to the interviewers about his organization's thinking and activities for planning in the matter of the vulnerability of headquarters, a field which seems to call for a company policy. We seldom found it a subject that our respondents could discuss apart from policy.

On the favorable side, the president's request that the respondent grant an interview provided us with the essential situation needed: the respondent was authorized to speak for the company. The subject, however, was such as to make the interviews difficult in a substantial number of cases. This is to say, a number of respondents took their role as spokesman for a company to mean that they should speak *only* in terms of company policy. We, of course, were interested not only in the company policy but also in the respondent's view as a high-ranking administrator or expert.

This is a type of problem that is likely to be encountered often in similar research. It originates in the necessary discipline enforced in a corporation. As one of our respondents said: "We [executives] can say just about anything at a policy meeting within the limits of common courtesy, but when a decision has been reached, it must guide the actions of everyone, including the dissenters." In another instance, when the

respondent was asked whether it would be possible to interview executives on the same problem in the lower echelons of the company (divisions, plants), he replied: "We tell the general manager our opinion on the matter. If he responded with anything different on the matter, it would be his own personal opinion and would not be relevant to what would happen if the actual crisis occurred."

Considerable probing is thus needed to distinguish between what is company policy for creating a desired public impression as against what actually obtains. One of our constant preoccupations was to phrase questions so as to get answers free of slavish adherence to policy dicta. For example, in companies that had made no plans for the continuance of headquarters functions in an emergency, we frequently found illogical views on the possible effects of the loss of headquarters: respondents were forced into taking the position that the loss would mean nothing because the company had made no plans, as though an admission that headquarters was in any way important to the operation of the company would amount to saying that the company was remiss. Our task was to get meaningful replies from the respondent as an expert administrator with an intimate knowledge of the company as a whole, in the face of a stand by the company that no planning was necessary for the contingency under discussion.

We found that a good way to overcome the hiatus between the respondent-as-expert and the respondent-as-policy-spokesman was to state explicitly that we were not "selling plans" and to emphasize the exploratory nature of the study as aimed at determining the range of planning problems and the feasibility of planning.

We found important differences between respondents who were well-versed in the matters of headquarters' vulnerability and those to whom the subject was entirely new. On the surface, the former provided much more valuable interviews, but on closer analysis it turned out that planning had become a sort of specialty among certain large com-

panies and that persons responsible for planning developed stereotyped ways of thinking. Several of the interviews with executives conversant with planning matters were much alike—the same things were mentioned as being important, and the same planning measures were recommended. A case in point is the microfilming of essential records. Among many respondents this is a matter beyond argument—something that every company provides for, as a matter of course. However, some with only limited familiarity with planning, or none at all, perceived what their better-informed counterparts overlooked: that microfilms of complex records were useful only to the extent that personnel with the ability and equipment to process them were available in emergency. Thus, many of our most valuable insights were obtained from respondents completely unfamiliar with the matter of headquarters' vulnerability or from those who were belatedly considering the need for planning and were therefore free to give opinions uninfluenced by a tradition of thought and practice.

There is reason to think that the problem of stereotyped responses is common. For example, when we asked most of our respondents about their companies' "executive development programs," a highly fashionable thing among large corporations nowadays, replies were often clichés and platitudes. Any subject which had no direct or immediate relation to the success of the company might be subject to the same caution.

Interviewing procedure.—To a greater extent than is probably the case in most types of interviewing, interviews with the business elite must be flexible. It is more sensible—indeed often inescapable—to shape the interview around the respondent rather than conduct it in a certain specified manner. Some of our respondents simply refused to be bound by a given sequence of questions, and in such cases it was necessary to fit in the queries where possible. Others would ask at the beginning of the interview whether we had a printed schedule or questionnaire for

their consideration.

The interviewer of executives is likely to pay for lapses in alertness. Respondents of this caliber are generally quick to pick up the fact that the same question has been asked twice, even in an unstructured interview; they quickly perceive and often point out the illogical. In general, we perceived a decided distaste for the inefficient, uneconomical, and inconsistent. The positive side of this tendency toward frankness is that such respondents did not seem to object to our questioning their own inconsistencies, particularly if the respondents were of the highest ranks and commanded knowledge of the over-all operations of the company.

Unqualified respondents.—Our entrance into the company through the chief executive necessarily meant that we interviewed anyone assigned to us, and in most cases the respondent had ample qualifications for our purposes. But in a few instances the persons assigned were in the bottom ranks among the ordinarily small group of administrators at headquarters. If such a person has a sound grasp of the company as a whole and expresses himself adequately, the results are often as good as those obtained from persons at the top. It happened in some cases, however, that he was not articulate or knowledgeable or was fearful of "sticking his neck out." In such cases our position was untenable and we wrote off the company; it would have been impolitic to request another, more expert, respondent.

It might be that in a study similar to ours, but of less presumed practical significance, the interviewer would face the problem of unqualified respondents more often than was the case in our experience. That is, the researcher would perhaps be assigned to persons whose time was judged least valuable and who might be poorer respondents. Certainly, if the views of persons with a sound picture of over-all company organization and operations are needed, the lower-ranking respondents will not in general be suitable.

Tandem respondents.—In a few cases we found our respondent had arranged for an

additional person to be present at the interview. (One respondent, the president of the company, midway in the interview called in the entire executive staff of headquarters. The ensuing discussion was most interesting but of little value for the project.) In principle, we anticipated that this would be advantageous in that the added person was a potential source of new ideas, but in practice it usually worked to our detriment. For one thing, where the difference in ranks was considerable, either there was complete silence on the part of the subordinate or the superior wasted time by asking the subordinate his views on matters about which the superior had expressed himself. The interviewers were rarely afforded the opportunity to ask the subordinate his views directly because the superior officer made clear that questions were to be directed to *him*. In cases where the rank of the two respondents was about equal, time was wasted by the respondents' tendency to end a statement by seeking confirmation from each other, a democratic mannerism which was universally encountered. In these cases the presence of more than one respondent was decidedly inhibiting: respondents were less likely to say what they thought in plain terms and to voice personal opinions in front of a colleague.

The practice of having colleagues sit in on group discussions is evidently common in all formal organizations, business and government in particular. In any research which requires the responses of persons as experts in their line, the presence of others (experts or not) is a serious matter. In this study we had tandem respondents in 12 of the 47 interviews—26 per cent of the cases—which means that in a large-scale study of business executives the problem of dealing with more than one respondent in a given interview can be expected.

The research experiences reported here cannot be applied with any great degree of generality to other research. Some of our problems, however, are likely to be encountered in studies requiring interviews with

high-ranking executives in large corporations. The following points may be taken as the probable crucial phases in such research.

Enlisting co-operation of large corporations is clearly prerequisite to successful research, and careful planning should precede this phase of work. The sponsorship and affiliation of the researchers and the "practical" emphasis given the subject of the study may be decisive.

The use of two interviewers has both advantages and disadvantages. In our experience the advantages outweigh the disadvantages: tandem interviewing eliminates a major source of bias inherent when there is a single interviewer and thus, while more costly, is probably justified. Moreover, the increased efficiency with which data are gathered reduces in large part the presumed added expense.

The qualifications of interviewers should be carefully considered. The generally high level of ability of respondents means that

the use of unqualified interviewers would be a gross waste of everyone's time.

Persons undertaking research in this field should be prepared for a slow-moving and rather expensive project. Arrangements for interviews, their preparation, and then the write-up of the results severely limit the number of interviews that can be made in a given period of time. Moreover, qualified interviewers are expensive.

Careful attention must be given to the respondent's conception of his role in the interview. Our experience showed a widespread tendency for executives to speak more as spokesmen on company policy than as expert administrators.

Many of these points are common to interviews with respondents in other strata of society. Many of our problems and solutions are obvious, but only in retrospect, because we did not see them as such until we tried various tactics and discovered what worked.

STANFORD RESEARCH INSTITUTE
COLUMBIA UNIVERSITY

LETTERS TO THE EDITOR

KOLKO'S "MOBILITY AND STRATIFICATION"

July 26, 1957

To the Editor:

In an article entitled "Economic Mobility and Social Stratification," which appeared in your July, 1957, issue, Mr. Gabriel Kolko asserts that since 1929 the economic mobility of selected occupations in the income deciles has remained relatively stable; that, because of the wide inequalities in income between the deciles, symbols of economic status have not been equitably distributed; and that the widely held image of America as an increasingly middle-class society based on a general distribution of high economic status symbols finds little justification.

Kolko's own statistics fail, however, to support his argument. In Table 1 Kolko shows the relative position in income deciles of unskilled workers and of semiskilled and skilled workers in six selected industry groups in 1929 and 1949. Of the six groups of unskilled workers, one was in the same income decile in 1949 as in 1929, three had moved up one decile, and two had moved up two deciles. Of the six groups of semiskilled and skilled workers, two remained in the same decile, three had moved up one decile, and one had moved up two deciles.

In Table 2 Kolko portrays the relative median income position of the major occupational classes in 1939 and 1954 in form of an index, using clerical wages as equal to 100. All manual groups show significant increases in their incomes between 1939 and 1954, as do farmers and professional and technical workers. The only group that shows a decline is the top group of managers, officials, and proprietors. As a result, the income differentials between the top groups

and the bottom groups have decreased during those fifteen years.

Moreover, the use of percentages and other ratios without indication of their base is entirely misleading. It is hardly surprising that in the 1950's, as in 1929, the highest income decile is composed primarily of professionals, managers, and businessmen and that ditch-diggers have remained in the tenth income decile, too. What is significant is that the dollar basis of these percentages has changed dramatically. Comparing the income distribution of American families and unrelated individuals in 1929 and 1953 in after-tax, 1953 cash dollars, we find that the percentage of units receiving less than \$2,000 has declined from 42.7 to 22.9. Those receiving between \$2,000 and \$4,000 have decreased from 36.2 to 32.2 per cent, while the proportion receiving between \$4,000 and \$7,500 increased from 15.1 to 34.9 per cent, and the proportion receiving more than \$7,500 rose from 6.0 to 10.0 per cent. Moreover, of the fifteen and a half million families in the \$4,000-\$7,500 after-tax, cash-income bracket in 1953, 58 per cent were headed by manual workers. (Statistics are from Editors of *Fortune*, *The Changing American Market*, pp. 262-64.)

These shifts in dollar incomes clearly describe a massive push of large segments of manual workers' families into income brackets which indeed make the acquisition of middle-class status symbols possible. It is quite true, of course, that most of the working-class families in the middle-income brackets had somebody earning besides the family head, usually the wife, and that they make extensive use of credit in their consumption. But what is relevant, of course, is the willingness and ability of a great many

working-class wives to supplement the husband's earnings in order to attain a middle income and the flexibility of the economic system which permits them to enjoy a middle-class pattern of consumption.

Kolko cites the following statistics on car ownership, a major status symbol:

Car ownership spread from about 55 per cent of the consumer units in 1935-36 to 59 per cent in 1941 and to 70 per cent in early 1956. Yet only 29 per cent of the 11 per cent of all consumer units earning less than \$1,000 in 1955 owned cars, while among the top 14 per cent of the consumer units (\$7,500 plus) 94 per cent were car owners. Between the two poles existed a situation reflecting income inequality—a similar situation to that in 1936 and 1941.

This reader is inclined to interpret an increase in car ownerships from 55 to 70 per

cent as a significant advance in the standard of living. And, as regards consumer units earning less than \$1,000, Kolko should certainly have mentioned that their proportion dropped more than by 50 per cent and now amounts to less than 8 per cent of the total.

No responsible social scientist makes the claim, of course, that economic inequality or social stratification has disappeared in the United States, but the facts certainly show marked reductions in income inequalities and very substantial improvements in the real income of broad groups of the American working population.

KURT B. MAYER

*Department of Sociology
Brown University*

REJOINDER

August 15, 1957

To the Editor:

The point of my discussion on the economic mobility of occupations was not that there has been no mobility since the depression but that the movement of occupations into higher income deciles has been relatively slight, especially since 1935 and 1939, and certainly not so great as to warrant the popularly accepted picture of America as an economically classless society. The Census data given in Miller's study show that only 6 of 118 occupations listed had by 1949 moved two or more deciles up the income scale since 1939. My assumption is that an upward or downward movement of one decile does not represent a very significant change. Had Dr. Mayer compared the 1949 income position of industrial workers given in Table 1 to their position in 1935-36, and not 1929, or taken my calculations on Census data into account, he would have come up with an analysis of mobility similar to my own.

As I suggested in my article, the narrowing of the median income differentials among occupational classes from 1939 to

1954 cannot be viewed other than in a historical context. In 1939 there were much larger proportions of unemployed unskilled and manual workers than white-collar workers, and full employment from 1942 on brought them greater percentage increases in income. As Horst Mendershausen shows in his study of income distribution in the last depression, the income of low-income groups fell substantially faster than among the rich. From 1914 to 1920 the occupational income differentials also declined, but later they increased sharply. All the available evidence favors this cyclical theory of occupational class income differentials.

I am frankly skeptical of the statistical reliability of *Fortune's The Changing American Market*, which Dr. Mayer cites in his fourth paragraph. One cannot use sources with rigid ideological commitments objectively to evaluate a problem of vast import to social theory and practice. Not using detailed source references, the *Fortune* study tends to produce statistics not easily verified and which no one else has yet managed to find in the government sources. For example, in the tenth article in "The Chang-

ing American Market" series (*Fortune*, May, 1954), *Fortune* asserted that 17 per cent of the farmers in 1953 earned less than \$2,000, while data given in the *Statistical Abstracts* show that it was 37 per cent. Similarly, *Fortune* had about 12 per cent of the American families earning less than \$2,000 in 1953, while the *Federal Reserve Bulletin* placed the figure at 19 per cent of the families and 23 per cent of all spending units.

In terms of real income there have certainly been increases since 1939 but not to the extent that Dr. Mayer implies. "Compared with 1929," says a Department of Labor report in 1955, "average real income measured in constant dollars has increased roughly 30%; 40% on a per capita basis, reflecting the smaller size of the family." This is comparable with an increase of about 40 per cent from 1914 to 1929. But long-range averages often obscure important short-range trends: from 1944 through 1956, for example, the real income of the average manufacturing worker has increased but 12 per cent.

Even though the total amount of goods consumed by the average family can in-

crease and absolute consumption can rise within every income group, the same ratio of inequality among the income tenths will remain in their economic power and absolute consumption so long as the distribution of income remains unchanged. Inequality, when prestige and necessity are taken into account, can remain unaltered. This view is given factual support both in my article and in the much-neglected Bureau of Labor Statistics' *Survey of Consumer Expenditures* in 1950. Let me repeat that the average income of the members of the highest income decile is twice as great as that of the members of the second highest decile and nearly thirty times greater than the income of those in the lowest decile. Such inequalities in income create objective economic classes in every important sense—in consumption of durable goods, health, education, housing, etc. There must be additional research on these phases of economic inequality to achieve a realistic view of class in America.

GABRIEL KOLKO

Cambridge, Massachusetts

NEWS AND NOTES

Durkheim-Simmel commemorative issue.—

The year 1958 is the one hundredth anniversary of the births of two of our most distinguished sociologists—Georg Simmel and Émile Durkheim. To celebrate this occasion, the *Journal* is planning a special issue, tentatively set for May, 1958, devoted entirely to their work.

The basic theme of the issue is to be the continuity between the work of Simmel and Durkheim and that presently being done in the field of sociology and related disciplines.

Scholars who wish to contribute to this issue must submit their manuscripts to the editors not later than January 1, 1958.

University of Alabama.—The University has developed a new curriculum in experimental statistics sponsored by the departments of economics, mathematics, psychology, industrial engineering, and sociology and is now planning a modern computations center.

Members of the University of Alabama department will offer five courses in Stillman College during the coming academic year. Paul Foreman has assumed an advisory sponsorship for development of the social sciences at Stillman, an accredited four-year Presbyterian college for Negroes in Tuscaloosa. Dr. Foreman is also chairman of a committee planning a new social sciences building to house the departments of history, philosophy, political science, and sociology and anthropology and the Bureau of Public Administration.

Charles D. McGlamery, formerly of Emory University, has been appointed to an associate professorship. He will assume responsibility for courses in industrial sociology for the personnel management and industrial engineering programs as well as for the Department of Sociology. He will also take over instruction in technology and social change for Senior engineering students.

David M. Shaw, of the University of Minnesota, has been appointed to an assistant professorship. He will teach social psychology and a Senior first-year graduate sequence in research techniques and design. This latter sequence is being developed to provide a more

effective transition between undergraduate and graduate major programs.

Marion Pearsall's leave of absence has been extended to cover the 1957-58 academic year. She will continue with the Russell Sage project at the Boston University School of Nursing.

David L. DeJarnette, archeologist of the Moundville State Park and Museum, has been appointed to an instructorship in anthropology.

Bar-Ilan University.—Samuel Koenig, associate professor of sociology and anthropology at Brooklyn College, and deputy chairman of the department, has been awarded a Fulbright grant to establish a department of sociology at Bar-Ilan University, the new, American-patterned university at Ramat Gan, Israel.

Bar-Ilan University, established in 1955, is an American-sponsored liberal arts institution of higher learning. Its president, Dr. Pinkhos Churgin, was formerly dean of Yeshiva University in New York.

Canadian Political Science Association.—The Anthropological and Sociological Chapter of the Canadian Political Science Association held its second annual meeting in Ottawa in June. Professor Everett C. Hughes of the University of Chicago was guest speaker. About fifty members attended. The following officers were elected: honorary chairman, Carl A. Dawson, emeritus professor of sociology, McGill University; chairman, Harry B. Hawthorne, University of British Columbia; vice-chairman, Aileen Ross, McGill University; secretary-treasurer, Kaspar D. Naegle, University of British Columbia; members of the executive, Frank E. Jones, McMaster University; Jean-Charles Falardeau, Laval University; Douglas Pullman, University of New Brunswick; and Jacques Brazeau, Research Branch, Ministry of Defense.

All members of the Association are eligible to join the chapter.

Carleton College.—Samuel M. Strong, chairman of the Department of Sociology, was elected chairman of the Committee on Mental

Health of the Society for the Study of Social Problems at the annual meeting in Washington, D.C., in August. Dr. Strong was also reappointed to the Committee on Social Research of the American Sociological Society.

University of Chicago.—During the absence of both Everett C. Hughes and Helen MacGill Hughes, Peter H. Rossi, assistant professor of sociology, will be editor of the *American Journal of Sociology*; Mary Lea Meyersohn will be managing editor. The Hugheses will return to the campus in September, 1958.

James A. Davis, assistant professor of sociology, formerly of Yale University, is to be the new book-review editor of the *Journal*.

Ernest W. Burgess, emeritus professor of sociology, will return in the Winter Quarter to his work as consultant in aging and retirement to the Industrial Relations Center.

Philip M. Hauser, Donald J. Bogue, and Leo Goodman attended the meetings of the International Statistical Institute and the International Union for the Scientific Study of Population in Stockholm in August.

Five fellowships for research in law and the behavioral sciences at the University of Chicago have been granted for the academic year 1957-58. They are awarded to W. Howard Mann, associate professor, Indiana University School of Law, Bloomington, Indiana; Erwin O. Smigel, associate professor of sociology, Indiana University; Louis Kriesberg, instructor in sociology, School of General Studies of Columbia University, New York; Reginald A. H. Robson, assistant professor, University of Nebraska College of Law, Lincoln, Nebraska; and Joseph Lazar, assistant professor of business law and labor relations at the United States Air Force Institute of Technology, Wright-Patterson Air Force Base, Ohio.

Duncan MacRae has joined the Department of Political Science as assistant professor, with responsibility for developing political sociology.

Chicago Area Project.—The *Journal* notes with regret the death of Clifford R. Shaw on August 1, 1957. Dr. Shaw was perhaps best known for two contributions to the study of crime and delinquency: the use of the autobiographical document as data in the study of juvenile delinquency and the use of differences in rates of juvenile delinquents between areas of the city as indexes of differences in features of local social life significant in causing delin-

quency. The former is presented in *The Jack-roller* and in *The Natural History of a Delinquent Career*; the latter in *Social Factors in Juvenile Delinquency* and in *Juvenile Delinquency and Urban Areas* (with Henry D. McKay). These works formed part of the tradition of University of Chicago studies of social process and urban social life inspired by the late Robert E. Park.

Less familiar to the current generation of sociologists were Shaw's achievements in developing a field of practice based almost entirely on sociological postulates. The Chicago Area Project, which he founded with the assistance of Ernest W. Burgess, established an experimental program for the prevention of delinquency in which the key role was assigned to influential local residents of neighborhoods with high rates of delinquency. Shaw's work in elaborating this program is in some measure responsible for the fact that practitioners in the prevention and control of delinquency today are turning increasingly to sociology as a source of fresh insight.

Community Studies, Inc., Kansas City.—Irwin Deutscher has been appointed director of the newly formed Division of Research in Health and Welfare, which began operations in October.

Warren Peterson and Jacquetta Burnett have recently completed a study of the current status of 1956 graduates from public high schools in the urbanized area and are currently engaged in an analysis of age, sex, and social class differences in adult leisure interests.

Audrey Forrest, a Community Studies fellow from the University of Nebraska, is engaged in research on interracial leadership.

Andrew L. Wade, a Community Studies fellow from the University of Wisconsin, is completing a study of vandalism and has accepted a position at the University of Iowa for the coming year.

Codification of methods of gathering participant-observation data and analyzing them has been occupying the attention of Howard S. Becker and Blanche Geer, who are also preparing a monograph on a study of medical students at the University of Kansas. Anselm Strauss and Everett C. Hughes are collaborating in the preparation of the monograph.

Peter New received a grant from the United States Public Health Service to enable Community Studies to sponsor a national nursing research conference in Kansas City in September

attended by social scientists and nurses currently engaged in nursing research.

Helen Maxine Brown is developing current demographical characteristics and future population estimates of the Kansas City Metropolitan Area.

Research in the field of education will be undertaken this fall at Community Studies by Bahjat B. Khleif, of Nazareth, Israel, who is now completing requirements for his Ph.D. degree at Johns Hopkins University.

Thomas S. McPartland is teaching anthropology at Park College in the fall term. He is completing a report on career patterns in nursing and also working with John H. Cumming, research director for the Greater Kansas City Mental Health Foundation, on a two-year project studying the sociocultural factors and psychological states in mental illness.

Elijah White is joining the staff of the National Health Survey, to work in the Special Studies Division, in Washington, D.C.

Community Studies has received a matching grant of \$150,000 from the Ford Foundation for the study of metropolitan area problems.

Dan C. Lortie, of the University of Chicago Studies in Kansas City, has completed his work with Elaine Cumming on the Kansas City Study of Adult Life.

Lois Dean, formerly with the University of Louisville, has joined the Study of Adult Life staff.

Cornell University.—Rodney Francis White, an industrial sociologist from the University of Chicago, has been named a research associate in the Sloan Institute of Hospital Administration in Cornell's Graduate School of Business and Public Administration. He will also be an assistant professor of administration in the School and an assistant research director in the Cornell Study of Nursing, a project in the University's Department of Sociology and Anthropology.

Duke University.—Howard E. Jensen, who has completed twenty-six years of service in the department, including twelve as chairman, will continue on as an active member of the department until retirement in 1958.

John C. McKinney, formerly of Michigan State University, has been appointed professor of sociology and has taken up the duties of chairman of the Department of Sociology and

Anthropology. Dr. McKinney will conduct work primarily in the areas of sociological theory and large-scale organization.

Robert Guy Brown, who has just completed work for the Ph.D. degree at the University of North Carolina, has joined the staff as an instructor in sociology and psychiatry. Dr. Brown's dual appointment is part of a developing collaboration with the Department of Psychiatry on research in the sociology of the aging.

Hornell Hart retired from the University on September 1 after nineteen years of service to the department, including eleven years as director of graduate studies. He has been appointed John Hay Whitney Visiting Professor of Sociology at Centre College, Danville, Kentucky, for the academic year 1957-58.

Lewis J. McNurlen, assistant dean of Trinity College, who has been devoting part of his time to teaching in the department, has been appointed chairman of the Department of Sociology at Drake University, Des Moines, Iowa.

The United States Public Health Service has awarded the Duke University Council on Gerontology \$300,000 a year for at least five years for the purpose of establishing the first of five regional centers to conduct basic research in the problems of aging. The departments of economics, political science, psychology, and sociology and the professional schools of medicine, law, and divinity will participate in the program. The Council operates under the supervision of a steering committee of three, consisting of Ewald Busse (Psychiatry), Eliot H. Rodnick (Psychology), and Howard E. Jensen (Sociology).

Échanges.—The attention of American sociologists is drawn to No. 31 of the periodical *Échanges*, which is on the subject of "Les Noirs dans le monde" ("The Black Peoples of the World"). It contains brief articles on color prejudice, Negro African relations, South African segregation, the Negro oral culture, the blacks in film and theater, the Negro problem in the United States, and the black people of Latin America.

Échanges may be ordered from the Editor, 16 rue St. J. B. de la Salle, Paris 6, France.

Educational Testing Service.—The eleventh series of research fellowships in psychometrics leading to the Ph.D. degree at Princeton University is offered for 1958-59. Open to men who

are acceptable to the Graduate School of the University, the two fellowships each carry a stipend of \$2,650 a year and are normally renewable. Fellows will be engaged in part-time research in the general area of psychological measurement at the offices of the Educational Testing Service and will, in addition, carry a normal program of studies in the Graduate School.

The closing date for completing applications is January 3, 1958. Information and application blanks may be obtained from the Director of Psychometric Fellowship Program, Educational Testing Service, 20 Nassau Street, Princeton, New Jersey.

University of Illinois.—J. Ed Hulette, Jr., and Richard Dewey have been promoted to professors of sociology.

Daniel Glaser and Robert W. Janes have been promoted to associate professors of sociology.

Bernard Farber has been promoted from research associate to assistant professor of sociology. For the past two years, Dr. Farber has been in charge of a research project on the effects of a severely mentally retarded child on family integration, sponsored jointly by the Department of Sociology and Anthropology and the Institute for Research on Exceptional Children. He will continue in charge of this project and will also offer a graduate course on research in family relations.

Edward H. Winter has been promoted to associate professor of anthropology. During the coming year he will be on leave of absence to conduct field research in East Africa. This field trip is a part of a three-year project to study regularities of change among contemporary native populations, under the direction of Julian Steward, research professor of anthropology. The field-work phase of this project is now under way, with a staff of eleven anthropologists stationed in Mexico, South America, East and West Africa, Indonesia, Thailand, and Japan.

Max Kaplan has resigned as assistant professor of sociology and of music to accept a position as associate professor in the School of Fine and Applied Arts at Boston University.

Eleanor P. Godfrey has resigned from her position as assistant professor of sociology and is moving with her family to Washington, D.C., where her husband will be employed in the United States Geological Survey.

Oscar Lewis, professor of anthropology, will

return to the staff after a year's leave of absence for study in Mexico City of the effects of urbanization and industrialization on families of rural background who have moved to the city. This research has been supported by a Guggenheim Fellowship and a grant from the University Research Board.

John McGregor, professor of anthropology, has established on the campus the central repository of the Illinois Archaeological Survey, which is engaged in cataloguing and mapping archeological sites in Illinois. He is also in charge of the Illinois Highway Salvage Program, conducting reconnaissance and preliminary investigation of potential archeological sites along the rights of way of projected new highways in the state.

Marquette University.—Bela Kovrig and Rudolph E. Morris have been promoted to the rank of professor.

Frank Atelsek and Paul J. Reiss have joined the Department of Sociology as instructors.

In the year 1957–58 the department will be directed by a committee consisting of Professor Morris as chairman and Professors Atelsek and Reiss as members.

University of Massachusetts.—Arnold Levine has been appointed an instructor.

Mary Griffin Powers has been appointed a part-time instructor.

J. Henry Korson is on sabbatical leave as visiting professor of sociology at Kwansai Gakuin University, Japan, where he is also doing research on a grant from the Asia Foundation.

In the absence of Dr. Korson, C. Wendell King is acting head of the Department of Sociology.

National Science Foundation.—Dorothy S. Thomas, of the University of Pennsylvania, has been granted \$15,000 for a year's study of migration differentials.

University of Pittsburgh.—The Administrative Science Center has appointed as staff members Buford H. Junker and Peter B. Hammond. Mr. Junker, who holds the Doctor of Philosophy degree in sociology from the University of Chicago, was visiting professor of sociology at the University of Kentucky during the 1956–57 school year. Mr. Hammond was trained as an

anthropologist in the Program of African Studies at Northwestern University. Under the auspices of the Ford Foundation, he recently completed a field study of cultural processes involved in the adjustment of an African people to an economic organization structured and administered by Europeans. He will also hold the rank of assistant professor of anthropology.

Society for the Scientific Study of Sex (SSSS).

—The Society has been organized to foster interdisciplinary exchange in the field of sexual knowledge. The aim of the Society is to bring together scientists working in the biological, medical, anthropological, psychological, sociological, and allied fields who are conducting significant sexual research or whose profession confronts them with sexual problems.

The SSSS will hold periodic scientific meetings for the presentation of research papers. It will organize symposiums, seminars, workshops, conferences, etc., to consider theoretical and practical problems in the sexual area. It will also publish a scientific journal devoted to relevant original studies and reports.

Further information concerning the Society and its activities may be obtained from Robert Veit Sherwin, 285 Madison Avenue, New York 17, New York.

Tulane University.—Forrest E. LaViolette has been appointed as University chairman of sociology for a three-year term, succeeding William L. Kolb. He is working with Joseph Taylor, of Dillard University, on a study of Negro housing in New Orleans, for the Commission on Race and Housing.

Arden R. King has been promoted to be professor.

Warren Breed, Munro S. Edmonson, Robert Lystad, and Robert C. Stone have been advanced to the rank of associate professor.

Leonard Reissman taught at the summer session of Columbia University.

Harlan W. Gilmore has received a grant from the University Council on Research to study the sex ratio of recent births, and Thomas

Ktsanes has a similar grant to study differential class participation in community problem activities.

Robert Lystad is on a year's leave in Africa, working with the West African Comparative Analysis Project, sponsored by the Carnegie Corporation.

William L. Kolb is the American editor of an English-language dictionary of basic social science concepts, sponsored by UNESCO. Robert C. Stone is assistant editor.

Vanderbilt University.—The *Journal* notes with regret the death of Andrew F. Henry at thirty-six. An Associate professor of sociology, Dr. Henry had published some twenty research articles and was co-author (with James Short) of *Suicide and Homicide* (1954).

Dr. Henry received his M.A. degree from the University of Chicago in 1949 and the Ph.D. degree in 1950. He was awarded a Social Science Research Council postdoctoral fellowship at Harvard University for 1950–51, remaining there as a research associate in the Laboratory for Social Relations and as a lecturer in the Department of Social Relations until 1955, when he joined the Department of Sociology and Anthropology at Vanderbilt.

His death cut short a most promising career and deprived his colleagues and students of a warm friend and an inspiring intellect. We shall all miss him greatly.

University of Wisconsin—Psychiatric Institute.—Ionel F. Rapaport, Wisconsin Psychiatric Institute, is organizing a new teaching and research program focused on the social aspects of mental disorders and juvenile delinquency. Marshall Clinard and others on the staff of the Department of Sociology are participating in this new program with the purpose of introducing new social concepts into medical education. Leslie A. Osborn, professor of psychiatry and director of the Division of Mental Hygiene, and the Staff of the Psychiatric Institute are in charge of the clinical education.

BOOK REVIEWS

La Musique, la radio, et l'auditeur: Étude sociologique. By ALPHONS SILBERMANN. Paris: Presses Universitaires de France, 1954. Pp. 229.

Introduction à une sociologie de la musique. By ALPHONS SILBERMANN. Paris: Presses Universitaires de France, 1955. Pp. 225.

Few sociologists have found music a rewarding subject of inquiry. Not that changing styles, taste, and tonality do not raise interesting and pertinent questions, but so far few have applied any type of scientific method to the subject and turned up significant findings. Some studies, conducted with adequate research equipment, suffer from an insufficient familiarity of their authors with music, while others reveal musical competence but do not transcend the scope of spirited improvisations. This state of affairs is particularly striking in view of the rapid diffusion of an increasing variety of music through the radio and the phonograph record—media particularly well adapted to the study of the subject. The present publications seek to fill that gap. The first volume grew out of a survey of the musical program of the French broadcasting system, aimed at finding and recommending to broadcasting stations norms for the transmission of music.

While the author appears to be familiar with representative instances of contemporary research in communication, he chose to use the "phenomenological" approach of an individual design as the one most appropriate. He does not attempt to measure the radio listeners' choices or satisfaction with the transmitted program. Since program planning is an educational function, Silbermann seeks to discover the norms which should guide broadcasting programs. He concedes the usefulness of quantitative studies for assessing the extent and types of music consumption, but he sees little value in statistics for the student of music broadcasting and the planner of radio programs. The function of the radio is cultural diffusion, and as such it is a factor of cultural change. Since music is an integral component of culture, and not only its reflection, the music broadcaster must equip himself with an image of the culture which he helps to disseminate

and guide, an endeavor in which he should have the benefit of a broadly representative advisory committee.

The primary motive for listening is to escape isolation and the oppressive "silence" which the tired worker experiences in his leisure time. The radio, in satisfying him, manipulates his emotions. The broadcaster can influence public taste, interpret music trends, state music values, commission and popularize new works, and promote or ignore certain styles and idioms. But the radio cannot, at will, impose any style or taste on the public. The broadcaster cannot overcome the distance between himself and his listeners: radio music lacks the ceremonial setting of the concert hall and the empathy of its audience. The radio listener is in a position to choose his broadcast or withdraw, divide or reduce his attention. The composition of the radio audience is more heterogeneous than that of the concert public. The broadcaster must therefore anticipate the effect of his program; he should be aware of the characteristics of his listeners and their preferred sonorities. These vary with sex and age: high frequencies, for instance, attract young persons and repel older ones. Occupations, on the other hand, bear no relationship to typical preferences.

The second volume is a case study in the musical expression of the *Zeitgeist*. The selected case is the career of the contemporary conductor and composer Eugene Goossens. Goossens was born in England, a country whose metamorphoses since Cromwell all but silenced the musical impulse of its citizens. Toward the turn of the century, however, English society became receptive to foreign influences: French impressionism, the Russian ballet, German functional architecture, the beginnings of non-objective painting, Schönberg's atonality, and American rhythm. Goossens' personal development reflects the transition of English music from its atrophied state in the Victorian middle-class salon to the revival of the tonal craft in the twentieth century. Silbermann traces Goossens' musical activities as a composer and conductor through his twenty-three years in the United States, his travels in France, and his more recent role in the musical life of Australia.

Throughout the book the author reminds

us that music is an expression of culture and social aspirations. Yet the reader in whom such pronouncements raise an expectation of subsequent analytical explorations and a statement of procedures finds, instead, a demonstration of the close kinship between the music of a period and its literature, philosophy, and fine arts. The assurance that social phenomena are mental and mental phenomena are social does not redeem the claim made in the titles of the two volumes. The titles are a definite disservice to the two publications which, it should be stressed, convey much interesting information to the musically interested reader.

ERNEST MANHEIM

University of Kansas City

Soziologische Exkurse: Nach Vorträgen und Diskussionen ("Sociological Commentaries Based on Lectures and Discussions"). Edited by THEODOR W. ADORNO and WALTER DIRKS. ("Frankfurter Beiträge zur Soziologie," Vol. IV.) Frankfurt-am-Main: Europäische Verlagsanstalt (for the Institut für Sozialforschung), 1956. Pp. 181.

This fourth volume in the series "Frankfurt Contributions to Sociology" presents some critical reflections of the members of the Institut für Sozialforschung in twelve edited and thoroughly footnoted lectures originally delivered over the radio in 1953-54. They are a succinct statement of the evolution and current status of selected concepts and substantive areas in sociology, offering a general perspective of sociology, with specific reference to Germany today, and implicitly dedicated to the proposition that sociologists develop and use empirical research to test broad social theory.

The twelve chapters are self-contained and so arranged that the development of the conceptions of sociology, society, the individual, the group, the mass, culture, and civilization is traced, beginning with Greek philosophy. Art and music, sociology and empirical social research, the family, community studies, prejudice, and the sociology of knowledge are then similarly treated.

In contrast to earlier volumes in this series—particularly, *Betriebsklima*, a sociological study in the industrial Ruhr—which were explicitly empirical, this volume proposes perspectives that are meaningful for an analysis of social con-

sequences. Reporting no new research and citing primarily early German sociologists and recent American research (including that of Horkheimer and Adorno while in the United States, empirical and descriptive material is used only for illustration. The central question is the relationship of quantitative and qualitative analysis.

The approach of the Institut is clear: the conflict between theory and research is so serious because "sociology, as the science of socialization, still today, and in no way anachronistically, is akin to philosophy, whence it sprang" (p. 107). Particularly in the mass society of today the Institut holds that sociology's most necessary task is to maintain a critical approach toward, and often counteract, pressures and ideas of mass media and powers that be. Focusing on what are deemed the basic social problems of Germany today—mass culture, family disintegration, and totalitarian influences—the Institut is explicitly prodemocratic and views these phenomena with alarm rather than detached objectivity.

For the Institut the future of sociology depends on understanding that historical and political conditions are, on the one hand, determinants of current society and, on the other, limiting factors unless viewed in a spirit of free research.

In the United States, even if theory is "tolerated as a necessary evil" (p. 108), we can develop empirical research because sociology is accepted. The major effort of the Institut, however, appears to be the re-establishment of sociology in an environment of major historical and political conflicts. This requires, according to the editors, a sound theoretical foundation and then the ability to deal with practical problems through empirical research.

BERNARD H. BAUM

Chicago

Potentialities of Women in the Middle Years. Edited by IRMA H. GROSS. East Lansing: Michigan State University Press, 1956. Pp. xiv+198. \$3.00.

Social scientists interested in problems of personality development have traditionally focused their attention upon childhood and adolescence. During the last decade, spurred on by an aging population, there has been a rapidly growing body of theory and research data

on the aged. The middle years of life, however, are still understudied; but one emerging concern is with the problems of discontinuity in the lives of middle-aged women.

This book reports a symposium conducted by the College of Home Economics at Michigan State University's centennial. Twelve contributors from such diverse fields as gynecology, nutrition, psychiatry, home economics, community organization, and "human relations" gave speculative and research papers on the conditions that hinder or enhance adjustment and development in the middle years. It is perhaps indicative of sociological lag in the study of aging that no professional sociologist was included among the contributors.

Organized in three sections, the book first presents a view of changes in women's roles during the last century and the expansion of middle age. This is followed by analyses of health, psychological, economic, interpersonal, and social problems in the middle years. The final section suggests solutions and the lines of approach to be taken by educators.

Like so many studies of other social problems, there is throughout the book an uncritical commitment to middle-class norms, such as the claim that education provides the most effective means to meet needs and understand changing roles. Despite this and the unevenness in quality of the various contributions, the book generally merits praise as a pioneering effort in the study of a grossly neglected age status.

MILTON L. BARRON

City College of New York

Conflict and Harmony in an Adolescent Interracial Group. By IRWIN KATZ. New York: New York University Press, 1955. Pp. 45. \$3.00.

This monograph is an intensive case study of an interracial group. The major conclusion is that in a society which defines Negroes as inferior to whites the opportunity for friendly interracial contact is likely to be exploited by members of both races, with the result that persons will learn unfavorable things about each other.

The subjects were forty-six Negro and white high-school students, who met once or twice a week to plan, rehearse, and present educational shows on brotherhood. Conditions seemed ideal for friendly interracial contact. Members of the

group came from middle-class homes, were superior students, active in extracurricular events. They had a concrete but creative task to do, and the official sponsorship of the municipal government gave them high prestige.

These circumstances did, indeed, generate an unusual degree of interracial friendship and sociability. At the end of the year there were the usual reports of change in the direction of more favorable attitudes toward the other race. Negroes reported less change than whites. Katz suggests that this was because Negroes had more realistic initial attitudes, based on a lifetime of close observation of middle-class whites. Also, in so far as Negro attitudes reflected resentment of discriminatory practices in the community, they remained unaffected by experiences with friendly individuals.

The paradoxical finding of the study, however, was that members who had the most frequent outside social contacts with persons of the other race were also those who, when interviewed, made the largest number of spontaneous unfavorable comments about the other race. This correlation was .83 for whites and .67 for Negroes.

This evidence of covert unfavorable learning led to an analysis of individual motivation. Exploitative status-seeking motives were found dominant in those who had most contacts with the other race. Negroes wanted to enhance their own status through association with whites; whites wanted relationships in which they could feel comfortably superior. The status-oriented individual was likely both to judge others adversely and to be judged adversely himself.

Several questions can be raised about the study. No report, for instance, is given on what persons not motivated by considerations of status learned and demonstrated. A detailed account of group activities during the year indicates that there was trouble in both performance and leadership; but Katz does not discuss the possibility that these group failures might be responsible for some of the criticism directed at individuals.

Finally, the value of a small case study of course rests upon the presentation and analysis of qualitative materials. But Katz has emphasized presentation at the expense of interpretation. The extensive use of quotations and descriptive materials serves to obscure rather than to clarify his very real theoretical contributions.

JEANNE WATSON

University of Chicago

The Age of Television. By LEO BOGART. New York: Frederick Ungar Publishing Co., 1956. Pp. xii+348. \$6.50.

In more than one field of research speculation runs far ahead of the hard facts unearthed by research, but of hardly any field is this more true than it is of television. Leo Bogart, in *The Age of Television*, has gathered together and drawn up an account of the facts research workers have thus far reported. A synopsis, within the space of a short review, of what is itself a synopsis would leave nothing but headings of chapters and subparagraphs. Suffice it to say that what has been found out about television audiences and their inscrutable habits is concisely summarized. Illustrative tables are carefully chosen and are thus very helpful. Besides, Bogart gives enough information on the research methods to expose, when necessary, the grounds for doubting any unwarranted inferences. In short, he does well what he sets out to do.

And what are the hard facts revealed by research? Once we move beyond description, research evidence is equivocal. When dealing with conflicting opinion, one can do little more than quote both sides. On occasion, however, divergent findings have something to do with the time at which the study was undertaken. Much that is known of the impact of television from the early studies is affected by the way television was diffused and by so-called "novelty" effects. Now that so many have television sets and have had them for some time, this information has little more than historical value or, at most, gives clues about the short-run changes produced by a major advance in mass communication.

But these short-run effects should be clearly separated from changes in basic outlook and in the balance among communication media which television may be ushering in. Bogart regrets that it is too late for a well-planned before-and-after study, so that we lack a systematic account of the social changes brought in the wake of television. Indirectly one can get at some of the same issues by paying attention to the categories and types of people who use the new medium in various ways. The elementary distinction between "light" and "heavy" users is gradually replacing that of owner versus non-owner. But do we know whether the "high" consumer of a certain kind of television content will also be, according to a previously established generalization, a high

consumer of the same thing in the other mass media?

Closely akin is the distinction between the deliberate use of television as a supplement to other media and the indiscriminate use of it to the virtual exclusion of other sources. The National Opinion Research Center study on football attendance took a pioneering step by distinguishing "marginal" football fans, attracted by the most exciting game, from those whose interest stemmed from old college ties. The notion of "marginal fanship" might apply to other interests developed by television, in politics and the arts, for example, when interest follows spectacle. If these are the orientations promoted by television, an effect on institutions would result, and thus non-owners as well as television addicts would come under the sway of the age of television.

Bogart's summary shows the gaps in knowledge in such areas as television and politics and the rise of a "TV-generation." His treatment of the educational uses of television is, incidentally, very sketchy. Other than citing evidence for the relative effectiveness of television as a classroom tool, he overlooks its potential impact on schools, standards, and teachers. The French téléclubs are mentioned in one sentence, just as foreign research, generally, seems to have been regarded as outside the focus of this book. However, the inclusion of one or two more sources would not change the over-all picture drawn by this collation of commercial and academic research.

I do regret at times the way the author has kept himself in the background. He seems at his best when he infers boldly what can be learned about television from general mass-media research, as he does in the chapter on "Popular Culture and the Appeals of Television." His evaluations also are usually worth paying attention to. All in all, this book is a valuable guide in television research, showing what is lacking and providing an authentic summary of the array of supported facts which should be the indispensable foundation for all future endeavor.

KURT LANG

Queens College

Processes of Organization. By ROBERT S. WEISS. Ann Arbor: Survey Research Center, Institute for Social Research, University of Michigan, 1956. Pp. ix+117.

This report, part of a larger study of a federal agency devoted to sponsoring research related to national defense, is concerned with the structure and functioning of the organization. The main group of respondents are "scientific administrators," whose main functions are the investigating of proposals received by the agency and the assisting of organizations which have been granted funds. Reports of the scientists' superiors, who are responsible for final decisions on these proposals, add another dimension to the data.

The description and categorization of the functions performed by these officials are based on responses to questions in an interview about their jobs, their supervisors, and the organization itself. These functional activities are characterized as policy-making—the making of relatively autonomous decisions concerning research proposals; informational—supplying information and orientation to the policy-maker; implementing—carrying out the policies decided upon; administrative—the co-ordination and distribution of work among the members of the bureau; and service—performing the routine duties necessary to carrying out the goals of the organization.

Systematic sociometric techniques are used to describe the structure of the organization. A matrix of mutual choices in response to a question about work contacts is analyzed, and the structure discerned agrees quite closely with the official organization chart. The groups analyzed here are the scientific administrators and the administrators of the organization. The latter exhibit closer agreement with the organization chart than the former. The sociometric data also disclose liaison individuals who connect two separate work groups. Several appendices are devoted to discussing the sociometric techniques.

In his discussion of adaptation, Weiss concentrates on the gratification which the scientists get from the types of work they perform. Even though the scientists disavow personal relations as a major attraction of their work, a comparison of more and less cohesive work groups would clarify the discussion of satisfaction with work. It should have been possible to construct an index of cohesion from the sociometric data. Also, the reported convergence between organization chart and sociometric data may be due to a correct representation of the structure of the organization, or it may be biased by reports based on adherence to

the official norms of the agency. Empirical data would have aided in distinguishing between these possibilities.

Processes of Organization makes a useful contribution to the study of formal organization by the development and application of appropriate systematic sociometric techniques. Validation of these techniques by observation would have enhanced the work.

JOE L. SPAETH

Chicago, Illinois

The Practice of Unionism. By JACK BARBASH. New York: Harper & Bros., 1956. Pp. xii + 465. \$5.00.

Only one paragraph in Jack Barbash's book particularly moved me. It is in the last chapter, and it reads: "I find it more difficult to defend what may well be an inevitable outcome of 'bigness'—a bigness, perhaps made necessary to cope with the bigness of the problems. I miss most of all the kind of personal humility—a consciousness of doing God's work, as it were—on the part of many union leaders, that for me is a necessary quality of a humane movement, whether it is a labor movement or any other kind." I believe it is the interpersonal in organization, the conflict of egos, the motives, which play more significant parts in the determining of union history than records and convention reports. But Jack Barbash's description of the inside of labor unions did not really picture it as I had experienced it. Where are the caucuses, the trips to Miami and the races, the deals big and little? Certainly not in the minutes! Memos are not written to reveal events or to make writing easier for researchers and historians. Instead, they are more often than not written to cover up the tracks of the past and to project the direction of the future. So a book on the inside of trade unions is more useful if there is more interpretation of events and less recording.

Most of the emphasis in Barbash's book is on the period since 1933. He draws on labor's own sources and on his own very intimate knowledge. That which is included in the book is both factual and accurate. Reading it places the bread-and-butter activities of the union movement in focus. It also recalls to my mind the structure and superstructure. But there were no people in it. Only occasionally some

names! But perhaps people become names when the organization takes over.

However, the book is an excellent source for the person, in labor or out, who wants to get a picture of the union movement as a going concern (and a going concern it certainly is); it describes organizing the unorganized and presents unions as instruments of action with their problems of internal government and relations with other organizations and government. The unions bargain, sometimes they strike, and too often they become involved in jurisdictional strikes and raiding. They also have staffs. Some educate, at least to the extent of preparing their members to accept policy. They were once beset by Communists and developed leaders of many and varied talents. All this is elaborated on by Barbash.

Probably I am most impressed by the constant expansion of the AF of L-CIO activities even in the years since I left the CIO in 1948. Perhaps this is because most of the big and efficient headquarters of the federation and internationals have been built since then. The buildings are symbols not only of affluence but of the "broadening sphere of active union interest in politics, legislation and collective bargaining . . . and the business-like operation of unions." And Jack Barbash writes that there may be others in the labor movement "to whom a little less posturing, a little less conspicuous consumption, a little less of the phrases 'my unions,' 'my members,' might make them feel better about unions and the labor movement, if this is important."

It can mean the difference between enlisting loyalties on the basis of intellectual and emotional attachments and enlisting them solely on the basis of usefulness. Historically, movements, including the labor movement, have had to forfeit both needs to live and grow.

KERMIT EBY

University of Chicago

Codetermination: Labor's Middle Way in Germany. By ABRAHAM SHUCHMAN. Washington, D.C.: Public Affairs Press, 1957. Pp. vi + 247. \$4.50.

One may compliment Shuchman by saying that if the title page did not give his specialty as economics, the author could quite well have been identified as a sociologist or political sci-

entist. The social sciences seem to have matured considerably in recent years, and boundary lines between specialties are now defended largely by the small academic old guard. The book at hand has profited greatly by interdisciplinary cross-fertilization.

The author brilliantly, thoroughly, and concisely explores the history, theory, and implications of "codetermination," i.e., the participation, in varying degrees, of organized labor in economic decisions at all levels from the individual factory to national planning. He concentrates on German experiences, but the importance of these developments for all capitalist economies is obvious. Here is a system, already well developed in Germany, intended by its proponents to resolve the conflicts and contradictions of capitalism without paying the totalitarian price. While clearly in sympathy with his topic, the author succeeds in maintaining a critical perspective.

Of particular interest to sociologists is the final third of the book, in which the economic, political, and social implications of codetermination are described and discussed. The doctrinaire conception of capitalism and political democracy is avoided, and the author refrains from name-calling and stereotyping on a topic set in a country in which traditional bitterness is well known. For example, the book notes that German Catholic and Conservative groups, on the one hand, and the Social Democrats, on the other, favor codetermination in principle, although at first glance corporatism and socialism seem far apart. Again, the author carefully traces the origins of the movement from their development in the mid-1800's through the monarchy, Bismarck, the first World War, and the Weimar Republic.

This brief review cannot do justice to this excellent work on a crucial topic. The book should be widely read and much quoted, and deservedly so.

ARTHUR JORDAN FIELD

Brown University

Management Succession. By the ACTON SOCIETY TRUST. London: Acton Society Trust, 1956. Pp. 139. 10s. 6d.

Increasing attention has been paid by the social scientists in recent years to business organizations and the process of management,

and, in particular, to mobility and social characteristics of management personnel. This book reports research on succession in a sample of the managers in large manufacturing companies in Great Britain.

The report, largely factual, is both descriptive and normative. A survey of the educational background and career history of 3,300 managers in 27 large industrial organizations is followed by an account of the recruitment, selection, training, and promotion practices of a larger sample of 50 companies and then by a description of how lower levels of management and employees view promotion and the way it is practiced.

Apparently the Horatio Alger myth characterizes contemporary life in Great Britain as well as in America. Over half the managers started their working life as manual workers or clerks. But, whereas in the past only one in four possessed professional qualifications and higher education, there is now a distinct trend toward professionalization among younger managers.

The investigators set forth certain requisites for a successful policy of management succession. Paramount is the necessity that higher management be genuinely concerned about the development of future managers, yet they found that only one-third of the organizations studied paid systematic attention to this problem. Other requisites are talent-spotting and the early selection and training of potential managers, clarification and publicizing of promotion policies, and the reducing of inequalities in opportunity and of the blocking of aspirations.

Contrary to studies of management succession in America which point to increased opportunities for mobility, this investigation concludes that executives in Great Britain are becoming a more highly selected group than in the past: increasingly they are being drawn from more restricted backgrounds with higher educational qualifications. The research workers predict the creation of a managerial class unless industrial organizations begin to practice procedures for picking out exceptional individuals from their lower echelons.

The book is a contribution to the body of empirical data on occupations and mobility. While focusing primarily upon industrial organizations, it also has a bearing upon the study of social change and comparative social organization. Its weakness lies in lack of theory

and failure to relate findings adequately to the larger social scene.

NORMAN H. MARTIN

University of Chicago

Theory and Practice of the Social Studies. By EARL S. JOHNSON. New York: Macmillan Co., 1956. Pp. xviii+476. \$5.75.

I know of no book like this in the social studies, none that promises to change teacher education in any way as much. The fact that the book is a college text does not deny it literary merit. The writing is mature, motivational as well as informative, and obviously a summing-up of years of learning and teaching. Themes are several, chiefly the importance of the self, the "great oughts" (democratic humanism) of everyday life, and the unity of the social studies.

The story begins, as well it may, with students and teachers. What is the purpose of their work together, and how is this to be realized? The answer might have been a stockpiling of specific items; but it is not. "The supreme end of education in a democracy," and particularly (if I may add) in the social studies, "is the making of democratic character"—characters that are decently different; that get and give strength to the wholeness of our way of life; that are disciplined in faith and truth and justice, in beauty, courage, love, dignity, and humility. Next comes a chapter on the "therapeutic climate," the kind of classroom in which these attitudes can be taught or, better said, be caught.

The most critical points in the first fifty pages come in chapters ii and iii. The first deals with continuity and discontinuity in culture, hence in character, and then casts the teacher in the difficult roles of priest and prophet. As priest, one's task is to pass on the culture in which life has been shaped. As prophet, the job is to interrupt this continuity, to promote change by reflective inquiry. This is to be done, in chapter iii, by means of "unavoidable indoctrination," a kind of teaching-learning in which, with the techniques of logic and science, a search is made for the "changing permanents" in the democratic humanistic creed. All this is quite tricky as it is reasoned out, and no few lines can do justice to the author's insight, much less to his caution.

The next four units treat of society as a spatial pattern and a social order and as persons and institutions. The high point is chapter vii, "The Crisis in Valuation," on the value conflicts and consequent lags, confusions, and immoralities. The next two units focus on the social studies within the framework of general education, with attention to the relation of liberal and vocational education, the social, humane, and natural sciences. Two chapters deal with the formation of attitudes, conceived as "the ultimate focus of all the social studies." A pair of chapters analyze the method of inquiry, relating sciences to moral matters, and then a series of units deal more or less with the social psychology of teaching and learning. Emphasis here is on the "dynamic self," with interests and needs as central. Other units deal with communication, discussion, and transfer of learning.

Chapter xxi presents several perspectives in social studies teaching, the chief areas being United States history, world history, study of comparative cultures, current social problems, civics, economic theory, and geographic and ecological approaches to community study. Chapter xxvii, on field trips, is excellent in spite of its brevity. This unit also discusses, again briefly, mass media as aids to instruction and as data to be pondered for themselves. The concluding section brings the book full circle, back to democratic character. It is treated now under four "disciplines"—imagination, precision, appreciation, and synthesis.

At times the author speaks to students: read this, consider that, place yourself in the way of learning. Stress is put on the wise sayings of great men through all time rather than on the steady advance of empirical science; in fact, there are weighty omissions here, for example, the community's power system. The great strength of the book lies in the writer's own humanism, his tremendous involvement with all mankind as it seeks to live and learn and improve its lot. I strongly recommend the book as a social studies text.

LLOYD ALLEN COOK

Wayne State University

Society and Education. By ROBERT J. HAVIGHURST and BERNICE L. NEUGARTEN. Boston: Allyn & Bacon, Inc., 1957. Pp. xv+465. \$5.75.

A book designated as a text for courses in educational sociology can scarcely be said to enter the lists under the most exciting auspices. The reader can usually look forward only to a further watering-down of the already aqueous introductory sociology. However, the reader of this book is in for a most pleasant surprise. Here is a book which, save in certain respects to be noted below, is head and shoulders above the ordinary sociology text and, in fact, constitutes an interesting and important contribution to our understanding of American society.

Basically, this is a study of the American social structure and how we learn to take our places—or find our paths—within it. While the school and the teacher come in for special attention, neither they nor the larger world are distorted or prettified by that confused optimism which some educators and sociologists mistake for philosophic breadth.

In the first seven chapters the authors describe the system of stratification in the United States and the process of socialization in the family, peer group, and community. Chapters viii–xv, on the school, deal partly with types of schools and colleges as channels of mobility and partly with educational policy and social issues. Chapters xvi–xix constitute the final section, on the teacher, his origins and his roles in the classroom and the community. The authors have managed to cover almost all the matters which a sociologist might wish for and at the same time to develop them coherently from a central set of concerns without resorting to a mere compilation of studies taped together with transparent generalities. Warner's conception of social class guides the presentation and interpretation of much of the material, with, fortunately, little trace of the dogmatism, the coyness, or the avoidance of economic realities which have marred some other works in this intellectual tradition. The many case histories are for the most part truly illustrative. Questions, exercises, and annotated suggestions for further reading follow most chapters.

To the above encomiums certain not insignificant exceptions must be made. A number of the chapters (e.g., chaps. vii–ix, xiv, xv) depart from the high standards maintained in most of the volume and adopt practices which have discouraged so many students of education: the genre sentimentality over sheer humanity, after the fashion of *Look* magazine,

and the incontinent attempt to deal with substantive issues which might arise in classroom discussions—an effort which arises from the seductive notion that since issues (such as international affairs) involve attitudes and since attitudes are learned and since education studies learning, it is therefore the proper task of an education text to debate problems of world amity.

On the whole, however, this book deals with very central material and should prove most worthwhile for students in education and in sociology, even though they be indentured to a standard text. They will find that, with the exceptions noted above, discussions of both the familiar and the more special problems are freshly and persuasively presented and contain an unusual amount of relevant research data and intelligent argument. For these reasons, *Society and Education* can be a valuable and exemplary source work for teacher as well as student.

MICHAEL S. OLMSTED

Smith College

The Origins and Prehistory of Language. By G. RÉVÉSZ. Translated from the German by J. BUTLER. New York: Philosophical Library, Inc., 1956. Pp. viii+240. \$7.50.

In this book, originally published in 1954, the late distinguished Dutch psychologist from Amsterdam University presents his general theoretical position concerning the origin and nature of language. The title of the work is misleading: the emphasis is more on logical than on chronological development. The author's "contact theory" is presented persuasively and systematically after a fairly extended survey and criticism of earlier theories. The study is sociologically oriented, and, as it is generally in line with the view of language taught in most sociology departments in this country, the arguments and ideas are likely to seem familiar and lacking in originality. The book is more important for the sound general introduction which it provides to the literature and issues than for its specific theory.

The author contends that language must be interpreted functionally. He finds its motivational source in a biologically rooted need for contact which expresses itself in prelinguistic forms of communication among the lower animals. Contact may be physical, emotional, or

intellectual, and the last-named, requiring the use of language, does not take place among animals. Révész contends that psychologists who credit lower animals with language fail to distinguish between the various forms of communication, of which language is only one, albeit a very special one. He argues forcibly that there is no evidence to justify attributing genuine speech behavior to any infrahuman organism.

The "cry" and the "call" are identified as prelinguistic communication devices characteristic of the lower animals. They are described as expressions of desire or need and are distinguished by the fact that the first is directed to a group and the latter to a specific individual. The "call," according to the author, is a "signal" rather than a "symbol" and hence does not lead to intellectual exchange of the type which occurs in conversation. Thus lower animals and human beings may signal each other, but no symbolic exchange can take place between them.

The functions of language in order of logical priority are given as the imperative, the indicative, and the interrogative. The first may occur on the prelinguistic level, while the other two require the use of language.

ALFRED R. LINDESMITH

Indiana University

Exploring English Character: A Study of the Morals and Behavior of the English People. By GEOFFREY GORER. New York: Criterion Books, 1955. Pp. vii+483. \$8.50.

Gorer has attempted to analyze English character by means of a questionnaire on such topics as class, living arrangements, friends, neighbors, social life, shyness, love, sex, marriage, child-raising, law and order, religion, superstition, and ideals of personality. The intent of the many questions is to illuminate his hypotheses about English character, which postulate the following as central: orderliness and gentleness—the renunciation of overt aggression; shyness, isolation, and loneliness; and high morality, high ideals, and strong controls. At the same time, the intention is at least to touch on the basic institutions common to all societies (p. 318), in the tradition of comparative anthropology. This questionnaire was administered by mail to volunteers from among the

readers of the *People*, the Sunday paper of second largest circulation in Great Britain. The idea was ingenious, and the response to requests for volunteers impressive: over 14,000 originally volunteered, and some 72 per cent of them returned completed questionnaires, of which the analysis is based on a subsample of 5,000. In addition, Gorer obtained the British Gallup Poll's results for about a third of his questions, as a check on the representativeness of his self-selected sample of readers of the *People*.

Whatever may be said for the original conception, ineptitude is the only word for its execution. The wording of questions, the coding of answers, and the analysis and interpretation of the quantitative results are incredibly amateurish. Despite a claimed pretest, any number of equivocal questions were asked, and even more equivocal answers are presented and analyzed at length. What, for instance, does it mean that three-fourths say "No" to the question, "Do you think English people fall in love the way you see Americans doing it in the films?" Or, even worse, after it becomes clear from qualitative answers that the question, "Not counting marriage, have you ever had a real love affair?" means very different things to different people—ranging, roughly, from sex without love to love without sex—no attempt is made to code anything more than the ambiguous "Yes-No," and seven pages are given to substantive discussion of such dubious results. Over and over again, the richness of the copious illustrative material is entirely lost in the process of coding: either these quotations are not typical of the material available, or the codes constructed lagged far behind the data. Gorer remarks that the process of developing a code "which will fit, without too much ambiguity, a great variety of answers, calls for methodological ingenuity" (p. 314). Like almost every one of Gorer's passing remarks on survey techniques, this is not quite right: ingenuity is called for only if something more than the very least common denominator of the data is to be unambiguously captured in the codes, and this kind of ingenuity is nowhere apparent.

Much as was lost in the transition from questionnaires to punch cards, there is still a good deal more in the data than Gorer ever salvages. Most of his "analysis" consists of tabulating every item in the mail questionnaire and every item in the Gallup field survey by seven demo-

graphic variables: geographical area, town size, age, sex, marital status, economic level, and social class. In fact, the 149 pages of tables which go to make up the "complete edition with tables," for which the publisher charges several dollars more, consist entirely of this complete, routine, and not very meaningful approach to tabulation. It apparently does not occur to the author to hold more than one variable constant at a time, just as he appears to be entirely innocent of the subtler techniques by means of which indexes corresponding more closely to the psychological concepts he wishes to examine might have been created. At the same time, his handling of even the simple data he does present is frequently inaccurate. To give just two instances: at one point he says, "There is however the interesting correlation that those who consider sexual love 'very important' in marriage are much more likely to consider terminating the marriage if the spouse is discovered to be unfaithful than those who consider it 'fairly important'" (p. 145). In fact, 31 per cent of those who consider sexual love "very important" think of divorce or separation as the immediate solution to marital infidelity, while the figures continue as 25 per cent of the "fairly important's" ("much more likely"?), 34 per cent of the "not very important's," and 47 per cent of the "not important at all's." So the relationship is actually in the opposite direction from that which Gorer suggests and raises quite different questions. Then, in another context, Gorer remarks, "Something like four-fifths of the English population read more than one horoscope weekly" (p. 268). This statement may describe his sample of readers of the *People*, which carries a weekly astrology feature, but it is not an accurate depiction of the English people. The Gallup survey figures indicate that only 42 per cent read even one horoscope feature regularly, but, even so, Gorer concludes, "The figures from the field survey [Gallup] are strictly comparable" (p. 277). These are only two of many instances.

The upshot is that although many of the data are provocative, the total impression is much like that of any large compilation of poll results: some ideas will occur to every reader, interesting suggestions pop up here and there, but the outcome is always indigestion and frustration. Data are never brought sharply to bear on the underlying concepts or to the point needed to clarify the variables influencing the reported answers.

It is interesting to speculate on what this study might have been if Gorer had teamed his undoubted insight and interpretive skill with someone for whom quantitative research is a more familiar and congenial mode of intellectual activity. But, as the study was done, it can serve only as a self-fulfilling prophecy. Social psychologists who are antipathetic to quantitative research will find here proof positive of its emptiness, while their opposites will see it as quite another kind of horrible example.

SHIRLEY A. STAR

*National Opinion Research Center
University of Chicago*

The Labour Government and British Industry, 1945-1951. By A. A. ROGOW, with the assistance of PETER SHORE. Ithaca, N.Y.: Cornell University Press, 1955. Pp. viii+196.

This brief study is an attempt to evaluate the work of the British Labour government from 1945 to 1951. It is, however, limited in two ways: it is restricted to the relationship between the government and industry, and its emphasis is on politics rather than on economics. Indeed, the main thesis of the author is that what was at issue was power rather than property, control rather than dividends. Thus there is now systematic discussion of the results of nationalization as regards output, efficiency, industrial relations, and so on. Instead, nationalization is presented primarily from the angle of the conflict between labor and private enterprise; the main issues are the struggles for the nationalization of sugar and steel.

This is, of course, a perfectly proper and permissible way of studying the problem, and within these confines the author has done a remarkably good job, with the added advantage of pleasant writing. There are chapters on planning (which might have gained by a stricter definition of this vague term), controlling industry, the organization of private industry, industry and labor, taxation, public relations (containing some excellent observations), and the impact of nationalization. The author's main conclusions are presented under the title "The Politics of Stalemate." The brevity of the book—it is brief in comparison, say, with Brady's *Crisis in Britain*—reduces the evidence often to a few scattered quotations. Its main value therefore consists less in its scope

or depth than in the insights of the author. Thus he refers repeatedly to the limitations on Labour's policy set by the movement's lack of qualified personnel. Yet, he adds, no steps were taken to train personnel; nor is it certain that the Labour party was always aware of the problem. Pertinent remarks are made about the failure of the movement to demonstrate to the public how the concrete recommendations of the party program were related to the country's needs. The American reader will find some useful—and sometimes, perhaps, startling—documentation of the extent to which nationalization was accepted and expected by conservative opinion in Britain.

In the concluding chapter Rogow discusses the prospects of further social change in Britain. He does not believe that evidence supports Harold Laski's fear of revolutionary resistance to democratic socialist progress. Refusal of business to co-operate, counterpropaganda, attempts at dividing the unions from the party, seem to have been the preferred and sometimes effective weapons of industry. Generally speaking, British sophisticated conservatives—to use C. Wright Mills's expression—seem to have followed the example of their American brethren and managed to adjust to the welfare state quite easily. The various bureaucracies—business, state, unions—form a common managerial front. Where compromises are required, business has abandoned material values rather than control, to the point where disadvantaged shareholders formed an association to defend their interests against management as much as against the Labour government.

A similar process of accommodation has been occurring on the side of labor. Full employment, progressive income taxes, social security, and the establishment of a mixed economy provide satisfactions to the unionized worker that leave little impetus for further advances toward socialism. Only a profound theoretical critique, the author feels, can overcome the paralysis of the Left which he deplors. Considering, however, that considerable parts of British management have not yet accepted the views and strategies of their more sophisticated colleagues and that the British economy seems to have difficulties in providing the material basis for the welfare state, a long period may be required to complete the necessary adjustments. To rush forward may well cause a crisis that might endanger what has so far been achieved.

As a short, well-written, and thoughtful introduction to an important aspect of British Labour policies, this book is worth reading.

ADOLF STURMTHAL

Roosevelt University

Higher Civil Servants in Britain from 1870 to the Present Day. By R. K. KELSALL. New York: Grove Press; London: Routledge & Kegan Paul, Ltd., 1955. Pp. xvi+233. \$6.00.

More than a hundred years ago Gladstone's government adopted the Northcote-Trevelyan recommendations on the reorganization of the government service in Britain and appointed the first civil service commission. The gist of these recommendations was that patronage ought to be replaced by open competitive examinations and that promotion should be by merit and not mere seniority.

Like most reforms, this one had unanticipated consequences, notably the erection of almost impassable barriers between what came to be called the "higher civil service," staffed by university graduates from upper- and upper-middle-class families, and the rank and file of government clerks. Throughout the late nineteenth century, every attempted improvement in the system seems to have been adroitly twisted into a further reinforcement for the barrier. In the present century the bars have been let down, but certainly not all the way.

The author's admitted bias in favor of democratic recruitment does not blind him to the arguments on the other side, but he notes with satisfaction that nobody dares to uphold the old arguments in public any more. The procedures have not always kept pace with public opinion. The longest step backward was taken after World War I, when a casual but discriminatory interview was made part of the entrance examination for the higher division. Kelsall is not satisfied to assert on probable evidence that this procedure was discriminatory. He demonstrates it by comparing the relative performance of candidates from different social strata on the written and oral sections of the examination. He also analyzes their subsequent careers.

The difficulties of gathering background and subsequent career information on candidates who appeared for examinations a generation ago were fabulous. The author devotes a chap-

ter to the problems he encountered in assembling the basic data. It is a saga of sociological zeal. The results justify his effort, although one could wish that they had been set in a somewhat broader frame of reference.

THEODORE CAPLOW

University of Minnesota

Soziale Verflechtungen in der Bundesrepublik: Elemente der sozialen Teilnahme in Kirche, Politik, Organisationen und Freizeit ("Social Integration in West Germany: Elements in the Social Participation in Church, Politics, Organizations and Leisure"). By ERICH REIGROTZKI. ("Schriftenreihe des UNESCO-Instituts für Sozialwissenschaften Köln.") Tübingen: J. C. B. Mohr (Paul Siebeck), 1956. Pp. xi+302. DM. 19.80 (paper); 23.40 (cloth).

This is the second volume in a series of studies published by UNESCO's Social Science Institute at Cologne, in which the author, a sociologist on the staff of the Institute, reports the findings of a large-scale field survey undertaken in West Germany in the summer of 1953. A random sample of some 3,200 adults of both sexes were interviewed about leisure activities and participation in voluntary organizations. The questions also covered religious affiliation and churchgoing, as well as political affiliation and opinions.

To the American reader this book is interesting primarily as evidence of the far-reaching "Americanization" of contemporary German sociology; it mirrors in drastic fashion both the strength and the weakness of what used to be considered typically American empiricism. Methodologically, the study is highly sophisticated. The sample has been selected with meticulous care and appears representative of the West German population. The interview schedule, which is reproduced in full, consists of sixty-four well-constructed and carefully arranged questions. The analysis of the answers is thorough and the interpretation excellent. No efforts have been spared in the presentation of the findings: the book abounds in cross-tabulations and diagrams.

Yet the result is thoroughly unsatisfactory because it never becomes clear what the findings really mean. Presumably this is an investigation of social integration or at least of so-

ciability, but at no point is any problem clearly stated. The author is uneasily aware that his research bears no relation to any systematic theory and defends his procedure as a preliminary step designed to develop basic data for further investigations. But since he has neglected to formulate any explicit hypotheses which his questions could have tested, his findings, many of them intrinsically interesting, remain hanging in the air in a most tantalizing and frustrating fashion.

It is indeed good to know that German sociologists are skilful in handling research tools; but it is to be hoped that examples of raw empiricism like this will soon be supplanted by research related to, and solidly guided by, some of the theoretical insight for which German sociology has long been famous.

KURT B. MAYER

Brown University

The Negro Population of Chicago: A Study of Residential Succession. By OTIS DUDLEY DUNCAN and BEVERLY DUNCAN. Chicago: University of Chicago Press, 1957. Pp. xxiv + 367. \$6.00.

Chicago is the modern world's most fully documented urban community. While some may feel that an undue amount of attention has been concentrated on a single place, there is no gainsaying the fact that accumulated knowledge both facilitates and enhances the value of subsequent contributions. This book is a case in point. It draws heavily on past research in Chicago, and it derives added value from its part in extending a long continuity in research. It has independent merit, too, of no small consequence: it is perhaps the most technically competent and penetrating examination of the concomitants of changes in racial occupancy that has been made.

The course of Negro population growth and distribution in Chicago is followed through census data from 1910 to 1950. Attention is given to the relative contributions of natural increase and of net immigration to growth and to accompanying changes in composition. Despite an extraordinarily rapid growth, the pattern of Negro settlement did not change radically during the forty years: rather, the process was one of filling in a pattern outlined by earlier Negro invasions.

The main burden of the study is the incidence of change in different kinds of residential areas during the latest intercensal decade. This part of the book is based on the concept of succession, which, although it has rather dubious analogical foundations, becomes in the hands of the authors a very useful analytical tool. The total of 154 census tracts which had 250 or more non-white residents in 1940 are sorted into five stages of succession: invasion, early consolidation, consolidation, late consolidation, and piling-up. With distance from the city's center and sector location controlled, the demographic and housing characteristics of each class of tracts in 1940 and in 1950 are subjected to regression analysis. The main findings are that no particular type of change is peculiar to any stage of succession and that the consequences of succession may be inferred not from the direction but from the amount of population change.

The succession analysis is pressed further to explore the effects of change in the differentiation of residential areas. Two hypotheses are put forth to account for the pattern of differentiation: one of a gradient running through the five classes of succession, the other of a gradient by distance from the city's center. The hypothesis of an interclass gradient is not substantiated. But evidence in support of a distance gradient is fairly clear. In fact, the pattern of variation of social-economic characteristics of the non-white population, by distance zone, in 1950, was not substantially different from that in 1920.

This book is unquestionably necessary reading for all students of race and race relations. More than that, however, it is highly instructive as a study of population distribution and for its use of method, and its technical appendixes are especially interesting.

AMOS H. HAWLEY

University of Michigan

Political Tendencies in Louisiana, 1812-1952. By PERRY H. HOWARD. ("Louisiana State University Studies, Social Science Series," No. 5.) Baton Rouge: Louisiana State University Press, 1957. Pp. xix + 231. \$3.75.

The basic theme of this study is that the political history of Louisiana may be explained by the conflict of rational interests, mainly

economic, between the various segments of its population. The method consists primarily of an examination of voting by parish in state and national elections and correlation with the economic and demographic characteristics of the population. Its framework and method identify it as a study in political ecology.

The author identifies as a major theme the antagonism between the plantation merchant and the family farmer; as minor themes, sugar interests versus cotton interests, Catholic versus Protestant, French versus American, and so on. Little evidence is given that election returns actually reflect the sociocultural characteristics or interests of the voters. Much of the analysis is little more than rationalization of deviations from patterns to be expected in consequence of the author's frame of reference.

State and national issues in each election, as well as party platforms, etc., are more often than not ignored—the election of 1860, for example, is treated primarily as a private fight for power between two Democratic party leaders rather than as the reflection of the breakdown within the national Democratic party on the same issues—Union versus slavery. His analysis assumes that had Slidell and Soulé avoided this showdown, Louisiana might never have joined the Confederacy. Much of the historical material is treated superficially and with evident bias. Huey Long and his movement are seen simply as a continuation of populism and Jacksonian Democracy—a victory for the farmers over the Bourbon elite.

The extensive time covered, one hundred and forty years, would reasonably require the tracing of the economic and demographic factors through time for purposes of analysis. Are we to assume, for example, that the economic and social organization in Louisiana of 1950 is identical with that existing during the entire history of the state? Yet economic statistics from the 1950 Census are the only data of this type offered. Demographic factors are at the core of this analysis, yet data on the composition of the population, e.g., by color, are presented only for 1950, although census data are readily available for many prior years. A heavy in-migration is noted, particularly to the Florida parishes, but nowhere does the author attempt to estimate this movement quantitatively especially in relation to the size of the vote. When a parish previously identified as plantation or family farm changes its vote, we are told that the explanation lies in population

shifts, although no evidence is presented. The influences of urbanization and industrial growth are likewise cavalierly treated.

Poor style and loose formulations in abundance add little to the value of this study. As one example: on page 22 the author states, "Plantation economy establishes social relationships similar to those which exist where big business prevails." (!). He makes little attempt to assist the reader, who may be unfamiliar with the geography of Louisiana, to keep in mind the distinctive characteristics of the sixty-four parishes or the regional groupings of the parishes. The relief and topographic maps intended to explain them are most unclear. His main conclusion appears to be that "Louisiana politics . . . can be made rationally understandable, but only against the background of historical circumstances which helped make the present what it is" (p. 154). The study does little to achieve this purpose.

FRANCES AND MONROE LERNER

New Rochelle, New York

Er ist wie du: Aus der Fruengeschichte des Antisemitismus in Deutschland (1815-1850) ("He Is Like You: From the Early History of Anti-Semitism in Germany"). By ELEONORE STERLING. Munich: Chr. Kaiser Verlag, 1956. Pp. 235.

The author, a young American scholar, went to Germany to study the mentality that permitted the persecutions and finally the destruction of German Jewry. But the enormity of the crimes committed under national socialism appeared to her beyond the scope of even the most cautious and objective analysis; instead, she chose to investigate the formative period of violent German anti-Semitism, the first half of the nineteenth century. The data she unearthed convinced her that her original plan of proceeding as a sociologist and historian would not do justice to the task; they led her deeply into theology and religious motivation.

Sterling's thesis is that modern German anti-Semitism, while secular in appearance, was rooted in and evolved with the disintegration of Christian belief in nineteenth-century Germany. At the bottom of Christian hostility toward the Jews she sees "unresolved" conflicts between Jewish and Christian doctrine, particularly Jewish monotheism and Christian

trinity. The Jews' rejection of Christ set them apart in the realm of religion, and the religious isolation, at once imposed and desired, led inevitably to social and cultural differences as well. So long as the antagonism was conceived of as essentially theological, it was also tempered by theology. The God of the Jews and the Christians is the same God; at the core of Christian morality is the love of man; and Christian doctrine assigns to the Jews a special place in God's plan. These moderating conditions of the conflict gave way as German Christian belief broke up under the impact of Enlightenment and industrialization. Social, economic, and political accusations against the Jews now took the place of the religious ones, and "emancipated" Jew-baiting began.

The study consists of four parts. The first outlines the changes which the rise of the new society brought about in German and Jewish history; the second deals with the theological and secular image of the Jews as it developed in the train of German resistance to the Enlightenment; the third traces the shift of theological concepts into the spheres of social and political theories; the last describes the concrete consequences of secularized anti-Semitism for the course of German political history.

In general, Sterling's proposition is not new, but new is the truly extraordinary wealth of documentation with which she presents it. The original source materials are taken from more than a hundred different archives, newspapers, and periodicals of the decades reviewed; the bibliography runs over more than forty tightly printed pages. The data are skillfully used for the elaboration of a typology relating specific distortions of Christian belief to specific ideologies characteristic of German social and political groupings. Sterling, for instance, convincingly traces the later Nazi myth of Aryan and Jewish blood to the paganized dogma of Christ's blood sacrifice.

This is a scholarly and exciting book. In some respects it is also frustrating. The author has a penchant for religious preaching; the presentation constantly moves between exhortation and analysis, between appeals to true Christianity and sociological explanations of why the new Germany was not a good place for raising true Christians. Nor is it quite clear how the "unresolved" theological problems that separate Judaism and Christianity could be solved theologically. An essential part of Sterling's argument is that the belief in Christ as

the Savior imposes a special strain on the Christian: he must reconcile the doctrine of the world having been delivered from evil with the evil world he lives in. The incongruity between doctrine and reality, particularly in times of social disorganization, engenders hostility again the group that does not believe what the Christians dare not *not* believe. "The hatred of the Jews manifests itself as the religious transposition of the hatred of men of themselves and the general condition of their miserable existence," Sterling writes; and again: "What the people did to the Jews was their attempt, impotent and only dimly realized, to do away with their own suffering. The concentration camp was to conceal what the world itself had threatened to become." Here are unmistakable echoes of Horkheimer and Adorno's *Dialektik der Aufklärung*, but their analysis of the blind destructive fury of modern anti-Semitism flows from a theory of modern society of which there is hardly a trace in Sterling's study.

PAUL W. MASSING

Rutgers University

Some Potentialities of Experimental Jurisprudence as a New Branch of Social Science. By FREDERICK K. BEUTEL. Lincoln: University of Nebraska Press, 1957. Pp. xvi+440. \$6.00.

If you are no longer enchanted with the potentialities of new branches of the social sciences, you may still be interested in the author's 155-page study of the problem of bad checks. This study illustrates an orientation toward legal research and drafting which the author chooses to call "experimental jurisprudence."

To practice experimental jurisprudence, one is encouraged to isolate and state carefully the social problems to which a specific law is directed. The author warns: little steps for little feet! Tree-planting ordinances, the sanitation of barber's tools, tobacco sales laws—these, according to the author's experience, offer greater immediate promise than delinquency and areas of major institutional confusion. The law in question must be accurately stated, and the high art of the experimental jurist is the exact description of the effect on society of adopting a particular rule. The author suggests that a hypothesis be constructed to explain society's reaction to a rule, and, if possible, this hypothesis should be placed in a wider theory

which would allow prediction of what would happen upon application of similar regulatory laws to similar problems. If the law is "inefficient"—this is a primitive term in the system—new laws may be enacted and the process repeated.

Illustrating this paradigm with the problem of bad checks, Beutel reports that only one check in two hundred "bounces," and, of these, only 10 per cent are "forgeries" or "no-account" checks. Even in those establishments where the losses are highest—gas stations and bars—the loss is less than \$50 per year per establishment. Less than \$500,000 in check losses is incurred each year in Nebraska, and the losses involved in court actions total less than \$100,000; yet—and this is the shocker—almost \$400,000 is spent detaining writers of bad checks in the penitentiary!

Complications do not end here. The problem of collections which stop short of criminal actions involve the sheriff, who, as a favor to the business community, acts as a collection agent, when theoretically he should be solely engaged in the prosecution of crime. When he accepts payment and dismisses a charge for forged, no-account, or insufficient-fund checks (over \$35 in Nebraska), he is close to compounding a felony; when he effects collection by threat of criminal action, he is close to blackmail.

To these complications Beutel adds the pragmatic finding that collection is most efficient in counties where the sheriff is most active. The author looks at enforcement in other states where passing insufficient-fund checks with intention to defraud is a felony rather than a misdemeanor, and in the process he adds a confirming bit of data to the oft-repeated assertion that severity of punishment, beyond a point, is more a handicap to enforcement than a deterrent to crime. While he did not write out a possible enactment of his model check statute so that a follow-up study might be included, it is only in this detail that the check investigation falls short of illustrating the proposed method.

To chide the law-professor author gently for the pretensions of his "new branch" of social science, one should require him to reconcile his indignation over the court's failure to use scientific evidence in the Chaplin case (pp. 22, 169) with his admiring quote from Lundberg's *Can Science Save Us?* (p. 28). The author could have called upon his sociological colleagues of

the "old branch" to learn the criticisms of the culture-lag hypothesis he discusses in chapter v, and, by reference to modern discussions of Mill's methods of proof, like Churchman's, he could have made more explicit the conditions under which experimental jurisprudence as he visualizes it could be expected to provide valid knowledge. In fairness it must be added that one closes the book with great respect for what may be learned from careful study of the enforcement of a single law but with some doubt that experimental jurisprudence should now be regarded as a new branch of the social sciences.

FRED L. STRODTBECK

University of Chicago

The Values of Veblen: A Critical Appraisal. By BERNARD ROSENBERG. With a Foreword by MAX LERNER. Washington, D.C.: Public Affairs Press, 1956. Pp. vii+127. \$2.50.

At one point in this book Rosenberg correctly observes that it is difficult to write about Veblen with cool detachment and nice objectivity and that much of the literature on him is tendentious. He adds, however, that moderation itself is a feeble alternative. The main trouble with this highly literate study is that when he gets down to his critique of Veblen's ideas, Rosenberg himself—despite his erudite writing—is moderate, unoriginal, and sometimes pedestrian. At this late date, and considering the work of previous Veblen students, we hardly need to be reminded, for example, that Veblen romanticized primitive life, ignored the "larger" function of religion, bore down hard on businessmen; or that he was, in a real sense, writing about himself in his essay on "The Intellectual Pre-eminence of Jews in Modern Europe."

When, on the other hand, Rosenberg permits Veblen's ideas to stimulate his own thinking on such matters as higher education and the methodology and content of American social science, he gives to his study sufficient bite and invective to make it spiritually, if not analytically, worthy of its subject.

In little more than a hundred pages, Rosenberg endeavors to summarize and dissect six of Veblen's major works, his methodology, his uses (and misuses) of psychology and anthropology, and his intellectual kinship with such varied scholars as Marx, Freud, Weber, Lewin,

and Burnham. Along the way he aims occasional barbs at the behavioral disciplines and is provocatively irreverent about more than a few elder statesmen of American social science—among them, Elton Mayo, Logan Wilson, and Talcott Parsons, “the majordomo of American sociology.”

Since so much is attempted, it is perhaps not surprising that the book reads with considerable unevenness, and that there is not much logic or organization in the assembling of the nine short chapters. Even more disconcerting, Rosenberg’s almost constant display of his own higher learning frequently reaches the pedantic and irrelevant. And despite the author’s patent sophistication, one is now and again taken aback by seeming naïveté—as when, apparently with a straight face, he “counters” Veblen’s caustic view of sports by quoting Frederic Thrasher to the effect that athletics provided Chicago gangs with wholesome experience and escape from ennui. One can only be eternally thankful that Veblen was never chastened by exposure to any of these salubrious juvenile activities during his Chicago days!

No doubt many theoreticians will object to Rosenberg’s elevation of Veblen to a par with Marx and Weber (chap. v, “Three Titans”); yet his effort to trace the kinship of their ideas—especially those of Veblen and Marx—is stimulating, and becomes, at last, the central concern of this study. Portraying Veblen as an intellectual offspring of Marx, Rosenberg carefully defends the position that Veblen was, nevertheless, too realistic and perhaps too cantankerous ever to succumb to the father’s discipline of essential hopefulness. And “insofar as Veblen escapes the Romanticism of Marx, he ascends to a correspondingly higher rank as thinker and soothsayer.” On the other hand, Rosenberg’s contrast of Veblen with Weber is pretty cursory. It may perhaps be arguable that Veblen was the first important social scientist to deny that the world must choose between Marxist socialism and free-enterprise capitalism and to discern the new class of bureaucratic collectivists. But to maintain that the bureaucrat is the main character in Veblen’s work is to distort Veblen’s thesis and his prescience.

In a crucial sense, of course, the main character in Veblen’s work was always *Veblen*—doggedly refusing to abandon hard intellectual integrity for “the essential yes.” Rosenberg himself has quite enough of this rare quality to make his book worth reading. What makes it,

at the same time, disappointing is that he too frequently discards his own imaginative disaffection in favor of a mere show of intellectual virtuosity.

ROBERT O. SCHULZE

Brown University

Women’s Two Roles: Home and Work. By ALVA MYRDAL and VIOLA KLEIN. New York: Humanities Press, 1956. Pp. xiii+208. \$5.00.

This addition to the recent books on the “woman’s problem” is a well-reasoned, direct, and unequivocal argument in favor of one particular solution. While the book occasionally reflects a sensitiveness to the subtler social-psychological aspects of its subject, its strength lies rather in delineating the broader outlines of the problem.

The solution advocated is that of gainful employment of married women during the major part of their lives. “Under present conditions,” write the authors, “with an average family of only slightly more than two children, and reasonable amenities, an average housewife can be considered to be employed full-time on tasks which are necessary for home-making only during a quarter to one-third of her normal adult life.” Making a number of assumptions—that the families of the future will average three children, that the interval between children will be two years, that continued full-time supervision by the mother is required until the youngest child reaches the age of nine or at the most fifteen—the authors claim that the average woman in her early forties can and, indeed, *should* resume gainful employment. The authors fear that overemphasis on part-time rather than full-time jobs as the solution will sidetrack women into a blind alley: part-time jobs will continue to be the exception rather than the rule in our economy. Furthermore, unless women are regarded as regular workers, social measures facilitating the combination of familial and economic functions are not likely to be developed.

Another remedy usually proposed for the problems of “ex-mothers,” that of civic and cultural volunteer activities, is not favored by the authors. “Our modern economy cannot afford nor can our democratic ideology tolerate the existence of a large section of the population living by the efforts of others.” A fairer distribution of work and leisure among men and

women in the middle years of life should result in a much shorter working day and more leisure for men also. That, in turn, may lead to the renaissance of family life and a rapprochement between the separate worlds of husbands and wives.

The most interesting and new data will be found in the chapter on the economic status of women in four Western countries. The emerging similarities are as interesting as are the differences. In each of the four, women constitute about one-third of the labor force. Some division of employment along sex lines, and more specifically along traditional sex lines, is to be found everywhere, but the divergences are intriguing: for example, in France and Sweden, in contrast to the United States, pharmacology and dentistry are largely feminine fields; medicine attracts more women in Britain than in the United States. The authors also note that there is no one-to-one correspondence between national ideologies as to women's roles and the extent of gainful employment. High rates of employment are sometimes found in countries which proclaim that a woman's place is in the home.

The authors deal with obstacles to the employment of married women, such as absenteeism and labor turnover, and with the effects on children. The latter issue is, of course, the critical one and the one shortest on reliable data and most charged with emotion. The authors take the view that up to the age of three the child needs a continuous relationship with the mother. Between the ages of three and six the mother could absent herself for work during the hours of nursery school or kindergarten with no harm to the child. Then, as the child begins school, "its interests are no longer incompatible with the mother's desire to do other things," provided that school lunches and other arrangements are available.

The nature of these needed measures is discussed in the chapter on "The Next Steps." They fall into three classes: individual adjustments, adjustments in the labor market, and social adjustments. As to the first, the authors recommend that women choose occupations which are compatible with family life and which can be easily resumed after the interval of child-rearing. To that extent the authors depart from the older feminist ideal of similar occupational patterns for men and women. The book urges that women themselves, women's organizations, and the community at large keep the vocational

spirit alive in married women during the child-rearing years by organizing training courses and programs to maintain skills and nurture vocational affiliations. The other measures recommended range from housing development planned for working women to keeping shops open at hours convenient to the working population.

With all its realism, the book somehow conveys an excessive optimism as to the rate of possible progress, at least in the United States. Despite recent increases in employment of older married women, the normative and organizational obstacles to universal salvation by work appear more numerous and resistant than the general tone of the book suggests. Veblen's status-symbol role of the wife will not easily be reversed into a social sanction *against* her economic dependence. The ideal of individual home ownership, the unabated elaboration of standards of homemaking, the difficulties in rationalizing housework, the comforts frequently enjoyed by the middle-class wife coupled with the scarcity of suitable and attractive jobs, the widespread legitimization of volunteer civic and cultural pursuits—these and other values and circumstances will continue to retard the recommended changes.

But if work is not the exclusive answer to women's dilemmas in the foreseeable future, it certainly is a major answer. The book therefore makes an important contribution by its well-argued case for reconciliation of women's two roles.

MIRRA KOMAROVSKY

Barnard College

Sociologie électorale de la Nièvre au XX^{me} siècle (1902-1951) ("Sociological Study of the Electorate in Nièvre in the Twentieth Century").

By JEAN PATAUT. With a preface by M. FRANÇOIS GOGUEL. 2 vols. Vol. II: *Cartes et graphiques*. Paris: Éditions Cujas, 1956. Pp. 330; 26.

In the manner established by André Siegfried and his disciples in election geography, M. Pataut gives a painstaking description and analysis of the geographic patterns of voting in a largely rural, though industrialized, region of central France. Mainly concerned with basic political tendencies rather than with parties, in accord with Siegfried's contention that French politics must be understood in terms of persist-

ent basic attitudes, Pataut operates with the conceptions of movement and order, or the progressive and the conservative tendencies. In more than a hundred diagrams he traces the changing patterns of party strength by cantons and the more stable partition of the region between the major tendencies.

Pataut describes first the geographic, demographic, and economic conditions of voting behavior and thereafter the roles of church, labor unions, and newspapers in the political life of the region. He then analyzes the elections in three periods: before the first World War, between the wars, and after liberation. In the last part he turns to the geographic distribution of the fundamental tendencies. This is followed by a discussion of the influence of the election system on voting. He comes to the conclusion that a change from majority voting to proportional representation did not affect the basic tendencies but tended to reduce the importance of local issues and local politicians (p. 187). Finally, Pataut discusses the significance of abstentions, distinguishing among inevitable abstentions (because of old age, sickness, etc.), those due to specific local conditions (sparse settlement, etc.), and those which are politically motivated—omitting what I believe is important: the abstentions of protest.

The region which the author has selected represents a difficult case for this kind of study: its physical and cultural geography does not permit an easy distinction of clear-cut subregions, nor does the geographic distribution of party voting strength form clearly distinct patterns. Furthermore, considerable changes in the structure of the economy and in the distribution of the electorate have occurred which had to be taken into account. Considering these handicaps, Pataut has done a diligent job.

In contrast to the regions studied by André Siegfried, the Catholic church has little political power here except in one canton; and it is around this canton (Brinon) that a small hard core of conservative territory can be discerned in a relatively isolated area where middle-sized farms prevail. The *département* as a whole is thus left-oriented. It is interesting that the socialists in the years before the first World War had their main support not in cities but in areas where the lumber industry had created a semi-proletariat of part-time farmers; here, ten years earlier, Boulanger had a strong following, and it was among these lumber workers that the trade-union movement first appeared in the region. Seen against this background, the success of the

Communist party (25 per cent of the registered voters) after liberation is not surprising! The Communists had the advantage of being not only the most leftist party but also the most patriotic because of their role in the resistance. They gained their main strength in almost the very territory where the socialists had their first strongholds. The MRP—the new Christian Democratic party—gained no more votes than the Communists and thus could not replace the old parties of order. In 1951 a threefold division of tendencies seemed likely, owing to the rise of Gaullisme, which could not be considered as a party of “order.”

The reliance on graphic presentation of correspondences between social phenomena has its disadvantages, particularly in regions like this one. The author could have strengthened his findings by testing some of his inferences by more refined statistical methods; it is quite possible that he would have found significant correlations in cases where the graphic method fails to give this impression. I also doubt whether the computation of party support in percentages of registered voters instead of total valid votes is defensible.

RUDOLF HEBERLE

Louisiana State University

Family and Neighbourhood: Two Studies in Oxford. By J. M. MOGEY. New York: Oxford University Press, 1956. Pp. ix+181. \$4.80.

One might classify this book as a study of the social effects of houses. The author contrasts the behavior and attitudes of two samples of the working class in Oxford (England), one aggregate living in St. Ebbe's, an obsolescent if not deteriorated area in the central zone of the university city, and the other in Barton, a low-cost housing estate about three miles out from the center of town started in 1920 and very much enlarged by the municipal authorities since 1946.

Mogey's monograph is packed with facts and ideas centered around firsthand observations, including “free” or “open” interviews. His two areas, apart from being roughly comparable in size (having about a thousand households each) and closely similar in the commonly experienced but important fact that their present tenants and inhabitants are not at all responsible for house design, street layout, or other geographic facts of life, are otherwise quite different in certain broader aspects of physical environment

and in certain ways of acting *en famille* and toward kinfolk, neighbors, friends, and more remote others such as might be encountered at work or in formal associations, including trade unions and churches.

Since 1920, Oxford has been changing from what Americans might call "a small college town," one with incidental farm-market functions, to one of the most prosperous, high-wage, industrial towns in Britain. An influx of "foreigners" from elsewhere in England and Wales has occurred, along with a "new rhythm of work," namely, year-round employment, since 1939 in the automobile factories, and Mogey's report suggests that a profound shift has modified the traditional dominance of gown over town. There are lessons in this experience for a number of American situations, given our current mode of industrial decentralization.

For the sociologist interested in methods of research, this monograph exemplifies the exploratory survey—something which may be distinguished from both the small-sample case study concentrating on selected social factors and the more extensive social survey. If case study is represented in English sociology by *Living in Towns* (edited by Leo Kuper and reviewed in this *Journal* in March, 1955) and the social survey by the long tradition headed by Charles Booth, then *Family and Neighbourhood* stands at the third, or highly strategic, point of the triangle as a "pilot" study: that kind of sociological inquiry which aims "to find out where the real problems lie in any particular area of social research in the belief that, as a consequence of collecting information in existing circumstances, it will be possible to formulate these problems in such a way that a research design can be developed and some of the rigours of the scientific method applied to social phenomena" (p. viii).

In the Foreword the distinguished chairman of the Oxford Pilot Social Survey Committee, Professor G. D. H. Cole, remarks that "much more knowledge than we have at present of the essentially subjective aspects of living in rapidly changing and developing urban communities is badly needed," and he cautiously adds that "the method of 'open interview' . . . is at any rate a necessary element in such studies" even though the results are doubly subjective, since they are influenced by the interests and attitudes of both parties to the interview. With such strictures, Cole expresses the belief, however, that "a good deal of objectively ascertained fact about subjective attitudes does

emerge" in this report and that it does illuminate problems of the new housing estates and how they differ from those of older and congested neighborhoods.

Mogey, university lecturer in sociology at Oxford and also author of *The Study of Geography* ("Home University Library," No. 214—touted on the publisher's dust jacket as dealing with "the ground plan of life"), points out, in the Preface, amply substantiated by a thread of argument which may be followed through a labyrinthine text, that "it is difficult to be purely factual and to use scientifically sound methods in social research" (p. viii). This statement may be regarded as an Oxford, or Ultra-Superior, Paradox, but the reviewer hopes it will be taken as a stimulus to read the book. Indeed, the work may serve teachers and encourage fellow workers in research by providing a model of the study report which concerns itself not only with small situations but also with the minor instance that makes the point. Along that line, see the discussion in chapter viii ("Status and Class") regarding working-class attitudes to be distinguished as "status-assenting" and "status-dis-senting," or the account in chapter iii ("The Methods of Research") of how an American Rhodes scholar tried to help an English instructor in a Workers' Educational Association course lead a discussion of class divisions in society: he was attacked by the students in such phrases as "That's America; it's not England" and "What you say doesn't hold for this country" (p. 47).

Mogey's visit to America in 1954 is possibly reflected in some of his interpretations and footnotes as well as in the title of chapter vii, "Work, the Fireside Stance," but his book does not attempt the cross-cultural comparisons it suggests to the American reader of, say, William F. Whyte's *Street Corner Society* (1955), the "Yankee City Series" by W. Lloyd Warner and others (1941—), or *Crestwood Heights* by John R. Seeley and others (1956). There is something of Mogey's Barton and St. Ebbe's in each of these, and something of these all in his Oxford.

BUFORD H. JUNKER

University of Pittsburgh

Intelligence in the United States: A Survey—with Conclusions for Manpower Utilization in Education and Employment. By JOHN B. MINER. New York: Springer Publishing Co., 1957. Pp. xii+180. \$4.25.

This is a most serious endeavor to dig deeper into the phenomenon of intelligence. The postulate of the author is not new—that native intelligence depends for its development upon motivation and available opportunities for learning and that the score attained on an intelligence test is a composite of the interacting results of different processes. There is the physical factor, the distracting factor, and the intelligence the individual brings into the testing situation. Miner's main contribution is that he forces us to re-examine concepts of intelligence and intelligence testing as they relate to the culture of the United States.

The author's basic theory is utilized in a nation-wide survey which includes a representative sample of the population of the United States. Fifteen hundred persons tested match this population of persons aged ten years and over. Intelligence scores are analyzed as to education, sex, mental status, age, race, occupation, geographical area, religion, class identification, and size of city. Through a carefully worked out formulation of models of the occupational structure, detailed estimates of the extent to which intellectual resources are being wasted or underused are presented. In addition, the volume contains suggestions of a "practical" nature for vocational guidance, personnel management, and education. Miner is careful to consider social class in considering differences of intelligence, age of retirement, adult education, difficulties of subject matter and ways, and means of obtaining an adequate work force.

There is also a carefully selected bibliography of the pertinent literature since 1940. The shortcomings are negligible, particularly when one grasps the significance of this contribution at a time when the validity of existing standards of intelligence and intelligence testing are being challenged.

ARTHUR LERNER

Los Angeles City College

Against the Tyrant: The Tradition and Theory of Tyrannicide. By OSCAR JÁSZI and JOHN D. LEWIS. Glencoe, Ill.: Free Press and the Falcon's Wing Press, 1957. Pp. ix+288. \$5.00.

History has a way of returning to issues and dilemmas which it had seemed to have long left behind, unilinear theorists of progress to the contrary, and the problem of tyranny and tyrannicide is an example.

To the Victorian thinker such problems had but antiquarian interest as the subject matter of learned disquisitions on the past history of political theories. But to us, who live in a world threatened by totalitarian dominance, these are problems as alive as they were during the French religious wars of the sixteenth century or in the days of the English revolution.

Professors Jászi and Lewis must hence be congratulated for having written a book which helps to focus attention on theories of tyrannicide since Plato and Aristotle as well as on its recent practice. While one welcomes the enterprise, one cannot avoid being critical of its execution. The first part of the book, written by Lewis, deals in rather conventional terms with the theories of tyrannicide developed in classical antiquity and in scholastic thought and traces them through the French pamphleteers of the religious wars and the monarchomachs to the ideologists of the English revolution. There is little here that is not available in textbooks on political theory. Hardly an effort is made to connect systematically the various theories with the social structure of their time, the audiences to which they were addressed, or the conditions of existence which gave rise to them. We have here a useful exposition of relevant theories, but a sociologist may be forgiven for feeling that an excellent opportunity has been missed.

Jászi, who examines the development of the tradition of tyrannicide in the nineteenth and twentieth centuries, is more aware of social history. He points to the symbolic significance of the concept of tyrannicide in the American and French revolutions and proceeds to a quite useful distinction between tyrannicide and political murder. Several sections deal with the uses and distortions of the symbol of tyrannicide in revolutionists and counterrevolutionaries in the nineteenth and twentieth centuries. The discussion of the German resistance movement during World War II is especially noteworthy. The discussion of the religious and moral justifications by which the conspirators against Hitler attempted to legitimize their projected action are of great interest. But Jászi's part of the book lacks organization. He seems never quite sure whether he intended writing a sociopolitical study or an ethical treatise on the justification of tyrannicide. His last sections, especially, suffer from a mixture of genres; there is a constant switch from the rhetoric of analysis to that of exhortation.

This book brings into focus a most-neglected topic and one, moreover, which should be most rewarding for the historical sociologist or the student of political sociology. We must be thankful to the authors for raising the problem, even though we may not be fully satisfied with the manner in which they have done so.

LEWIS A. COSER

Brandeis University

The World of Oneness. By BYRAM CAMPBELL. New York: Vantage Press, 1956. Pp. xv+240. \$3.50.

This is a tract against the times, against the "age of unreason." It is a kaleidoscope of short paragraphs dealing with the psychology of unity and the philosophy of oneness as well as the intellectual history of the West. Oneness, in this diagnosis, is tantamount to collectivism, and Campbell is against it. He is quite brief in what he is for—presumably pluralism and some sort of individual independence. Besides the usual villains of history, for him Roosevelt and UNESCO are particular embodiments of evil; he manages to stay with these longer than with his innumerable other topics. In the main, the book is poorly informed and relies with moving faith on haphazard news dispatches and many quotations, as though they settled things forever. In its opposition to monists, who also turn out to be people who are for equality and against discrimination, Campbell comes out squarely on the side of those who believe race prejudice to be "implanted in us by nature." In this way Sir Arthur Keith becomes one of his heroes, and social classes are described as involving significant differences in height, heads, and faces. In a section on science and society we are told: "Raymond Moley says: 'Economics, sociology, politics and psychology are still arts trying to look like sciences.' I might add that these arts give great scope to very poor artists."

At first thought one inevitably pushes this away with weariness at having to consider a document made up of so much error, opinion, and moral fervor masquerading as considered judgment. On second thought one should perhaps be pleased that *one* society facilitates such a book and hundreds of others which have dealt with similar issues and which come to different conclusions. Whatever of value is to be found in Campbell's meditations—be it valid statements of fact or imaginative proposals of moral

choice—has been said much better elsewhere. The existence of the book helps document the reality of our intellectual freedom; its content is a liability in the balance of qualities necessary for productive scientific or public debate.

KASPAR D. NAEGELE

University of British Columbia

Individuals, Groups, and Economic Behavior. By C. ADDISON HICKMAN and MANFORD H. KUHN. New York: Dryden Press, 1956. Pp. xvii+265. \$4.75.

In this very significant work an economist and a social psychologist undertake a fresh analysis of three popular economic problems: the effect of managerial motivation, the feasibility of making "interpersonal comparisons" of utility satisfaction, and the compatibility of economic planning and economic freedom.

Following a preface in which their rationale of interdisciplinary research and problem orientation is developed, they critically review in their first chapter the schools of social psychology represented in Freudian, field (Lewin), behavioristic learning, and self theories. The last is discussed as a derivative of symbolic interactionism, modified by concepts of the reference group, and is the doctrine to which the authors subscribe. The other schools are rejected because, they say, each violated general scientific principles, the assumptions and findings of the social sciences, or both (p. 45). Two chapters are devoted to managerial motivation and the corporation in its cultural setting, and one chapter each to interpersonal comparisons and to planning and freedom. The final chapter summarizes the research techniques considered to be applicable to the problems according to their version of self-theory. These turn out to be the familiar Thurstone, Likert, Guttman, and Berelson techniques. An ingenious new device for sociogramming is introduced. Thus behaviorism, rejected in theory, is retained in methodology.

The work further exhibits the crying need for recognition of the multiplicity of organic groups as the environment of the self that has been manifest in much recent writing in social psychology. It contains in fragmentary form some of the ablest theoretical discussion of the conceptual problems involved with self-group

relationships that I have seen outside ethnology. In common with economists and social psychologists generally, the authors seem to be incapable of seeing the problem of groups in any objectively organic perspective, although they come fairly close to it from time to time. In discussing differential motivation, for example, they point out that "there is a tremendous number of values that have efficacy only within relatively narrow sectors of the society. . . . The norms of the National Association of Manufacturers differ from those of the National Council of Churches of Christ, and both differ from those of the American Chemical Society or those of the Americans for Democratic Action" (p. 92). These particular organizations, they explain, belong to one type of group, and there are other identifiable types. Here we have named groups mentioned—and names are potent symbols—and recognition of some functional taxonomy of groups. But here, as elsewhere, this promising theoretical fragment is cut short by reverting immediately to a pious reassertion of the individualistic axioms of their doctrines, the conceptualism by which both economics and psychology are so rigorously culture-bound.

The authors blame the failure of social psychologists to deal adequately with the problem of planning versus freedom on the pervasive social definition of the problem in terms of conflict between the individual and society (pp. 201-2). They might have gone on to point out that all the social sciences are oriented by this conflict; that the metamorphosis of Western institutions from feudalism to capitalism involved innumerable instances and forms of individuating mass disaffection and institutional displacement of peoples; that these were resolved through conflict into institutional reorganizations represented in Protestantism, parliamentarianism, and individualistic, pecuniary business institutions.

In my opinion the conceptual orientation of social psychology, with its "preoccupation with the development of rigorous method" (p. 201), is that of rigorous adherence to culturally pervasive axioms of individualism most articulately elaborated in academic institutions, of which the authors are members. Content analysis, attitude scaling, and the other techniques for identifying, measuring, and comparing attitudes, interests, shared norms, and values are rigorously focused upon samplings of various masses of individuals as individuals. Social psychology still retains all the individualistic ab-

solutism of the "group mind fallacy" doctrine; yet the interconnection of minds through language is the basis of attitude testing. The behavioristic conceptual model of human society is focused on the individual "organism," a creature standing resplendent at the zenith of the phylogenetic continuum, a teleologic imagery that biologists of recent decades have been most embarrassed about.

It is an inescapable fact that social structures do not spring into being full blown. Moreover, rarely, if ever, do they remain static for appreciable lengths of time. The techniques of social analysis involving time are comprehended in historiography, while those adapted to the direct study of what Hughes calls "on-going groups" are substantially ethnological field methods. But these require enormous, careful, tedious, and painstaking pedestrian labor as well as uncommon skill and insight. Modern physical sciences have not achieved their successes by application of some small neat kit of chromium-plated statistical tools alone.

This book undoubtedly will be justly acclaimed as a thoughtful, carefully written, thought-provoking attempt at interdisciplinary research. The limitations it exhibits are not exclusively those of its authors, for they have seen substantial glimpses of light through the cracks in their dogma, an achievement beyond the ken of most of their colleagues.

WILLIAM C. LAWTON

University of Chicago

Rituals of Kinship among the Nyakyusa. By MONICA WILSON. New York: Oxford University Press, 1957. Pp. xi+278. \$5.60.

Monica Wilson in this work has performed her duty to traditional ethnology in her detailed and careful explication of the rituals of kinship among an African people studied by herself and her husband over a period of years. But she has not limited herself to mere description; the introductory and summary chapters deal with the general concepts of ritual in society, as well as with the basic concepts of this particular group, one which seems almost "classical" in its concern with primitive ideas of pollution and fertility. Moreover, she has endeavored to interpret the symbolic and general social significance of the rituals with a special stress on the interpretation given by the

people themselves. This is in part, then, an attempt to deal with an "inside view" of culture.

Of particular interest to the student of society is the material on individual differences in interpretation and action, not only between Christian and pagan—not all Christians completely abandon traditional rituals, for example—but between the more thoughtful and reflective and the more superficial minds of the community. Mrs. Wilson makes the statement (following British social-anthropological tradition, one feels) that "the anthropologist cannot look into men's minds." Yet she has done so here, perhaps in spite of herself; one only wishes that she might more fully have set forth the look inside the minds of the Nyakyusa which she has, in fact, taken.

MARGARET PARK REDFIELD

Chicago

Intellectuals in Labor Unions: Organizational Pressures on Professional Roles. By HAROLD L. WILENSKY. Glencoe, Ill.: Free Press, 1956. Pp. xiii + 336. \$6.00.

This is a study of large-scale organization done with rare sophistication. Professor Wilensky set for himself the problem of determining the characteristics of central staff personnel in labor unions, carrying on largely intelligence functions. He is fully conversant with classic organization theory, but he had the good sense and keen analytical insight to ask the right empirical questions of real functionaries and those who have observed them in action. The result is a brilliant study of organizational behavior which makes important additions to the theory of organization.

The study is based on an analysis of 28 unions and 175 staff experts in them. Sufficient data were available on 129 of the staff men to make the core around which the analysis is based. The architecture of the study is clean and straightforward. Wilensky first sets down the general functions of staff experts in the union setting and then describes in detail three main types: "Facts and Figures Man," "Contact Man," and "Internal Communications Specialist." He marshals effective facts from interviews to give depth to the description of the empirical types derived from his data. The second major part of the analysis deals with the

role orientations of staff specialists. Here Wilensky finds four major types: "Missionaries," "Professional Service Experts," "Careerists," and "Politicos," the first three of which are further subdivided, making a total of eight types of role orientation. The final section of the volume centers on the influence of staff men over organization policy and leader behavior. Here the author sees the expert "moving in" on the organization in four types of capacity, to achieve a vantage point of influence or actual authority: as "High Influence Staff Expert," "Braintruster-Confidante," "Housekeeping Administrative Assistant," or elected or appointed line official.

For those interested in the perennial problem of self-image versus organizational office and the consequences of divergencies between the two, Wilensky's section on experts' role orientations provides one of the best sets of data and analysis yet brought together. He traces modifications in role orientation and describes the organizational pressures that cause them, so as to show that even unions, as imperfectly bureaucratized as they are, significantly mold their functionaries. He never fails to keep the other side of the coin fully in view, too: that changes in role orientation require extensive rationalization, often accompanied by bitter misgivings and a sense of running out of purpose. It is probable in the study of business organizations, for example, that careerist and professional service orientation found among labor experts will prevail. If my own studies have any relevance, they seem to confirm abundantly that business staff experts go through the same metamorphoses of role orientation as those Wilensky so ably analyzes for union experts.

The final two sections of the book dealing with staff men's influence is an equally significant analysis of another classic problem in organization: the relation between influence and authority. This study shows the stratagems used by staff people to influence policy or leaders. It demonstrates the techniques for controlling channels of information in the struggle for influence. The author also uses the last sections of the analysis to examine the bonds between the leader and his staff. Wilensky concludes that little of the bureaucrat's loyalty is wasted on "the impersonal order," with most of it centered on the leader's charisma or acceptance of his patrimonial ways of ruling.

In his treatment of role orientation versus

office, of influence and authority, and of leader-staff bonds, Professor Wilensky has made significant contributions to the theory of organization. It would be a shame if students of formal organization were misled by the title of this work to ignore it as "just another book about labor." The contribution to an understanding of union organization is real, and the addition to our knowledge about formal organizations in general is even more important.

ROBERT DUBIN

*Center for Advanced Study in
the Behavioral Sciences*

Operations of Sociological Inquiry. By MARIO LINS. Rio de Janeiro: Sociedade Brasileira de Sociologia, 1956. Pp. 122.

"To the classic peril of being impaled on the horns of a dilemma, we moderns should add a new one, being split by a false dichotomy," wrote George Homans in *The Human Group*, and the latter part of his aphorism could well serve as epigram for Lins's essay. Lins's chief concern is to show how many of the problems plaguing sociological inquiry are rooted in faulty and outmoded logicoscientific forms, a legacy of the Aristotelian philosophical tradition.

The use of this logic presents sociology with four false dichotomies: the naturalist versus antinaturalist debate grows out of the separation of scientific disciplines into *Naturwissenschaften* and *Geisteswissenschaften*, which division is in turn based on the dichotomous classification of mind and matter as "ontological ultimates"; concern with the absolute essence of

concepts has led to our separating logicosymbolic formulations from empirical operations; the dichotomy of form and content, which ensnared Simmel, is a consequence of this logic which accentuates the static aspect of reality; and the problem of the unique as against the general results from the unbridgeable categories posited by the logic.

In general, Lins concludes that the Aristotelian logic is unsatisfactory as a basis for scientific research, inasmuch as it implies a fundamentally static conceptual substantialism [*sic*]; an atomistic or elementalistic [*sic*] structure; and a non-functional relationality [*sic*]. As a corrective, a new logicoscientific form based on functional categories is proposed—a "field theory" to replace the "class theory." Such a theory would substitute an organic for an atomistic basis, posit a reciprocal dependence between context and its constituents, facilitate the dialectical synthesis of the symbolic-conceptual structure and the observed data, and replace static with functional invariance as a basis for scientific generalization.

Lins's essay is relatively short, considering its scope, and, as his prose is boiled down to essentials and is heavy with neologisms, it will prove rather difficult reading for the average non-philosophical sociologist. The work was somewhat disappointing, for neither are the ideas making up the general thesis particularly novel, as Lins would probably be the first to admit, nor are their specific implications for methodology carefully stated, to guide the pedestrian sociologist knee-deep in empirical data.

W. RICHARD SCOTT

Chicago, Illinois

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- ANDERSON, WOLFRIED A. *Bibliography of Researches in Rural Sociology*. Ithaca, N.Y.: New York State College of Agriculture, 1957. Pp. 186. \$2.00.
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The University of Chicago Press

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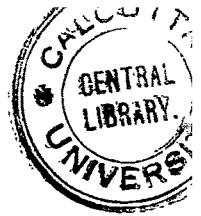
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THE AMERICAN JOURNAL OF SOCIOLOGY

Volume LXIII

JANUARY 1958

Number 4

SUPERORDINATE GOALS IN THE REDUCTION OF INTERGROUP CONFLICT¹

MUZAFAER SHERIF

ABSTRACT

This paper summarizes an experimental study on intergroup relations, with emphasis on the reduction of conflict between groups. In the first phase, two groups were established independently by introducing specified conditions for interaction; in the second phase, the groups were brought into functional contact in conditions perceived by the members of the respective groups as competitive and frustrating. Members developed unfavorable attitudes and derogatory stereotypes of the other group; social distance developed to the point of mutual avoidance, even in pleasant activities. In the final phase of the experiment the measure that proved effective in reducing tension between groups was the introduction of goals which were compellingly shared by members of the groups and which required the collaborative efforts of all.

In the past, measures to combat the problems of intergroup conflicts, proposed by social scientists as well as by such people as administrators, policy-makers, municipal officials, and educators, have included the following: introduction of legal sanctions; creation of opportunities for social and other contacts among members of conflicting groups; dissemination of correct information to break down false prejudices and unfavorable stereotypes; appeals to the moral ideals of fair play and brotherhood; and even the introduction of rigorous physical activity to produce catharsis by releasing pent-up frustrations and aggressive complexes in the unconscious. Other measures proposed include the encouragement of co-operative habits in one's own community, and bringing together in the cozy atmosphere of a meeting room the leaders of antagonistic groups.

Many of these measures may have some value in the reduction of intergroup conflicts, but, to date, very few generalizations have been established concerning the circumstances and kinds of intergroup conflict in which these measures are effective. Today measures are applied in a somewhat trial-and-error fashion. Finding measures that have wide validity in practice can come only through clarification of the nature of intergroup conflict and analysis of the factors conducive to harmony and conflict between groups under given conditions.

The task of defining and analyzing the nature of the problem was undertaken in a previous publication.² One of our major statements was the effectiveness of superordinate goals for the reduction of intergroup conflict. "Superordinate goals" we defined as goals which are compelling and highly appealing to members of two or

¹ The main points in this paper were presented at the Third Inter-American Congress of Psychology, Austin, Texas, December 17, 1955.

² Muzafer Sherif and Carolyn W. Sherif, *Groups in Harmony and Tension* (New York: Harper & Bros. 1953).

more groups in conflict but which cannot be attained by the resources and energies of the groups separately. In effect, they are goals attained only when groups pull together.

INTERGROUP RELATIONS AND THE BEHAVIOR OF GROUP MEMBERS

Not every friendly or unfriendly act toward another person is related to the group membership of the individuals involved. Accordingly, we must select those actions relevant to relations between groups.

Let us start by defining the main concepts involved. Obviously, we must begin with an adequate conception of the key term—"group." A group is a social unit (1) which consists of a number of individuals who, at a given time, stand in more or less definite interdependent status and role relationships with one another and (2) which explicitly or implicitly possesses a set of values or norms regulating the behavior of individual members, at least in matters of consequence to the group. Thus, shared attitudes, sentiments, aspirations, and goals are related to and implicit in the common values or norms of the group.

The term "intergroup relations" refers to the relations between two or more groups and their respective members. In the present context we are interested in the acts that occur when individuals belonging to one group interact, collectively or individually, with members of another in terms of their group identification. The appropriate frame of reference for studying such behavior includes the functional relations between the groups. Intergroup situations are not voids. Though not independent of relationships within the groups in question, *the characteristics of relations between groups cannot be deduced or extrapolated from the properties of in-group relations.*

Prevalent modes of behavior within a group, in the way of co-operativeness and solidarity or competitiveness and rivalry among members, need not be typical of actions involving members of an out-group.

At times, hostility toward out-groups may be proportional to the degree of solidarity within the group. In this connection, results presented by the British statistician L. F. Richardson are instructive. His analysis of the number of wars conducted by the major nations of the world from 1850 to 1941 reveals that Great Britain heads the list with twenty wars—more than the Japanese (nine wars), the Germans (eight wars), or the United States (seven wars). We think that this significantly larger number of wars engaged in by a leading European democracy has more to do with the intergroup relations involved in perpetuating a far-flung empire than with dominant practices at home or with personal frustrations of individual Britishers who participated in these wars.³

In recent years relationships between groups have sometimes been explained through analysis of individuals who have endured unusual degrees of frustration or extensive authoritarian treatment in their life-histories. There is good reason to believe that some people growing up in unfortunate life-circumstances may become more intense in their prejudices and hostilities. But at best these cases explain the intensity of behavior in a given dimension.⁴ In a conflict between two groups—a strike or a war—opinion within the groups is crystallized, slogans are formulated, and effective measures are organized by members recognized as the most responsible in their respective groups. The prejudice scale and the slogans are not usually imposed on the others by the deviate or neurotic members. Such individuals ordinarily exhibit their intense reactions within the reference scales of prejudice, hostility, or sacrifice established in their respective settings.

The behavior by members of any group toward another group is not primarily a problem of deviate behavior. If it were, in-

³ T. H. Pear, *Psychological Factors of Peace and War* (New York: Philosophical Library, 1950), p. 126.

⁴ William R. Hood and Muzafer Sherif, "Personality Oriented Approaches to Prejudice," *Sociology and Social Research*, XL (1955), 79–85.

tergroup behavior would not be the issue of vital consequence that it is today. The crux of the problem is the participation by group members in established practices and social-distance norms of their group and their response to new trends developing in relationships between their own group and other groups.

On the basis of his UNESCO studies in India, Gardner Murphy concludes that to be a good Hindu or a good Moslem implies belief in all the nasty qualities and practices attributed by one's own group—Hindu or Moslem—to the other. Good members remain deaf and dumb to favorable information concerning the adversary. Social contacts and avenues of communication serve, on the whole, as vehicles for further conflicts not merely for neurotic individuals but for the bulk of the membership.⁵

In the process of interaction among members, an in-group is endowed with positive qualities which tend to be praiseworthy, self-justifying, and even self-glorifying. Individual members tend to develop these qualities through internalizing group norms and through example by high-status members, verbal dicta, and a set of correctives standardized to deal with cases of deviation. Hence, possession of these qualities, which reflect their particular brand of ethnocentrism, is not essentially a problem of deviation or personal frustration. It is a question of participation in in-group values and trends by good members, who constitute the majority of membership as long as group solidarity and morale are maintained.

To out-groups and their respective members are attributed positive or negative qualities, depending on the nature of functional relations between the groups in question. The character of functional relations between groups may result from actual harmony and interdependence or from actual incompatibility between the aspirations and directions of the groups. A number of field studies and experiments indicate that, if

the functional relations between groups are positive, favorable attitudes are formed toward the out-group. If the functional relations between groups are negative, they give rise to hostile attitudes and unfavorable stereotypes in relation to the out-group. Of course, in large group units the picture of the out-group and relations with it depend very heavily on communication, particularly from the mass media.

Examples of these processes are recurrent in studies of small groups. For example, when a gang "appropriates" certain blocks in a city, it is considered "indecent" and a violation of its "rights" for another group to carry on its feats in that area. Intrusion by another group is conducive to conflict, at times with grim consequences, as Thrasher showed over three decades ago.⁶

When a workers' group declares a strike, existing group lines are drawn more sharply. Those who are not actually for the strike are regarded as against it. There is no creature more lowly than the man who works while the strike is on.⁷ The same type of behavior is found in management groups under similar circumstances.

In time, the adjectives attributed to out-groups take their places in the repertory of group norms. The lasting, derogatory stereotypes attributed to groups low on the social-distance scale are particular cases of group norms pertaining to out-groups.

As studies by Bogardus show, the social-distance scale of a group, once established, continues over generations, despite changes of constituent individuals, who can hardly be said to have prejudices because of the same severe personal frustrations or authoritarian treatment.⁸

Literature on the formation of prejudice

⁶ F. M. Thrasher, *The Gang* (Chicago: University of Chicago Press, 1927).

⁷ E. T. Hiller, *The Strike* (Chicago: University of Chicago Press, 1928).

⁸ E. S. Bogardus, "Changes in Racial Distances," *International Journal of Opinion and Attitude Research*, I (1947), 55-62.

⁵ Gardner Murphy, *In the Minds of Men* (New York: Basic Books, 1953).

by growing children shows that it is not even necessary for the individual to have actual unfavorable experiences with out-groups to form attitudes of prejudice toward them. In the very process of becoming an in-group member, the intergroup delineations and corresponding norms prevailing in the group are internalized by the individual.⁹

A RESEARCH PROGRAM

A program of research has been under way since 1948 to test experimentally some hypotheses derived from the literature of intergroup relations. The first large-scale intergroup experiment was carried out in 1949, the second in 1953, and the third in 1954.¹⁰ The conclusions reported here briefly are based on the 1949 and 1954 experiments and on a series of laboratory studies carried out as co-ordinate parts of the program.¹¹

The methodology, techniques, and criteria for subject selection in the experiments must be summarized here very briefly. The

experiments were carried out in successive stages: (1) groups were formed experimentally; (2) tension and conflict were produced between these groups by introducing conditions conducive to competitive and reciprocally frustrating relations between them; and (3) the attempt was made toward reduction of the intergroup conflict. This stage of reducing tension through introduction of superordinate goals was attempted in the 1954 study on the basis of lessons learned in the two previous studies.

At every stage the subjects interacted in activities which appeared natural to them at a specially arranged camp site completely under our experimental control. They were not aware of the fact that their behavior was under observation. No observation or recording was made in the subjects' presence in a way likely to arouse the suspicion that they were being observed. There is empirical and experimental evidence contrary to the contention that individuals cease to be mindful when they know they are being observed and that their words are being recorded.¹²

In order to insure validity of conclusions, results obtained through observational methods were cross-checked with results obtained through sociometric technique, stereotype ratings of in-groups and out-groups, and through data obtained by techniques adapted from the laboratory. Unfortunately, these procedures cannot be elaborated here. The conclusions summarized briefly are based on results cross-checked by two or more techniques.

The production of groups, the production of conflict between them, and the reduction of conflict in successive stages were brought about through the introduction of problem situations that were real and could not be ignored by individuals in the situation. Spe-

⁹ E. L. Horowitz, "Race Attitudes," in Otto Klineberg (ed.), *Characteristics of the American Negro*, Part IV (New York: Harper & Bros., 1944).

¹⁰ The experimental work in 1949 was jointly supported by the Yale Attitude Change Project and the American Jewish Committee. It is summarized in Sherif and Sherif, *op. cit.*, chaps. ix and x. Both the writing of that book and the experiments in 1953-54 were made possible by a grant from the Rockefeller Foundation. The 1953 research is summarized in Muzafer Sherif, B. Jack White, and O. J. Harvey, "Status in Experimentally Produced Groups," *American Journal of Sociology*, LX (1955), 370-79. The 1954 experiment was summarized in Muzafer Sherif, O. J. Harvey, B. Jack White, William R. Hood, and Carolyn W. Sherif, "Experimental Study of Positive and Negative Intergroup Attitudes between Experimentally Produced Groups: Robbers Cave Study" (Norman, Okla.: University of Oklahoma, 1954). (Multitheted.) For a summary of the three experiments see chaps. vi and ix in Muzafer Sherif and Carolyn W. Sherif, *An Outline of Social Psychology* (rev. ed.; New York: Harper & Bros., 1956).

¹¹ For an overview of this program see Muzafer Sherif, "Integrating Field Work and Laboratory in Small Group Research," *American Sociological Review*, XIX (1954), 759-71.

¹² E.g., see F. B. Miller, "Residentialism" in *Applied Social Research*, *Human Organization*, XII (1954), 5-8; S. Wapner and T. G. Alper, "The Effect of an Audience on Behavior in a Choice Situation," *Journal of Abnormal and Social Psychology*, XLVII (1952), 222-29.

cial "lecture methods" or "discussion methods" were not used. For example, the problem of getting a meal through their own initiative and planning was introduced when participating individuals were hungry.

Facing a problem situation which is immediate and compelling and which embodies a goal that cannot be ignored, group members *do* initiate discussion and *do* plan and carry through these plans until the objective is achieved. In this process the discussion becomes *their* discussion, the plan *their* plan, the action *their* action. In this process discussion, planning, and action have their place, and, when occasion arises, lecture or information has its place, too. The sequence of these related activities need not be the same in all cases.

The subjects were selected by rigorous criteria. They were healthy, normal boys around the age of eleven and twelve, socially well adjusted in school and neighborhood, and academically successful. They came from a homogeneous sociocultural background and from settled, well-adjusted families of middle or lower-middle class and Protestant affiliations. No subject came from a broken home. The mean I.Q. was above average. The subjects were not personally acquainted with one another prior to the experiment. Thus, explanation of results on the basis of background differences, social maladjustment, undue childhood frustrations, or previous interpersonal relations was ruled out at the beginning by the criteria for selecting subjects.

The first stage of the experiments was designed to produce groups with distinct structure (organization) and a set of norms which could be confronted with intergroup problems. The method for producing groups from unacquainted individuals with similar background was to introduce problem situations in which the attainment of the goal depended on the co-ordinated activity of all individuals. After a series of such activities, definite group structures or organizations developed.

The results warrant the following con-

clusions for the stage of group formation: When individuals interact in a series of situations toward goals which appeal to all and which require that they co-ordinate their activities, group structures arise having hierarchical status arrangements and a set of norms regulating behavior in matters of consequence to the activities of the group.

Once we had groups that satisfied our definition of "group," relations between groups could be studied. Specified conditions conducive to friction or conflict between groups were introduced. This negative aspect was deliberately undertaken because the major problem in intergroup relations today is the reduction of existing intergroup frictions. (Increasingly, friendly relations between groups is not nearly so great an issue.) The factors conducive to intergroup conflict give us realistic leads for reducing conflict.

A series of situations was introduced in which one group could achieve its goal only at the expense of the other group—through a tournament of competitive events with desirable prizes for the winning group. The results of the stage of intergroup conflict supported our main hypotheses. During interaction between groups in experimentally introduced activities which were competitive and mutually frustrating, members of each group developed hostile attitudes and highly unfavorable stereotypes toward the other group and its members. In fact, attitudes of social distance between the groups became so definite that they wanted to have nothing further to do with each other. This we take as a case of experimentally produced "social distance" in miniature. Conflict was manifested in derogatory name-calling and invectives, flare-ups of physical conflict, and raids on each other's cabins and territory. Over a period of time, negative stereotypes and unfavorable attitudes developed.

At the same time there was an increase in in-group solidarity and co-operativeness. This finding indicates that co-operation and democracy within groups do not neces-

sarily lead to democracy and co-operation with out-groups, if the directions and interests of the groups are conflicting.

Increased solidarity forged in hostile encounters, in rallies from defeat, and in victories over the out-group is one instance of a more general finding: Intergroup relations, both conflicting and harmonious, *affected the nature of relations within the groups involved*. Altered relations between groups produced significant changes in the status arrangements *within* groups, in some instances resulting in shifts at the upper status levels or even a change in leadership. Always, consequential intergroup relations were reflected in new group values or norms which signified changes in practice, word, and deed within the group. Counterparts of this finding are not difficult to see in actual and consequential human relations. Probably many of our major preoccupations, anxieties, and activities in the past decade are incomprehensible without reference to the problems created by the prevailing "cold war" on an international scale.

REDUCTION OF INTERGROUP FRICTION

A number of the measures proposed today for reducing intergroup friction could have been tried in this third stage. A few will be mentioned here, with a brief explanation of why they were discarded or were included in our experimental design.

1. Disseminating favorable information in regard to the out-group was not included. Information that is not related to the goals currently in focus in the activities of groups is relatively ineffective, as many studies on attitude change have shown.¹³

2. In small groups it is possible to devise sufficiently attractive rewards to make individual achievement supreme. This may reduce tension between groups by splitting the membership on an "every-man-for-himself" basis. However, this measure has little relevance for actual intergroup tensions,

which are in terms of group membership and group alignments.

3. The resolution of conflict through leaders alone was not utilized. Even when group leaders meet apart from their groups around a conference table, they cannot be considered independent of the dominant trends and prevailing attitudes of their membership. If a leader is too much out of step in his negotiations and agreements with out-groups, he will cease to be followed. It seemed more realistic, therefore, to study the influence of leadership within the framework of prevailing trends in the groups involved. Such results will give us leads concerning the conditions under which leadership can be effective in reducing intergroup tensions.

4. The "common-enemy" approach is effective in pulling two or more groups together against another group. This approach was utilized in the 1949 experiment as an expedient measure and yielded effective results. But bringing some groups together against others means larger and more devastating conflicts in the long run. For this reason, the measure was not used in the 1954 experiment.

5. Another measure, advanced both in theoretical and in practical work, centers around social contacts among members of antagonistic groups in activities which are pleasant in themselves. This measure was tried out in 1954 in the first phase of the integration stage.

6. As the second phase of the integration stage, we introduced a series of superordinate goals which necessitated co-operative interaction between groups.

The social contact situations consisted of activities which were satisfying in themselves—eating together in the same dining room, watching a movie in the same hall, or engaging in an entertainment in close physical proximity. These activities, which were satisfying to each group, but which did not involve a state of interdependence and co-operation for the attainment of goals, were not effective in reducing inter-

¹³ E.g., see R. M. Williams, *The Reduction of Intergroup Tensions* (Social Science Research Council Bull. 57 [New York, 1947]).

group tension. On the contrary, such occasions of contact were utilized as opportunities to engage in name-calling and in abuse of each other to the point of physical manifestations of hostility.

The ineffective, even deleterious, results of intergroup contact without superordinate goals have implications for certain contemporary learning theories and for practice in intergroup relations. Contiguity in pleasant activities with members of an out-group does not necessarily lead to a pleasurable image of the out-group if relations between the groups are unfriendly. Intergroup contact without superordinate goals is not likely to produce lasting reduction of intergroup hostility. John Gunther, for instance, in his survey of contemporary Africa, concluded that, when the intergroup relationship is exploitation of one group by a "superior" group, intergroup contact inevitably breeds hostility and conflict.¹⁴

INTRODUCTION OF SUPERORDINATE GOALS

After establishing the ineffectiveness, even the harm, in intergroup contacts which did not involve superordinate goals, we introduced a series of superordinate goals. Since the characteristics of the problem situations used as superordinate goals are implicit in the two main hypotheses for this stage, we shall present these hypotheses:

1. When groups in a state of conflict are brought into contact under conditions embodying superordinate goals, which are compelling but cannot be achieved by the efforts of one group alone, they will tend to co-operate toward the common goals.

2. Co-operation between groups, necessitated by a series of situations embodying superordinate goals, will have a cumulative effect in the direction of reducing existing conflict between groups.

The problem situations were varied in nature, but all had an essential feature in common—they involved goals that could

not be attained by the efforts and energies of one group alone and thus created a state of interdependence between groups: combating a water shortage that affected all and could not help being "compelling"; securing a much-desired film, which could not be obtained by either group alone but required putting their resources together; putting into working shape, when everyone was hungry and the food was some distance away, the only means of transportation available to carry food.

The introduction of a series of such superordinate goals was indeed effective in reducing intergroup conflict: (1) when the groups in a state of friction interacted in conditions involving superordinate goals, they did co-operate in activities leading toward the common goal and (2) a series of joint activities leading toward superordinate goals had the cumulative effect of reducing the prevailing friction between groups and unfavorable stereotypes toward the out-group.

These major conclusions were reached on the basis of observational data and were confirmed by sociometric choices and stereotype ratings administered first during intergroup conflict and again after the introduction of a series of superordinate goals. Comparison of the sociometric choices during intergroup conflict and following the series of superordinate goals shows clearly the changed attitudes toward members of the out-group. Friendship preferences shifted from almost exclusive preference for in-group members toward increased inclusion of members from the "antagonists." Since the groups were still intact following co-operative efforts to gain superordinate goals, friends were found largely within one's group. However, choices of out-group members grew, in one group, from practically none during intergroup conflict to 23 per cent. Using chi square, this difference is significant ($P < .05$). In the other group, choices of the out-group increased to 36 per cent, and the difference is significant ($P < .001$). The findings confirm observations

¹⁴ John Gunther, *Inside Africa* (New York: Harper & Bros., 1955).

that the series of superordinate goals produced increasingly friendly associations and attitudes pertaining to out-group members.

Observations made after several superordinate goals were introduced showed a sharp decrease in the name-calling and derogation of the out-group common during intergroup friction and in the contact situations without superordinate goals. At the same time the blatant glorification and bragging about the in-group, observed during the period of conflict, diminished. These observations were confirmed by comparison of ratings of stereotypes (adjectives) the subjects had actually used in referring to their own group and the out-group during conflict with ratings made after the series of superordinate goals. Ratings of the out-group changed significantly from largely unfavorable ratings to largely favorable ratings. The proportions of the most unfavorable ratings found appropriate for the out-group—that is, the categorical verdicts that “all of them are stinkers” or “. . . smart alecks” or “. . . sneaky”—fell, in one group, from 21 per cent at the end of the friction stage to 1.5 per cent after interaction oriented toward superordinate goals. The corresponding reduction in these highly unfavorable verdicts by the other group was from 36.5 to 6 per cent. The over-all differences between the frequencies of stereotype ratings made in relation to the out-group during intergroup conflict and following the series of superordinate goals are significant for both groups at the .001 level (using chi-square test).

Ratings of the in-group were not so exclusively favorable, in line with observed decreases in self-glorification. But the differences in ratings of the in-group were not statistically significant, as were the differences in ratings of the out-group.

Our findings demonstrate the effectiveness of a series of superordinate goals in the reduction of intergroup conflict, hostility, and their by-products. They also have

implications for other measures proposed for reducing intergroup tensions.

It is true that lines of communication between groups must be opened before prevailing hostility can be reduced. But, if contact between hostile groups takes place without superordinate goals, the communication channels serve as media for further accusations and recriminations. When contact situations involve superordinate goals, communication is utilized in the direction of reducing conflict in order to attain the common goals.

Favorable information about a disliked out-group tends to be ignored, rejected, or reinterpreted to fit prevailing stereotypes. But, when groups are pulling together toward superordinate goals, true and even favorable information about the out-group is seen in a new light. The probability of information being effective in eliminating unfavorable stereotypes is enormously enhanced.

When groups co-operate in the attainment of superordinate goals, leaders are in a position to take bolder steps toward bringing about understanding and harmonious relations. When groups are directed toward incompatible goals, genuine moves by a leader to reduce intergroup tension may be seen by the membership as out of step and ill advised. The leader may be subjected to severe criticism and even loss of faith and status in his own group. When compelling superordinate goals are introduced, the leader can make moves to further co-operative efforts, and his decisions receive support from other group members.

In short, various measures suggested for the reduction of intergroup conflict—disseminating information, increasing social contact, conferences of leaders—acquire new significance and effectiveness when they become part and parcel of interaction processes between groups oriented toward superordinate goals which have real and compelling value for all groups concerned.

INSTITUTE OF GROUP RELATIONS
UNIVERSITY OF OKLAHOMA

EVIDENCE AND PROCEDURE CHARACTERISTICS OF "RELIABLE" PROPOSITIONS IN SOCIAL SCIENCE¹

ROBERT C. HANSON

ABSTRACT

How have differences in research procedures affected the reliability of propositions advanced in American social-science research journals? Data on a hundred and twenty independently tested propositions are presented in this study. When "more reliable" propositions were identified by their confirmation in independent replication tests, the original formulations of these propositions proved to be significantly associated with the following evidence and procedure characteristics: (a) presented data interpreting all concepts in the propositions; (b) "large" amount of evidence; (c) an initially abstract, "organized" form of data; (d) systematic selection procedures; (e) quantitative analysis of data; and (f) explicit criterion of confirmation.

Replication—the independent testing of propositions originally advanced by some other investigator—is used in this study as a means of determining empirically how differences in procedures in social science have affected the reliability of propositions. "More reliable" propositions were operationally defined as propositions that have been confirmed in independent tests as reported in replication studies; "less reliable" propositions were those refuted in the independent tests. An "independently tested" proposition was identified as one to which a later investigator made direct comparison in his report of independent research bearing on the proposition. The outcome of the comparison designated whether the original proposition was essentially confirmed or refuted. Given these sets of propositions that obviously differed in their ability to withstand an independent test, the object of the study was to go back to the original works to determine what research practices had proved most effective for the formulation of reliable propositions.

The replication studies were found by utilizing indexes of social-science journals

and by scanning introductory and summary sections of articles suspected of being possible replications by the phrasing of titles. A portion of the list of replications published by Rose,² those he classified as "direct comparison" replications, provided an initial group of replications included in the present study. Altogether, sixty replication articles were found, yielding a hundred and twenty independently tested propositions, an average of two per article. Seven of these were eliminated from the statistical analysis on failing to pass a set of "fair-test" criteria which required congruency of concepts and adequate objectivity standards of the replicator's research in relation to the original study.

The group of replications covered in the study is briefly described as follows. In time, the replications date from 1917 to 1954; the original works from 1914 to 1952. The majority of both replications and originals appeared after 1930. The replications were published in American social-science journals and are distributed approximately equally among the fields of anthropology, psychology, and sociology. A data requirement for a replication excluded those evaluations of another's work that occur in "discussion" or "critical review" type articles.

When a suspected replication article was

¹ Revision of a paper read at the annual meeting of the American Sociological Society, September, 1956. The data presented in this report are taken from the writer's unpublished doctoral dissertation, "Confirmation in Social Research" (University of California, 1955). The helpful criticisms and suggestions of Kenneth E. Bock, Wolfram Eberhard, Duncan MacRae, Jr., and E. W. Strong are gratefully acknowledged.

² Arnold M. Rose, *Theory and Method in the Social Sciences* (Minneapolis: University of Minnesota Press, 1954), pp. 268-72.

found to be oriented to a prior study, as indicated by consistent references to the previous work and by the direct comparison of the conclusions of the later research with the propositions advanced in the original report, the study was accepted as a "purposeful" replication suitable for this investigation. Such decisions on the identification of replications for inclusion in the study were made without reference to any of the variables to be studied (e.g., whether confirmations or refutations were reported, whether there was quantitative or qualitative analysis of data, etc.).

Information was coded on a data sheet for each pair of compared propositions. The data presented here test hypotheses (a) evidence and procedure variables involved in the formulation of a proposition and (b) the reliability of the proposition as indicated by its confirmation or refutation in an independent test.

1. *Presentation of data.*—Hypothesis: *Original propositions advanced with relevant evidence are more frequently confirmed in independent tests than propositions advanced lacking relevant evidence.*

An example of one of the eighty-five propositions classified as containing concepts *all* interpreted in terms of presented data is this one from Allport and Kramer's study of prejudice (1):³ "Clinging to parental patterns is conducive to prejudice. A critical attitude toward parental patterns is conducive to freedom from prejudice." A question or series of questions on the subject's relation to his parents in the questionnaire on prejudice provides the interpretation of "clinging to" or "critical attitude toward" parental patterns. Prejudice score of the subject is the interpretation of "prejudice" and "freedom from prejudice." Data from the subjects' questionnaire forms can be grouped into these four categories to show the relation between "prejudice" and "clinging to parental patterns."

³ Numbers in parentheses refer to studies listed in the bibliographical appendix, "Studies in Support of the Data."

Data exist for each cell of the fourfold table.

An example of a proposition classified as "lacking evidence," since *some* of the concepts were not interpreted in terms of data, is this one from Jersild's study of memory for the pleasant and unpleasant (26): "Many unpleasantnesses are forgotten simply through lack of rehearsal . . . the result is that pleasant associations are strengthened through rehearsal, while unpleasant associations wane through disuse." Jersild's data show that more items classified as "pleasant" were remembered than "unpleasant" ones. However, the rehearsal variable advanced to account for the distribution of pleasant and unpleasant experiences reported is not interpreted (i.e., there are *no data* given on the presence or lack of rehearsal).⁴

Examples of original articles and books that advanced propositions with no presented interpretation in data are Dodd's theoretical discussion of the interactance hypothesis (14), Guthrie's theoretical arti-

⁴ Waters and Leeper (26) did present data on rehearsal in their test of the Jersild proposition. The concepts in the proposition they advance are all data-related, in contrast to the lack of data for the rehearsal variable in the original study. However, replicators' conclusions were not all completely data-related. Although "lacking evidence" themselves, a *few* replication comparisons were accepted under the fair-test criteria, since the replication conclusions were as "adequate" (really "inadequate") in terms of evidence as the original propositions presented lacking evidence. Such cases were accepted only when other propositions in the same replication article were all data-related or when procedures were explicit for part of the proposition. It is significant to note that this kind of pseudo-testing—claiming to refute or confirm another's original assertion without advancing evidence on all parts of the proposition—may mean that errors are being transmitted and perpetuated in the literature because of the lack of adequate empirical testing.

With regard to the classification of the relatively few cases where part of an original proposition is grounded in presented data and part of it not, such propositions are coded in this study as lacking descriptions of rules of procedure, or they are coded according to the evidence and procedure characteristics obtained from descriptions of procedures on the data-related part of the proposition.

cle on conditioning as a principle of learning (20), and Birkhoff's a priori measure of aesthetic value (5).

Table 1(1) shows that approximately 71 per cent of the propositions whose concepts were all interpreted in terms of the presented data were confirmed upon independent test; only 32 per cent of the propositions in which some or all of the concepts were *not* accompanied by relevant data were confirmed.

2. *Scope of evidence.*—Hypothesis: *Original propositions based on a large amount of evidence are more frequently confirmed in independent tests than propositions based on a small amount of evidence.*

The ninety-nine propositions for which rules of procedure could be determined in the original published report were categorized as being based on "cases," "small sample" (operationally defined as involving less than a hundred units), "large sample," and the "population."

The distinction between *cases* and *population* versus *samples* was easily maintained, since in the case of samples the presentation included description of the number of units and how they were collected (1, 7, 11, 15, 27). The distinction between cases and population is not so easy to make, but may be clarified by these examples. Only one proposition was classified as tested against a population—Stouffer's intervening opportunity proposition, tested independently by Bright and Thomas (51) and by Isbell (52). In this case operational definitions for the concepts of the proposition were given with reference to a particular population, prediction of the configuration of the *total* number of units of the population was derived from the working hypothesis, and the resulting configuration was checked against the *actual* distribution of the total number of units in that population. The replicators started with movements between units from different kinds of populations requiring different operational definitions, but again they predicted from their working hypotheses the configuration

for the *total* number of units in that population. Then the predicted distributions were checked against the actual distributions of the populations. Stouffer studied population movements between census tracts within a city, and Bright and Thomas studied population movements between states in the United States. While the rep-

TABLE 1

EVIDENCE AND PROCEDURE CHARACTERISTICS
AND RESULTS OF INDEPENDENT TESTS
OF PROPOSITIONS

EVIDENCE AND PROCEDURE VARIABLES IN THE ORIGINAL STUDIES	INDEPENDENT TEST RESULTS	
	Refuta- tions	Confirma- tions
1. <i>Presentation of data:</i>		
Evidence present.....	25	60
Evidence lacking.....	19	9
$\chi^2=11.5; p<.001$		
2. <i>Scope of evidence:</i>		
"Large" amount		
Population.....	0	2
Large sample.....	11	49
"Small" amount		
Small sample.....	12	9
Cases.....	13	3
χ^2 for collapsed table= 22.5; $p<.001$		
3. <i>Source of data:</i>		
Contrived documents.....	7	44
Given documents.....	14	16
Direct observation.....	15	3
$\chi^2=26.4; p<.001$		
4. <i>Initial organization of data:</i>		
Initial organization.....	20	53
No initial organization....	16	10
$\chi^2=8.25; p<.01$		
5. <i>Selection procedure:</i>		
Systematic.....	8	26
Not systematic.....	28	37
$\chi^2=2.9; p<.05$ (one-tail test)		
6. <i>Analysis of data:</i>		
Quantitative		
Test statistics.....	12	44
Descriptive statistics....	5	9
Qualitative		
With numbers.....	2	3
Without numbers.....	17	7
χ^2 for collapsed table= 13.29; $p<.001$		
7. <i>Criterion of confirmation:</i>		
Explicit.....	15	45
Implicit.....	29	24
$\chi^2=9.29; p<.01$		

lication study refers to a population of larger scope, the Stouffer data do not correspond to a *case* with reference to the replication study.

However, some original studies involving large numbers of units were classified as being *case* studies in relation to the replication. For example, observe Thomas' examination of Kennedy's (29) and Hollingshead's (24) hypothesis on mixed-marriage trends in the United States. Although Kennedy and Hollingshead base their generalization on the total number of units for a particular population (marriages in New Haven) and for a given time, the operational definition for "mixed marriage" remains the same if the generalization is extended to the state or the country. Thus, when Thomas presents data on mixed-marriage rates for other cities and sections of the United States, the Kennedy and Hollingshead data correspond to a *case* with reference to the replication study, and the original propositions were classified as based on a *case* study.

The propositions classified as being based on *cases* typically did not involve large numbers of units. Generalizations about non-literate societies seem usually to be grounded in personal observations of a few instances of the total possible number of relevant cases or on information gained from a few informants (19, 21, 47). Typically, there is neither mention of the number of items involved in the making of a generalization nor a description of a sampling procedure for selecting the units to be analyzed with reference to a given problem.

Table 1(2) shows an increasing percentage of confirmations, as the scope of data on which the original propositions were based increases. For example, of the thirteen propositions based on cases, only three, or 19 per cent, were confirmed; of the sixty propositions based on large samples, forty-nine, or 82 per cent, were confirmed in later independent tests.

3 and 4. *Source of data* and *Initial or-*

ganization of data.—No hypothesis had been formulated to predict later confirmation on the basis of information on "source of data." However, as shown in Table 1(3), significant differences became apparent.

Data sources were designated as *given documents*, *contrived documents*, or *direct observation*. Within each class a further distinction was made between data that appeared in an *organized* or arranged original form as opposed to data that were *not organized* or arranged in their original form.

The latter distinction was applied in the collection of data which test the following hypothesis: *Original propositions based on an analysis of phenomena already "organized" into datum categories at the outset of an investigation are more frequently confirmed in independent tests than propositions based on an analysis of phenomena not "organized" into datum categories at the outset of the investigation.* As the following illustrations indicate, the "initial organization of data" variable was aimed at differences in the precision and objectivity bases of original propositions. Theoretically, with regard to a precision dimension, the more precise the units of analysis which form the basis of a proposition, the more reliable the proposition is likely to be.

Given documents are distinguished by the fact that a person or agency other than the investigator was responsible for the original form of the data. Marriage-license records of a city (7), hospital records on patients of certain kinds (15), and city records on population movements (51) afford "given" *organized* data not originally contrived as such by the investigator. On the other hand, citations from published works (e.g., surveys of the research reports of other investigators), for the purpose of advancing a generalization, constitutes the use of "given" data that are not initially arranged in categories appropriate to the problem under consideration. Usually the necessary data are not so capable of precise and objective collection as the above-mentioned organized data in given docu-

ments. Steward's use of reports of ethnologists and archeologists (49), Whiting's survey of the literature on the origin of corn (58), and Hartland's references to others' reports on kinship in North American tribes (23) are examples of studies containing propositions classified as based on *given documents, data not initially organized*.⁵

Contrived documents are distinguished by the fact that the investigator himself determines what form the data will take. Questionnaire forms, interview schedules, and paper-and-pencil tests of various kinds yield *organized contrived* data (1, 11, 48, 54). In contrast to the relatively precise character of these data, contrived documents may yield data which appear in a form *not initially organized* or arranged for the purpose of the investigation. The autobiographical documents of students (32) are an example of this type.

Direct observation refers to data gathered by the investigator himself as a result

of firsthand observation in the field or in laboratory research. The *organized* or *systematic* subcategory here is characteristic of the laboratory situation in which the same observations are repeatedly made and results recorded systematically for each subject (6, 8, 27). Such data are relatively more precise than the typically non-systematic observations recorded in field notes or diaries, often characteristic of work with non-literate peoples (43, 45, 46, 47) classified as "direct observation, no initial organized form."

In a few instances a proposition advanced was supported by two or more kinds of data (e.g., both direct observation and given documents). In these cases a judgment was made as to the relative emphasis of the investigator, and the classification given the proposition, then, refers to the kind of data on which it is judged to be *primarily* based.

A null hypothesis that there is no significant difference in the frequencies of later confirmations of propositions based on data from different sources was rejected, as shown in Table 1(3). Propositions based on direct observation were least often confirmed; slightly over half of those based on given documents were confirmed, while 86 per cent of those originally based on data obtained from contrived documents were confirmed.

Table 1(4) shows that propositions dependent on an investigator's analysis of phenomena which involved the collection of data *not* initially organized or arranged into categories relevant to the problem under investigation were confirmed in less than 39 per cent of the later tests. Propositions based on an analysis of phenomena involving an initially abstract, organized form of data were confirmed in 73 per cent of the later tests.

5. *The selection procedure.*—Hypothesis: *Original propositions based on data collected under a systematic rule of selection are more frequently confirmed in independent tests than propositions based on data*

⁵ The hypotheses advanced in this study would be classified as being based on "given documents, data not initially organized." The phenomena under analysis here are propositions that appear in original studies—given documents. The propositions vary in their characteristics according to the descriptions of procedures and evidence collected relevant to the propositions. It is these descriptions that provide the basis for the designation of the characteristics of the propositions and thus for the collection of data in the present study. The propositions in the original studies are not classified as being based, for example, on "given documents," "contrived documents," or "direct observation." These designations depend on analysis of the raw descriptions in the reports. Hence, in this study, as in all cases where a proposition was classified as being based on "data not initially organized," the criterion is that the phenomena under analysis do not offer data conceptualized and presented in a form appropriate for the problem under investigation. The investigator must first build the unit categories relevant to his problem. His use of the materials requires a different organization of them. On the other hand, in the cases of propositions based on "data initially organized," the conceptual units have been previously used or established as in schedules, tests, and systematic records. The data here are organized in established units that can be used for the problem at hand.

collected under a non-systematic selection procedure.

The set of categories used in classifying propositions according to differences in selection procedure is based on the relative flexibility of rules followed by investigators in collecting data. At one extreme is the *population* category, which allows for no variation, since every unit in the population is considered in the analysis; *here* the selection of units is precise and objective. In the case of propositions based on samples, a distinction was made between "systematic" and "non-systematic" selection procedures. Propositions resulting from studies in which a sampling design was used to obtain a random or a representative sample, or where control of variables was obtained through the use of matched groups of subjects, were classified as employing a *systematic* rule of selection (7, 11, 12, 48, 59, 60). Propositions based on data from samples *not* involving sampling design were classified as employing a *non-systematic* selection procedure—typically in the use of subjects available in college classes or in the local community (1, 10, 26). Also, propositions advanced in studies in which the investigator surveys a portion of the literature in a problem area and brings together relevant cases to form the basis of a generalization were classified in the non-systematic category (37, 40, 49). Finally, the case studies were regarded as employing the most flexible rules of selection. We find here the extreme in possible variation and a relative lack of precision and objectivity with regard to selection procedure. The population and systematic sampling cases are grouped together as the *systematic* procedure instances, the non-systematic samples and the case studies make up the *non-systematic* procedure instances.

As shown in Table 1(5), over 76 per cent of the propositions formulated with data from units that had been systematically selected were confirmed independently as compared to 57 per cent of the propositions based on data from units not systematically selected.

6. *Analysis of data.*—Hypothesis: *Original propositions formulated as a result of quantitative analysis of data are more frequently confirmed in independent tests than propositions formulated as a result of qualitative analysis of data.*

Each of the ninety-nine original propositions based on presented data could be characterized as resulting from either *quantitative* or *qualitative* analysis of data. A further dichotomy was distinguished within each of these categories: whether the quantitative analysis included the use of *test statistics* or was confined to *descriptive statistics* and whether the primarily qualitative analysis included the use of *numbers* or not.

Quantitative analysis was indicated by the presence of tables of data and statistical terminology in the verbal presentation of results. Propositions accompanied by the results of tests of significance or correlation coefficients were classified under *test statistics*, typical, for example, in Allport and Kramer (1), Burgess and Cottrell (10), Centers (11), and Hollingshead (24). Propositions resulting from quantitative analysis of data, though lacking test statistics, were classified under *descriptive statistics* (7, 15, 27, 51).

Qualitative analysis, *with numbers*, while relatively rare, was indicated when tables were lacking but the conclusion included numerical terminology, usually percentages (17, 21, 32). Qualitative analysis *without numbers* was the classification used for propositions lacking any quantitative treatment of data (3, 4, 16, 19, 39).

As shown in Table 1(6), fifty-three out of seventy, or over 75 per cent, of the original propositions formulated with the help of quantitative analysis of data were confirmed; only ten out of twenty-nine, or less than 38 per cent, of the propositions resulting from qualitative analysis were confirmed.

7. *Criterion of confirmation.*—Hypothesis: *Original propositions advanced with explicit confirmation criteria are more frequently confirmed in independent tests*

than propositions advanced without explicit confirmation criteria.

In inductive science, propositions about populations are commonly advanced on the basis of the analysis of data about only a small portion of the total population. On what grounds do investigators believe their propositions will hold for the total population? How does a scientist decide that he has sufficient evidence to enable him to extend a generalization to the population? Evidently scientists in the same field agree on confirmation criteria—under certain conditions a proposition about a population will be tentatively accepted as confirmed; if these conditions are not met, the validity of the proposition will be regarded as questionable.

In contemporary social research, acceptable conditions for deciding that an extension of a generalization is justified appear to be the following: (a) the investigator reports that *all* the instances examined exhibit the proposed relation between variables and that *no* contradictory instances are known (44) and (b) the investigator reports the results of standard statistical techniques used in this situation in his field; for example, rejection of a null hypothesis at the 5 per cent level of significance allows, in practice, tentative acceptance of the positive hypothesis. There are studies in which statistical criteria were

used in presenting results on the acceptance or rejection of propositions (1, 10, 11). In all of the above cases, confirmation criteria are explicit. But in many studies nothing is said to specify the conditions under which a generalization should be accepted as confirmed or rejected as refuted (2, 3, 13, 15, 29). Here confirmation criteria are implicit and were regarded in this study as indicating less objective rules of procedure.

Table 1(7) shows that over 70 per cent of the original propositions advanced with explicit confirmation criteria were later confirmed in independent tests, while less than 46 per cent of the propositions advanced without explicit confirmation criteria were later confirmed.

When the reliability of propositions was identified by their confirmation or refutation in independent tests, more reliable propositions were found characterized by the following evidence and procedure characteristics in this study: (a) presented data interpreting all concepts in the proposition; (b) "large" amount of evidence; (c) an initially abstract, "organized" form of data; (d) systematic selection procedures; (e) quantitative analysis of data; and (f) explicit criterion of confirmation.

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APPENDIX: STUDIES IN SUPPORT OF THE DATA

Each pair of studies—the original and the replication—constitutes one item. The sixty items are listed alphabetically by original author. The item contains information on the number of direct comparisons made by the replicator, the judged outcome of these comparisons, and the classifications given the original propositions in abbreviated form.

KEY TO CLASSIFICATION ABBREVIATIONS IN THE APPENDIX

1. *Presentation of data*: 1A = evidence present; 1B = evidence lacking.
2. *Scope of evidence*: 2A = population; 2B = large sample; 2C = small sample; 2D = cases; 2E = no description of procedures.
3. *Source of data*: 3A = contrived documents; 3B = given documents; 3C = direct observation; 3D = no description of procedures.
4. *Initial organization of data*: 4A = initial organization; 4B = no initial organization; 4C = no description of procedures.
5. *Selection procedure*: 5A = systematic; 5B = non-systematic; 5C = no description of procedures.

6. *Analysis of data*: 6A = test statistics; 6B = descriptive statistics; 6C = qualitative analysis with numbers; 6D = qualitative analysis without numbers; 6E = no description of procedures.
 7. *Criterion of confirmation*: 7A = explicit; 7B = implicit.
1. ALLPORT, GORDON W., and KRAMER, BERNARD M. "Some Roots of Prejudice," *Journal of Psychology*, XXII (July, 1946), 9-39.
ROSENBLITH, JUDY FRANCIS. "A Replication of 'Some Roots of Prejudice,'" *Journal of Abnormal and Social Psychology*, XLIV (October, 1949), 470-89.
23 comparisons, 19 confirmations, 4 refutations.
Classifications: 1A(22), 1B(1); 2 all B; 3 all A; 4 all A; 5 all B; 6 all A; 7 all A.
 2. ARNOLD, MAGDA B. "On the Mechanism of Suggestion and Hypnosis," *Journal of Abnormal and Social Psychology*, XLI (April, 1946), 107-28.
MCBAIN, WILLIAM N. "Imagery and Suggestibility: A Test of the Arnold Hypothesis," *Journal of Abnormal and Social Psychology*, XLIX (January, 1954), 36-44.
2 comparisons, 1 confirmation, 1 refutation.
Classifications for both propositions: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
 3. BARBER, BERNARD. "Acculturation and Messianic Movements," *American Sociological Review*, VI (October, 1941), 663-69.
HILL, W. W. "The Navaho Indians and the Ghost Dance of 1890," *American Anthropologist*, XLVI (October-December, 1944), 523-27.
1 comparison, 1 refutation.
Classifications: 1B; 2D; 3B; 4B; 5B; 6D; 7B.
 4. BIRKET-SMITH, KAJ. *The Caribou Eskimos*. ("Report of the Fifth Thule Expedition, 1921-24," Vol. V.) Copenhagen: Gyl-dendal, 1929. ———. "The Question of the Origin of Eskimo Culture: A Rejoinder," *American Anthropologist*, XXXII (October-December, 1930), 608-24.
MATHIASSEN, THERKEL. "The Question of the Origin of Eskimo Culture," *American Anthropologist*, XXXII (October-December, 1930), 591-607.
1 comparison, refutation.
Classifications: 1B; 2D; 3C; 4B; 5B; 6D; 7B.
 5. BIRKHOFF, G. D. *Aesthetic Measure*. Cambridge, Mass.: Harvard University Press, 1933.
WILSON, DOUGLAS J. "An Experimental Investigation of Birkhoff's Aesthetic Measure," *Journal of Abnormal and Social Psychology*, XXXIV (July, 1939), 390-94.
1 comparison, refutation.
Classifications: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
 6. BLODGETT, HUGH CARLTON. "The Effect of the Introduction of Reward upon the Maze Performance of Rats," *University of California Publications in Psychology*, IV, No. 8 (1929), 113-34.
REYNOLDS, BRADLEY. "A Repetition of the Blodgett Experiment on 'Latent Learning,'" *Journal of Experimental Psychology*, XXXV (December, 1945), 504-16.
1 comparison, refutation.
Classifications: 1A; 2C; 3C; 4A; 5A; 6A; 7A.
 7. BOSSARD, JAMES H. S. "Residential Pro-pinquity as a Factor in Marriage Selec-tion," *American Journal of Sociology*, XXXVIII (September, 1932), 219-24.
KOLLER, MARVIN R. "Residential Pro-pinquity of White Mates at Marriage in Relation to Age and Occupation of Males, Columbus, Ohio, 1938 and 1946," *Amer-ican Sociological Review*, XIII (Octo-ber, 1948), 613-16.
1 comparison, confirmation.
Classifications: 1A; 2B; 3B; 4A; 5A; 6B; 7B.
 8. BRUNER, JEROME S., and POSTMAN, LEO. "Symbolic Value as an Organizing Factor in Perception," *Journal of Social Psychol-ogy*, XXVII (May, 1948), 203-8.
KLEIN, GEORGE S., SCHLESINGER, HER-BERT J., and MEISTER, DAVID E. "The Effect of Personal Values on Perception: An Experimental Critique," *Psychologi-cal Review*, LXVIII (March, 1951), 96-112.
1 comparison, refutation.
Classifications: 1A; 2C; 3C; 4A; 5A; 6A; 7A.

9. BURGESS, ERNEST W. "The Growth of the City: An Introduction to a Research Project," pp. 47-62, in R. E. PARK, E. W. BURGESS, and R. D. MCKENZIE, *The City*. Chicago: University of Chicago Press, 1925.
DAVIE, MAURICE R. "The Pattern of Urban Growth," pp. 133-61 in GEORGE P. MURDOCK, (ed.), *Studies in the Science of Society*. New Haven, Conn.: Yale University Press, 1937.
1 comparison, refutation; refutation judged not a "fair test," concepts not congruent.
10. BURGESS, ERNEST W., and COTTRELL, LEONARD S. *Predicting Success or Failure in Marriage*. New York: Prentice-Hall, Inc., 1939. ———. "The Prediction of Adjustment in Marriage," *American Sociological Review*, I (October, 1936), 737-51 (a preliminary partial report).
LOCKE, HARVEY J. "Predicting Marital Adjustment by Comparing a Divorced and a Happily Married Group," *American Sociological Review*, XII (April, 1947), 187-91.
7 comparisons, 7 confirmations.
Classifications: 1 all A; 2 all B; 3 all A; 4 all A; 5 all B; 6A(6), 6B(1); 7A(6), 7B(1).
11. CENTERS, RICHARD. *The Psychology of Social Classes*. Princeton, N.J.: Princeton University Press, 1949.
CASE, HERMAN M. "An Independent Test of the Interest Group Theory of Social Class," *American Sociological Review*, XVII (December, 1952), 751-55.
13 comparisons, 12 confirmations, 1 refutation.
Classifications for all twelve confirmed propositions: 1A; 2B; 3A; 4A; 5A; 6A; 7A.
Classifications for the refuted proposition: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
12. CHAPIN, F. STUART, JOHANSON, CLARENCE A., and JOHNSON, ARTHUR L. "Rental Rates and Crowding in Dwelling Units in Manhattan," *American Sociological Review*, XV (February, 1950), 95-97.
CHAPIN, F. STUART, and STRYKER, SHELDON. "Confirmation of Results of an Ex Post Facto Experimental Design by Replication," *American Sociological Review*, XV (October, 1950), 670-72.
2 comparisons, 2 confirmations.
Classifications for both propositions: 1A; 2B; 3B; 4A; 5A; 6A; 7A.
13. CHINOV, ELY. "The Tradition of Opportunity and the Aspirations of Automobile Workers," *American Journal of Sociology*, LVII (March, 1952), 453-59.
GUEST, ROBERT H. "Work Careers and Aspirations of Automobile Workers," *American Sociological Review*, XIX (April, 1954), 155-63.
2 comparisons, 1 refutation, 1 confirmation; refutation judged not a "fair test," concepts not congruent.
Classifications for the confirmed proposition: 1A; 2C; 3A; 4B; 5A; 6D; 7B.
14. DODD, STUART C. "The Interactance Hypothesis: A Gravity Model Fitting Physical Masses and Human Groups," *American Sociological Review*, XV (April, 1950), 245-56.
CAVANAUGH, JOSEPH A. "Formulation, Analysis and Testing of the Interactance Hypothesis," *American Sociological Review*, XV (December, 1950), 763-66.
1 comparison, confirmation.
Classifications: 1B; 2E; 3D; 4C; 5C; 6E; 7A.
15. DUNHAM, H. WARREN. "The Ecology of the Functional Psychoses in Chicago," *American Sociological Review*, II (August, 1937), 467-79.
FARIS, R. E. L. "Demography of Urban Psychotics with Special Reference to Schizophrenia," *American Sociological Review*, III (April, 1938), 203-9.
3 comparisons, 3 confirmations.
Classifications for all three propositions: 1A; 2B; 3B; 4A; 5A; 6B; 7B.
16. FARIS, R. E. L. "Cultural Isolation and the Schizophrenic Personality," *American Journal of Sociology*, XL (September, 1934), 155-64.
JACO, E. GARTLY. "The Social Isolation Hypothesis and Schizophrenia," *American Sociological Review*, XIX (October, 1954), 567-77.
1 comparison, confirmation.
Classifications: 1A; 2D; 3B; 4B; 5B; 6D; 7B.
17. FRAZIER, E. FRANKLIN. "The Negro Family in Bahia, Brazil," *American Sociological Review*, VII (August, 1942), 465-78.
HERSKOVITS, MELVILLE J. "The Negro in Bahia, Brazil: A Problem in Method,"

- American Sociological Review*, VIII (August, 1943), 394-402.
4 comparisons, 4 refutations; 3 judged not a "fair test," concepts not congruent. Classifications for one refuted proposition: 1A; 2C; 3C; 4B; 5B; 6C; 7B.
18. FREUD, S. *Collected Papers*, Vol. IV. London: Hogarth Press, 1925. ———. *The Psychopathology of Everyday Life*. New York: Macmillan Co., 1914.
SHARP, AGNES ARMINDA. "An Experimental Test of Freud's Doctrine of the Relation of Hedonic Tone to Memory Revival," *Journal of Experimental Psychology*, XXII (May, 1938), 395-418. 2 comparisons, 2 confirmations. Classifications for both propositions: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
 19. GOLDFRANK, ESTHER S. "Irrigation Agriculture and Navaho Community Leadership: Case Material on Environment and Culture," *American Anthropologist*, XLVII (April-June, 1945), 262-77.
COLLIER, MALCOLM C. "Leadership at Navaho Mountain and Klagetoh," *American Anthropologist* XLVIII (January-March, 1946), 137-38. 1 comparison, refutation. Classifications: 1B; 2D; 3B; 4B; 5B; 6D; 7B.
 20. GUTHRIE, E. R. "Conditioning as a Principle of Learning," *Psychological Review*, XXXVII (September, 1930), 412-28.
VOEKS, VIRGINIA W. "Acquisition of S-R Connections: A Test of Hull's and Guthrie's Theories," *Journal of Experimental Psychology*, XLVII (March, 1954), 137-47. 1 comparison, confirmation. Classifications: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
 21. HAAS, MARY R. "Creek Inter-Town Relations," *American Anthropologist*, XLII (July-September, 1940), 479-89.
SPOEHR, ALEXANDER. "Creek Inter-Town Relations," *American Anthropologist*, XLIII (January-March, 1941), 132-33. 1 comparison, confirmation. Classifications: 1A; 2D; 3C; 4B; 5B; 6C; 7B.
 22. HART, C. W. M. "A Reconsideration of the Natchez Social Structure," *American Anthropologist*, XLV (July-September, 1943), 374-86.
QUIMBY, GEORGE I. "Natchez Social Structure as an Instrument of Assimilation," *American Anthropologist*, XLVIII (January-March, 1946), 134-36. 1 comparison, refutation. Classifications: 1B; 2D; 3B; 4B; 5B; 6D; 7B.
 23. HARTLAND, E. SYDNEY. "Matrilineal Kinship, and the Question of Its Priority," *Memoirs of the American Anthropological Association*, IV, No. 1 (January-March, 1917), 1-87.
KROEBER, A. L. "The Matrilineal Again," *American Anthropologist*, XIX (October-December, 1917), 571-79. 1 comparison, refutation. Classifications: 1B; 2D; 3B; 4B; 5B; 6D; 7B.
 24. HOLLINGSHEAD, AUGUST B. "Cultural Factors in the Selection of Marriage Mates," *American Sociological Review*, XV (October, 1950), 619-27.
THOMAS, JOHN L. "The Factor of Religion in the Selection of Marriage Mates," *American Sociological Review*, XVI (August, 1951), 487-91. 1 comparison, refutation. Classifications: 1A; 2D; 3B; 4A; 5B; 6A; 7A.
 25. HULL, CLARK L. *A Behavior System*. New Haven, Conn.: Yale University Press, 1952.
VOEKS, VIRGINIA W. "Acquisition of S-R Connections: A Test of Hull's and Guthrie's Theories," *Journal of Experimental Psychology*, XLVII (March, 1954), 137-47. 1 comparison, refutation. Classifications: 1A; 2C; 3C; 4A; 5A; 6A; 7A.
 26. JERSILD, ARTHUR. "Memory for the Pleasant as Compared with the Unpleasant," *Journal of Experimental Psychology*, XIV (June, 1931), 284-88.
WATERS, R. H., and LEEPER, ROBERT. "The Relation of Affective Tone to the Retention of Experiences of Daily Life," *Journal of Experimental Psychology*, XIX (April, 1936), 203-15. 3 comparisons, 1 confirmation, 2 refutations. Classifications for the confirmed and one refuted proposition: 1A; 2C; 3A; 4A; 5B; 6A; 7A. Classifications for the other refuted proposition: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
 27. KEET, C. D. *Two Verbal Techniques in a*

- Miniature Counseling Situation.* ("Psychological Monographs," Vol. LXII, No. 7.) Washington: American Psychological Association, 1948.
- GRUMMON, DONALD L., and BUTLER, JOHN M. "Another Failure To Replicate Keet's Study, 'Two Verbal Techniques in a Miniature Counseling Situation,'" *Journal of Abnormal and Social Psychology*, XLVIII (October, 1953), 597. 1 comparison, refutation.
Classifications: 1A; 2C; 3C; 4A; 5A; 6B; 7A.
28. KEET, C. D. *Two Verbal Techniques in a Miniature Counseling Situation.* ("Psychological Monographs, Vol. LXII, No. 7.) Washington: American Psychological Association, 1948.
- MERRILL, R. M. "On Keet's Study, 'Two Verbal Techniques in a Miniature Counseling Situation,'" *Journal of Abnormal and Social Psychology*, XLVII (July, 1952), 722.
1 comparison, refutation.
Classifications same as No. 27 above.
29. KENNEDY, RUBY JO REEVES. "Single or Triple Melting Pot? Intermarriage Trends in New Haven, 1870-1940," *American Journal of Sociology*, XLIX (January, 1944), 331-39.
- THOMAS, JOHN L. "The Factor of Religion in the Selection of Marriage Mates," *American Sociological Review*, XVI (August, 1951), 487-91.
1 comparison, refutation.
Classifications: 1A; 2D; 3B; 4A; 5B; 6B; 7B.
30. KLINEBERG, OTTO. *Negro Intelligence and Selective Migration.* New York: Columbia University Press, 1935.
- LEE, EVERETT S. "Negro Intelligence and Selective Migration: A Philadelphia Test of the Klineberg Hypothesis," *American Sociological Review*, XVI (April, 1951), 227-33.
1 comparison, confirmation.
Classifications: 1A; 2B; 3B; 4A; 5B; 6A; 7A.
31. KOLLER, MARVIN R. "Residential Propinquity of White Mates at Marriage in Relation to Age and Occupation of Males, Columbus, Ohio, 1938 and 1946," *American Sociological Review*, XIII (October, 1948), 613-16.
- CLARKE, ALFRED C. "An Examination of the Operation of Residential Propinquity as a Factor in Mate Selection," *American Sociological Review*, XVII (February, 1952), 17-22.
3 comparisons, 2 confirmations, 1 refutation.
Classifications: 1 all A; 2 all B; 3 all B; 4 all A; 5 all A; 6A(1), 6B(2); 7A(1), 7B(2).
32. KOMAROVSKY, MIRRA. "Cultural Contradictions and Sex Roles," *American Journal of Sociology*, LII (November, 1946), 184-89.
- WALLIN, PAUL. "Cultural Contradictions and Sex Roles: A Repeat Study," *American Sociological Review*, XV (April, 1950), 288-93.
3 comparisons, 2 confirmations, 1 refutation.
Classifications for confirmed propositions: 1A; 2B; 3A; 4B; 5B; 6C; 7B.
Classifications for the refuted proposition: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
33. KRETSCHMER, E. *Physique and Character.* Trans. J. H. SPROTT. New York: Harcourt, Brace & Co., 1925.
- FARBER, MAURICE LEE. "A Critique and an Investigation of Kretschmer's Theory," *Journal of Abnormal and Social Psychology*, XXXIII (July, 1938), 398-404.
1 comparison, refutation.
Classifications: 1A; 2B; 3C; 4A; 5B; 6B; 7B.
34. KROEBER, A. L. *Zuni Kin and Clan.* ("Anthropological Papers of the American Museum of Natural History," Vol. XVIII, Part II.) New York, 1917.
- MURDOCK, G. P. "Bifurcate Merging: A Test of Five Theories," *American Anthropologist*, XLIX (January-March, 1947), 56-68.
1 comparison, confirmation.
Classifications: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
35. LAWRENCE, W. E., and MURDOCK, G. P. "Murngin Social Organization," *American Anthropologist*, LI (January-March, 1949), 58-65.
- RADCLIFFE-BROWN, A. R. "Murngin Social Organization," *American Anthropologist*, LIII (January-March, 1951), 37-55.
1 comparison, refutation.
Classifications: 1A; 2D; 3B; 4B; 5B; 6D; 7B.

36. LERNER, E. "The Problem of Perspective in Moral Reasoning," *American Journal of Sociology*, XLIII (September, 1937), 249-69. ———. *Constraint Areas and the Moral Judgment of Children*. Menasha, Wis.: George Banta, 1937.
MACRAE, DUNCAN, JR. "A Test of Piaget's Theories of Moral Development," *Journal of Abnormal and Social Psychology*, XLIX (January, 1954), 14-18.
1 comparison, refutation.
Classifications: 1B; 2B; 3C; 4A; 5B; 6C; 7B.
37. LOWIE, ROBERT H. "Exogamy and the Classificatory Systems of Relationship," *American Anthropologist*, XVII (April-June, 1915), 223-39.
MURDOCK, GEORGE PETER. "Bifurcate Merging: A Test of Five Theories," *American Anthropologist*, XLIX (January-March, 1947), 56-68.
1 comparison, confirmation.
Classifications: 1A; 2C; 3B; 4B; 5B; 6D; 7B.
38. MERRILL, E. D. "Tobacco in New Guinea," *American Anthropologist*, XXXII (January-March, 1930), 101-5.
LAUFER, BERTHOLD. "Tobacco in New Guinea: An Epilogue," *American Anthropologist*, XXXIII (January-March, 1931), 138-40.
1 comparison, refutation.
Classifications: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
39. MERRILL, E. D. "Tobacco in New Guinea," *American Anthropologist*, XXXII (January-March, 1930), 101-5.
LEWIS, ALBERT B. "Tobacco in New Guinea," *American Anthropologist*, XXXIII (January-March, 1931), 134-38.
1 comparison, refutation. (NOTE.—The comparison here was not to the same proposition that Laufer refuted in No. 38 above.)
Classifications: 1B; 2D; 3B; 4B; 5B; 6D; 7B.
40. MURDOCK, G. P. *Social Structure*. New York: Macmillan Co., 1949.
WILSON, THURLOW R. "Randomness of the Distribution of Social Organization Forms: A Note on Murdock's *Social Structure*," *American Anthropologist*, LIV (January-March, 1952), 134-38.
1 comparison, refutation.
Classifications: 1B; 2B; 3B; 4B; 5B; 6B; 7B.
41. PIAGET, J. *The Moral Judgment of the Child*. Trans. MARJORIE GABAIN from *Le Jugement morale chez l'enfant* (Paris: Alcan, 1932). Glencoe: Free Press, 1948.
MACRAE, DUNCAN, JR. "A Test of Piaget's Theories of Moral Development," *Journal of Abnormal and Social Psychology*, XLIX (January, 1954), 14-18.
2 comparisons, 2 refutations.
Classifications: 1A(1), 1B(1); both: 2B; 3C; 4A; 5B; 6D; 7B.
42. RADCLIFFE-BROWN, A. R. "Kinship Terminologies in California," *American Anthropologist*, XXXVII (July-September, 1935), 530-35.
OPLER, M. E. "Apache Data concerning the Relation of Kinship Terminology to Social Organization," *American Anthropologist*, XXXIX (April-June, 1937), 201-12.
1 comparison, refutation.
Classifications: 1B; 2E; 3D; 4C; 5C; 6E; 7B.
43. RADCLIFFE-BROWN, A. R. "Murngin Social Organization," *American Anthropologist*, LIII (January-March, 1951), 37-55.
ELKIN, A. P. "Murngin Kinship Re-examined, and Remarks on Some Generalizations," *American Anthropologist*, LV (August, 1953), 412-19.
1 comparison, refutation.
Classifications: 1B; 2D; 3C; 4B; 5B; 6D; 7B.
44. RADCLIFFE-BROWN, A. R. *The Social Organization of Australian Tribes*. ("Oceania" Monographs, No. 1.) Melbourne, 1931.
MURDOCK, G. P. "Bifurcate Merging: A Test of Five Theories," *American Anthropologist*, XLIX (January-March, 1947), 56-68.
1 comparison, refutation.
Classifications: 1A; 2C; 3C; 4A; 5B; 6D; 7A.
45. REICHARD, GLADYS A. "The Translation of Two Navaho Chant Words," *American Anthropologist*, XLIV (July-September, 1942), 421-24.
HAILE, BERARD. "Reichard's Chant of Waning Endurance," *American Anthropologist*, XLV (April-June, 1943), 306-11.

- 1 comparison, refutation.
Classifications: 1A; 2D; 3C; 4B; 5B; 6D; 7B.
46. RIVERS, W. H. R. *Kinship and Social Organization*. London: Constable, 1914.
MURDOCK, G. P. "Bifurcate Merging: A Test of Five Theories," *American Anthropologist*, XLIX (January-March, 1947), 56-68.
1 comparison, confirmation.
Classifications: 1B; 2C; 3C; 4B; 5B; 6D; 7B.
47. SAPIR, E. "Terms of Relationship and the Levirate," *American Anthropologist*, XVIII (July-September, 1916), 327-37.
MURDOCK, G. P. "Bifurcate Merging: A Test of Five Theories," *American Anthropologist*, XLIX (January-March, 1947), 56-68.
1 comparison, refutation.
Classifications: 1A; 2D; 3C; 4B; 5B; 6D; 7B.
48. SCHACHTER, STANLEY. "Deviation, Rejection and Communication," pp. 51-81 in LEON FESTINGER *et al.*, *Theory and Experiment in Social Communication*. Ann Arbor: Research Center for Group Dynamics, 1950.
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ON CRITERIA FOR SCALE RELATIONSHIPS

JAMES A. DAVIS

ABSTRACT

A conceptual model for "cumulative" scaling is set forth in terms of "dimensions," "reference points," and "response systems." On the basis of the model, criteria are suggested for assessing the validity, internal consistency, and reliability of scales. It is argued that "scaling" covers a range of research problems and that combinations of specific criteria provide more straightforward answers than a single coefficient of "scalability."

In practical research situations "cumulative" scales have many advantages. They are mechanically simple, they make only modest assumptions about quantification, and they enable the research worker to sort his cases into finer groupings than those given by a single "item." At the same time scaling presents many difficult problems: primarily, the criteria for accepting or rejecting a scale as a whole; secondarily, and related to the first, that, while the unwary worker may assume that a scale is somehow more "valid" or "reliable" than an individual item, he will find it difficult to assess these measurement properties directly. In this paper we shall attempt to discuss both problems in the context of a conceptual model of the scaling process.

CONCEPTUAL MODEL

Let us begin by considering three basic elements in the scaling process: (1) a dimension; (2) "reference points"; and (3) a response system.

Scaling begins with some underlying dimension of interest to the investigator, such as an attitude continuum, social status, or a behavioral sequence. We shall assume that our dimension exists before we begin our data analysis. Hence, we are not attempting to see what our data measure, but are aiming, instead, at discovering whether we have an adequate measure of the dimension. In addition, we must assume that the dimension is at least ordered. It may in truth be a very powerful scale (e.g., a nu-

merical continuum), but our approach will be sensitive only to ordinal differences.

Second, let us consider "reference points." We shall define them as *two sets of some sort, such that for each possible pair consisting of an element from set₁ and an element from set₂ one may ask their relative location on the dimension*. Perhaps we can make this clear with an example. Consider the dimension "education." We might construct the following simple items: (1) Did you graduate from the eighth grade? (2) Did you graduate from high school? (3) Did you graduate from college? If we administer these items to a sample of people, we can think of the results in terms of two sets—one a set of academic statuses (eighth grade, high school, college); the other, a set of people (eighth-grade graduates, high-school graduates, and college graduates within our sample). Within each of these sets the elements can be ordered along the dimension of education, and it is possible to compare the standing of any pair drawn from the two sets (e.g., Is John Smith's level of education higher or lower than the academic status "high-school graduate"?).

For most purposes, and in particular that of exposition, it would be much easier to think in terms of "subjects" and "objects" instead of set₁ and set₂, but for several reasons this formulation is too narrow. First, the distinction between subject and object appears to be a convention of questionnaire construction and not of the basic model of

scaling. For instance, we could give one person a questionnaire saying, "Does an eighth-grade graduate have as much education as John Smith? Does an eighth-grade graduate have as much education as Sam Jones? Does an eighth-grade graduate have as much education as Bill Brown?" We could give another person the same questionnaire with "high-school graduate" substituted for "eighth-grade graduate," and a third person might receive one using "college graduate." If each person knew John Smith, Sam Jones, and Bill Brown, we might get a cumulative scale, but our "subjects" would be academic statuses and our "objects" would be people (John Smith and his friends). Second, there is no intrinsic reason why the two sets have to refer to empirically different phenomena. We could scale heights in a small group by asking each person if he was taller than each other person in the group. Here the two sets differ only as logical classes, not as empirical phenomena. Finally, even if the respondents are rating "objects," it is not necessarily the respondents themselves who are being ordered. Suppose we were interested in perceptions of the social status of lawyers. We could ask, "Do you think lawyers have a higher social status than physicians?" "Do you think lawyers have a higher social status than librarians?" etc. We might get two orderings: one, occupations other than lawyer; the other, not the respondent's social status at all but his perception of the lawyer's status. Hence, we must think of *reference* points. Thus in the "social-distance" scale questions, such as "Would you agree to let Ruritians live in the next block?" "Would you agree to let Albigenians live in the next block?" the elements of the "subject" set are not the respondents' actual locations in social space but a social distance one block away. Although this, in effect, adds a constant to the subject's location and so preserves the ordering of subjects, the phenomenon is one of reference, not material location.

The third element in our model is the

"response system." We shall define it as an operation which orders pairs of reference points in terms of location on the dimension in question. Thus the datum that John Smith said "Yes" to the question "Did you graduate from high school?" can be seen as the end result of an operation which determines whether the reference point—John Smith's education—is higher on the dimension of education than the reference point "high-school graduate" from the other set.

Let us apply this approach to a standard attitude scale. Consider such items as "Communists should be deported" or "Communist books should be allowed in libraries." The dimension here is one of strength of sanctions against communism. The subject's reference point is that place on the continuum of sanctions beyond which he will not go. After that, he would consider the sanction too severe. A response of "agree" to the first question would tell us that deportation is below his reference point; a response of "agree" to the second would tell us that the sanction—removal of Communist books—is above his reference point. We should note that our definition does not require that subjects provide their own responses. A best friend might answer, or a "judge" might provide all data. Now, the statistical inferences to populations will be affected by the source of the data, but the model of scaling is indifferent to this question, although it does require that all data follow from the same kind of operation.

We shall distinguish three types of response systems in terms of their form of ordering, not in terms of their empirical operational details. Since we are considering measures of the power of an ordinal scale only, there are three possibilities for the comparison in a given response: (1) element_a is higher than element_b; (2) element_a is lower than element_b; or (3) element_a is the same as element_b. A response system which makes all three distinctions is defined as a "trichotomy." Thus, in an example to be developed later, we asked

women to judge photographs of living rooms as "higher," "the same," or "lower" in social status as compared with their own. (The continuum or dimension is social status—one reference point was the status of the subject's living room; the other, the status of a given pictured living room.)

The second type of response system we define as a "Guttman dichotomy." We shall call any response system such if it distinguishes only (1) above the reference point versus not above or (2) below the reference point versus not below. Putting it another way, this type of response system collapses "the same" with one of the other possibilities. In the education example, for instance, a response of "yes" to the question "Did you graduate from eighth grade?" tells us that eighth grade is below the subject's reference point, but it does not tell us that the subject is "an eighth-grade graduate." (He may have a Ph.D. degree.)

The third type of response system is the "Thurstone dichotomy."¹ It distinguishes the case where reference points are "the same" from all cases where they differ. "Was eighth grade your last grade in school?" would be an example. A "Yes" sorts out eighth-grade graduates, but a "No" might come from college graduates or persons who left school at sixth grade. Our model will be limited to trichotomies and Guttman dichotomies because Thurstone forms will not give us internal tests for properties of the scale, a phenomenon often noted in the literature on Guttman scaling.

Now let us assemble our elements to see how they might give us a complete "scale." If we should come across empirical data which are perfectly congruent with our model, and then design them in a matrix in

which the rows consist of elements from set_1 and the columns of elements from set_2 , and if we then arrange the rows and columns in terms of their true order on the dimension, we shall have constructed a "perfect scale pattern." For Guttman dichotomies the "step" pattern is too well known for discussion, but let us consider what a perfect scale for trichotomies should be. In Table 1 plus (+) indicates that the response shows the element from set_1 higher than the element from set_2 ; Zero (0) indicates that the response shows the element from set_1 is the same as the element from set_2 ; and minus (—) indicates that the response shows the element from set_1 to be below the element from set_2 :

TABLE 1
A PERFECT TRICHOTOMY

		SET ₂ ELEMENTS			
		1	2	3	4
SET ₁ ELEMENTS	1	+	+	+	+
	2	0	+	+	+
	3	—	+	+	+
	4	—	0	+	+
	5	—	—	+	+
	6	—	—	0	+
	7	—	—	—	+
	8	—	—	—	0
	9	—	—	—	—

To give these symbols a little content, let us think of the living-room status example noted above. Set_1 consists of people; set_2 of photographs. A plus (+) indicates that a given woman said that a given living-room picture is below her own living room, a zero (0) corresponds to the response "the same," and a minus (—) to the response that the pictured room is above the subject's.

In this context we may think of our matrix as dividing the dimension into a number of adjacent areas. These areas may be thought of as a sort of "club sandwich" composed of alternate layers from the two sets. In the status example, for instance, the dimension has been divided into nine areas, four of which are the locations of the "photographs" and five of which are the locations of the women, who are either

¹ The reference here is to the type of measurement implied by the standard "Thurstone scale." Coombs's "Pick 1" type of measurement would amount to the same thing (cf. Clyde H. Coombs, "Theory and Methods of Social Measurement," pp. 471-533, in Leon Festinger and Daniel Katz [eds.], *Research Methods in the Behavioral Sciences* [New York: Dryden Press, 1953]).

above, below, or between the levels occupied by the photographs. In general, under ideal conditions, we may say that data which fit the Guttman dichotomy model can yield $N + 1$ ordered classes in set_2 , where N equals the number of elements in set_1 ; while, for a trichotomy, set_1 can be used to define $2N + 1$ ordered classes in set_2 .

THE PROBLEM OF CRITERIA

It will be necessary to provide a set of specific operations to handle the problems arising in actual research situations. First, one almost never finds empirical data which consist only of the response types implied by a scaling model. Error appears to be inevitable. Without some criterion of *relative* fit to our model, all we could conclude is that our data fall short of our criterion. Second, even if we should happen to find data which appeared perfect, if our data are a probability sample from some universe, we would wish to make some statistical inferences to that universe, knowing that perfection in a sample is no guaranty of perfection in a universe.

The usual approach is to seize upon some general coefficient of scalability (perhaps reinforced by a few rules of thumb) such as "reproducibility." This, however, has some disadvantages. In the first place, it is rather difficult to fit this coefficient into any framework of sampling inferences, and, second, as a generalized index of perfection, it is sensitive to lapses from perfection and hence does not tell us the specific areas in which our data fall short.

We compute a reproducibility coefficient not because we have any real desire to reproduce response patterns from scale scores but, rather, because we hope that it is an index of certain measurement properties. Thus we proceed directly to assess the measurement properties of our data, and, if we are satisfied with the findings, let us accept our scale for the purposes at hand. We shall consider three such measurement properties: "validity," "internal consistency," and "reliability."

VALIDITY

If by "validity" we mean knowledge about what content dimension our scale actually measures, we must admit defeat at once. It is quite clear that there is no bootstrap operation for guaranteeing that our scale is valid in this sense of the term. Whether our device actually measures "authoritarianism," "anomie," or "social status" cannot be determined from an inspection of its working parts. Let us merely note that the usual procedure here is either to assess the measure's correlation with some outside criterion or to retreat into some philosophical position such as "operationalism."

In fact, this aspect of measurement would not merit comment except for the folk belief of many research workers that a scale is somehow "more valid" than a single item. The problem is actually rather complex, but we should like to suggest the following.

Let us begin with a mythical "perfect" scale. If our data should fit our model without any error at all, it follows that each "item" (or element) will be interchangeable with any other item and with the scale as a whole, except for the marginal cutting point. That is, we will get perfectly congruent rankings on set_2 regardless of which element or group of elements from set_1 we select. It follows, purely deductively, that each item or combination of items will have exactly the same degree of agreement with an outside criterion, and the scale is no more valid (or less valid) than any single element in it.

If, however, the scale is less than perfect, "error" responses will appear. Usually, the research worker proceeds to assign "error types" to the scale position of some perfect types (e.g., in a dichotomous Guttman scale scoring — — — + — — — as — — — — — —). In this case the scale as a whole no longer provides the same ordering as would a single element or item, and it is possible for differential validities to occur. The assumption that the reassignment has improved the validity in no way follows

from a theory of scaling but rather from some independent theory for ordering a "partially ordered" set.

Let us note that this distinction is not limited to our scaling model alone. One might argue, for instance, that, the lower the reproducibility of a Guttman scale, the greater the reason to use it, since only then does the scale pattern contribute to the orderings over and above what could be achieved with a given item. It is precisely when we have "bad" scales that we would wish to hedge our risk by using some combination of items. In "good" scales the only contribution of the scale as a whole is to provide a wider range of cutting points. Hence, we conclude that neither "reproducibility" nor an alternative index tells us much about validity. Conclusions about validity must come either from empirical correlations or from some more primitive theory about index formation and orderings.

INTERNAL CONSISTENCY

Even if we do not know the name of the dimension our data measure, can we at least ask that the "items" be internally consistent? This is something like asking whether the items in a test are factorially pure. However, given the ordinal nature of scaling data, we cannot meet the assumptions underlying factor analysis. We can, however, adopt the following formulation: If two elements from set_1 are ordering elements from set_2 on the same dimension, their orderings of set_2 should be similar except for marginal differences. Given such a formulation, "association" becomes the test for internal consistency.

Operationally, we may wish to test the null hypothesis that our results are a sample from a universe in which our elements have no relationship to each other. Any standard test for the significance of an association will do here. Or we may wish to add the criterion of "degree of association." A number of measures of this are available, but Goodman and Kruskal² present an index for ordinal contingency tables which seems

particularly appropriate, basing their measure (translated into our model) on the probability that two elements from one set will agree in their ordering of two elements from the other set.

When we come to apply our tests to actual data, we find that we have several alternatives. To begin with, we must decide whether to look at associations among set_1 elements or among set_2 elements, or both. Clearly, the outcomes cannot be independent, but the two approaches are not tautological. First, if the numbers of elements in the sets are unequal (e.g., six items and six hundred subjects), the significance-test probabilities may be expected to differ, since the conclusion of a significance test is partly a function of the number of observations. (In our example, subject associations would have an N of 6 and item associations an N of 600.) Second, to the extent that we are interested in a *particular* element (e.g., an especially doubtful item), associations among members of the other set do not provide the necessary information. Third, sometimes our research problem will dictate the answer. If we are interested in whether women agree in their perception of photographs of living rooms, because we want to make generalizations about women, we must compute subject associations. But, if we wish to know what sort of furniture makes for a given status placement of a picture, we must provide for a test of internal consistency by looking at photograph associations. Often, of course, we are interested in both and hence should compute both sets of figures. The important point is that there are two possible questions to be asked, and the answers, while related to each other, are not identical.

What about a generalized measure of internal consistency? We are at a loss to think of one in terms of a significance test (except, possibly, for a test of the hypothesis

² Leo A. Goodman and William H. Kruskal, "Measures of Association for Cross Classifications," *Journal of the American Statistical Association*, XLIX (1954), 732-63.

that the elements are samples from a universe in which there are just as many "positive" as "negative" associations), but the Goodman and Kruskal measure of ordinal association does suggest an index of the "degree" of internal consistency. If we think of drawing samples of two elements at a time, we can compute the three following probabilities from our data: (1) the probability that two elements will come from different perfect types; (2) the probability that two elements will come from the same perfect types; and (3) the probability that two elements will come from (a) two different error types, (b) the same error type, or (c) an error type and a perfect type.

TABLE 2
POSSIBLE JOINT RESPONSES
TO TWO ELEMENTS

ELEMENT A	ELEMENT B		
	+	0	-
+	1	2	3
0	4	5	6
-	7	8	9

Probability (3) amounts to the probability that our scale will give an internally inconsistent answer. This suggests the following two-dimensional general index: The ratio of probability (1) to the sum of probabilities (1) and (2) is a measure of the "sharpness" of the scale, while $1 - \text{probability (3)}$ is a measure of internal consistency. Better scales, thus, should show a high ratio of (1) to (1) plus (2) and a high value of $1 - \text{probability (3)}$. A scale which scored 1 on both dimensions would be perfectly internally consistent and perfectly "sharp." For example, the scale presented in Table 5 has the following properties: Probability (1) = .494; Probability (2) = .471; and Probability (3) = .036. Our sharpness index = .512, and our internal consistency index = .964. This scale appears to be quite consistent, although not terribly sharp. The "dulness" presumably is a function primarily of the low number of items and

secondarily of the unequal distribution of subjects among the perfect types.

RELIABILITY

We can think of "reliability" as the probability that we will get different results if we repeat our operations. Since the results of a scale, as defined by our model, consist of a complex of findings, let us begin by attempting to locate some specific reliability problems rather than thinking about reliability per se.

We shall limit our attention to the question of orderings. While estimates of other findings are possible (e.g., setting confidence limits on the proportion of subjects in scale type X), the heart of the problem is the stability of the orderings of the two sets which make up the scale. Each of the orderings, in turn, can be broken down into $(N^2 - N/2)$ statements about pairs of elements (e.g., A is higher than B; A is higher than C; B is higher than C). If, then, we can arrive at a conclusion about the reliability of any one paired ranking, we will be able to handle any problem of the reliability of the conclusions about three, four, five, six, etc., elements through repetition of our operations.

Let us then consider two elements—"A" and "B"—which we have ranked on the basis of our data. There are two hypotheses to be considered: one, the alternative hypothesis, is that A, say, is higher than B (this was our finding, and it is the stability that we wish to test); the other, the null hypothesis, is either that A and B come from the same area of the continuum or that B is actually higher than A and that sampling error can account for our finding.

All the information we have about A and B can be expressed in a contingency table in which A and B are the "stubs," and elements from the other set are the cell entries. Let us examine such a table (Table 2).

As in Table 1, plus (+) indicates that the element is below the reference point; zero (0), that they are the same; and minus (-), that the element is above the reference

point. For a concrete example, one may think of our living-room study, with elements A and B as the photographs. The subject's response, "My living room is higher than the picture," would be equivalent to +; "My living room is the same as the picture" would be equivalent to 0; and "My living room is lower than the picture" would be equivalent to —.

On a purely deductive basis we may say that the nine possible joint responses differ in their compatibility with the hypotheses we are testing. Thus, response 1 ("My living room is higher than either picture") is congruent with either hypothesis; but response 3 ("My living room is higher than A and lower than B") is inconsistent with the alternative hypothesis ("A is higher than B"), while it is perfectly consistent with the null hypothesis. The possibilities are formalized in Table 3. We find that responses 1 and 9 are consistent with either hypothesis; responses 4, 7, and 8 are consistent with the alternative and inconsistent with the null hypothesis; and responses 2, 3, 5, and 6 are consistent with the null hypothesis but inconsistent with the alternative.

What we propose, then, is simply this: that we limit our consideration to responses 2 through 8, that is, to those having differential congruence with the hypotheses. Then, if we have "significantly" more observations (which we must assume are a random sample from some universe of the elements in the cell entries) consistent with the alternative, let us accept it. The actual significance test used is largely a matter of the research worker's particular situation. If he has predicted the "direction," he should use some satisfactory, one-tailed, non-parametric test; if he has predicted a difference but has specified no direction, he should use a two-tailed test; and, if he has made no prediction, he might set confidence limits on the observed proportion consistent with his alternative hypothesis and accept it if the confidence limits exclude a population proportion of under 50 per cent.

The rationale for our approach is this. If we rank our elements in the course of the analysis in such a way as to minimize error, regardless of the technique used, we will find that all our information has come from the proportion of elements in cells 2 through 9. This, in turn, squares with the fact that if, in the true universe, we could manipulate the order of two items and thus reverse their standing, we would find only these "sensitive" cells changing in their frequencies. In short, we are assuming that the proportions in the "sensitive" cells are an index of relative standing in the universe. If, in addition, we are willing to believe, on

TABLE 3
CONSISTENCY OF JOINT RESPONSES
WITH VARIOUS HYPOTHESES

JOINT RESPONSE	HYPOTHESIS		
	Alter- native A > B	A < B	Null A = B
1.	Yes*	Yes	Yes
2.	No	No	Yes
3.	No	No	Yes
4.	Yes	No	No
5.	No	Yes	No
6.	No	No	Yes
7.	Yes	No	No
8.	Yes	No	No
9.	Yes	Yes	Yes

* Yes = Consistent with the hypothesis; No = Inconsistent with the hypothesis.

the basis of a statistical test, that further samplings from this same universe would preserve this disproportion, we must conclude that the observed disproportion is probably true of the universe and hence that the elements are reliably ordered.

In the case of Guttman dichotomy items, our contingency table is fourfold instead of ninefold, and our test amounts to comparing the sizes of the cells in the minor diagonal of the table. This comparison, of course, is McNemar's test for a significant difference in correlated proportions.³

³ Quinn McNemar, "Note on the Sampling Error of the Difference between Correlated Proportions or Percentages," *Psychometrika*, XII (1947), 153-57. McNemar, in another article, suggests that this test may be used to assess the "difficulty of items" (cf. *Psychological Statistics* [New York: John Wiley & Sons, 1949], p. 206).

Let us consider an example. In a classical study in social psychology Floyd Allport wanted to measure the devoutness of ritual behavior of Roman Catholics.⁴ He took two elements of behavior—"dipping finger in font" and "making the sign of the cross"—for his scale. While he presents all the possible combinations of responses and orders them in a priori fashion (on the basis of interviews with priests and nuns), we may treat them as a scaling problem. The dimension is that of behavioral devoutness; set₁ consists of a sample of persons entering the cathedral in Syracuse, New York. (We

TABLE 4

RELATION BETWEEN RELIGIOUS PRACTICES

"SIGN OF CROSS"	"DIPPED FINGER"	
	Yes	No
	Yes	No
Yes	975	140
No	28	414

will assume, for the sake of illustration, that this is a probability sample of churchgoers.) Set₂ consists of the two elements of ritual behavior. In this context, the datum—"Subject did not dip his finger"—can be taken to mean that "dipping is located above the subject's devoutness on the continuum." From his data⁵ we may make the cross-tabulation shown in Table 4.

If we are willing to assume that dipping the finger into the font is higher on the dimension than making the sign of the cross, we can construct the scale given in Table 5.

Since our two elements have a very healthy association ($Q = .98$), we may assume that they are internally consistent. Whether they are valid, of course, we do not know, except for Allport's interview

⁴ Floyd H. Allport, "The J-Curve Hypothesis of Conforming Behavior," *Journal of Social Psychology*, V (1934), 141-83.

⁵ The tabulation, which involved collapsing one category of "dipping," was made from data presented *ibid.*, p. 151.

evidence. Are the scale positions of the two elements reliable? McNemar's test gives a chi-square value of 73.3 for one degree of freedom, and hence we conclude that the difference between 140 and 28, and hence the scale difference, is reliable. If we had additional religious practices, extensions of our test to their relationships would tell us whether all or parts of our ordering were reliable.

What about the reliability of subject orderings? Again, our argument must be that

TABLE 5
SCALE OF RELIGIOUS PRACTICES

Dipping	Sign of Cross	N
+	+	975
-	+	140
-	-	414
+	error	-
+	-	28

TABLE 6
HYPOTHETICAL RELATIONSHIP
BETWEEN TWO SUBJECTS

BILL BROWN	JOHN SMITH	
	Yes	No
	Yes	No
Yes	34	21
No	2	47

reliability in one set is no guaranty of reliability in the other. For the other set, however, we may conduct a separate series of tests (analogous to the difference between "Q" and "R" approaches in factor analysis) by reversing the "stubs" and cell entries. Thus, if we had a large number of religious practices, we could test whether two specific subjects were reliably ordered by making a new table. A hypothetical example might be as shown in Table 6.

Here we have 34 religious practices shown by Brown and Smith, 21 shown by Brown and not by Smith, 2 shown by Smith and not by Brown, and 47 shown by neither. If the cells in the minor diagonals satisfy McNemar's test, we may conclude that

Brown's behavior is reliably more devout than Smith's. Let us note, however, that such an operation assumes that the practices are a probability sample from the universe of religious practices of the two subjects. In order that sampling criteria be applied to both sets, either for internal consistency or for reliability, it is necessary for both sets to be considered as probability samples of some sort.

Actually, of course, despite the fact that most scaling is aimed at ordering "subjects," we are seldom interested in the reliability of subject orderings. More usually, we are interested in group differences (e.g., differences between ethnic groups in devoutness). We may easily show that groups differ significantly on our scale without assuming that given pairs or triads of people do so also. Here again we note that, in most research designs, the structure of the study leads to certain criteria and not to others. What criteria are to be applied is a function of the design of the research. It is our belief that criteria of "scalability" or "reliability," which do not refer to precise problems within the data, are of little use.

SOME TYPES OF SCALE PROBLEMS

If we accept the proposition that scale evaluation should be in terms of a plurality of tests rather than through some over-all coefficient, it follows that a number of conclusions other than "acceptable" or "not acceptable" may flow from the analysis. In the following paragraphs we should like to sketch four modal types of problems in terms of the criteria they imply.

1. *Scalability*.—Many research problems are limited to the general question, Do my data measure such-and-such a dimension? If this is our only interest, and if we accept the approach outlined here, the answer lies in the assessment of validity and internal consistency. If our elements are significantly associated and the associations are of a "strength" acceptable to the investigator, and if we have evidence of their validity, we may conclude that the elements

are providing similar rankings and on the dimension we had in mind. Whether, in addition, some or all of the elements are reliably differentiated is not part of this question. If they are not, the scale may be of little practical use, but that is a separate question.

2. *Instrument construction*.—If we are interested in the construction of a measurement instrument (e.g., an attitude scale), we may proceed, after we have assessed "scalability," to select a subset from our set (e.g., four items from our battery) which is internally differentiated. The application of our criteria of reliability can be used to reduce an initially large number of elements down to a few, whose spacing on the dimension is optimal. Thus, in our approach, two or three item scales may be justified if they meet our standards. It is our impression that the rule of thumb of Guttman scaling which requires a large number of items is wasteful as well as vague. Too often one makes a scale from a large number of elements and then proceeds to trichotomize the subjects, since the number of cases will not justify further breakdown. Our suggestion would be to choose a smaller number of items whose orderings were known to be reliable.

3. *Class groupings*.—Sometimes we are interested in creating reliable groupings of elements. (For instance, we might want to select out a group of "high" subjects on our scale for further study.) One approach would be to pick certain "index" elements and then to create groups of other elements which are reliably "higher" or "lower" than the index. Through the use of multiple index elements we could create a large number of such ranked classes. Each element within such a class could be shown to be reliably higher than a member of another class without assuming that elements within any given group are ordered. In the trichotomy case, similarly, we could select out groups of elements which were reliably "the same" through modifying our test. In terms of Table 2, this would amount to

testing the number of "5" joint responses against the total number of 2-3-4-6-7-8 responses.

4. *Hypotheses about order.*—The most ambitious problem in scale analysis might well be to test the hypothesis that N elements from a set have a particular order on the dimension. Thus, one might test hypotheses about cultural stages, steps in role transition, or phases in organizational processes. Depending on the exact formulation of the null and alternative hypotheses—a tricky problem—it seems to us that this sort of hypothesis implies the simultaneous satisfaction of tests for reliability and internal consistency for the entire set. That the task is formidable and the odds against a satisfactory conclusion high may be less a drawback of this approach than a tribute to the essential complexity and audacity of such hypotheses.

CONCLUSION

It is our belief that in most research situations the essential issue is not "scalability" in the sense of some over-all tendency for the data to fit a measurement model. Rather, one is usually interested in specific problems, for example, unidimensionality, test construction, classification, and hypothesis-testing about orderings. If so, it is our belief that each specific problem requires a specific combination of criteria for assessing the scale results. We have presented a model which has suggested several specific criteria. It is our hope that the application of these criteria, in situations where the model is applicable, can enable the research worker to draw more precise and appropriate conclusions about the questions raised by his research design.

UNIVERSITY OF CHICAGO

SOCIOECONOMIC STATUS AND DELINQUENT BEHAVIOR¹

F. IVAN NYE, JAMES F. SHORT, JR., AND VIRGIL J. OLSON

ABSTRACT

Assumptions frequently made as to the differential status distribution of delinquent behavior are questioned. Data obtained from samples of institutionalized and non-institutionalized high-school-age pupils in western and midwestern communities revealed insufficient evidence to reject the hypothesis that there is no significant difference in the amount of delinquent behavior of boys and girls in different socioeconomic strata. The slight differences that were found favored the low-status group as often as the high-status group. The findings have implications for etiological theories based upon the assumed status differential in delinquent behavior.

Delinquency is commonly described in the literature as primarily a phenomenon of the lower economic strata.² Such studies, dealing with the relationship between juvenile delinquency and socioeconomic level, have used court records, police files, and other official records of delinquency. These bases are adequate, within certain limitations, for an examination of "official delinquency," but they are unreliable as an index of "delinquent behavior" in the gen-

eral population.³ Estimates of the extent of delinquent behavior in the general population indicate that such behavior may be more evenly distributed in the various socioeconomic strata than official records lead one to believe. Porterfield, for instance, found that college students committed many more delinquent acts than is commonly known and that these delinquent acts were as serious as those which brought other young people, less fortunate economically, into court.⁴ Research by Murphy, based on the case histories of adolescents, yielded similar results.⁵ Wallerstein and Wyle found that, in a group of upper-income individuals, 99 per cent answered affirmatively to

¹ From two studies of adolescent delinquent behavior supported in part by grants from the College Committee on Research of the State College of Washington and the Social Science Research Council.

² Ernest W. Burgess, "The Economic Factor in Juvenile Delinquency," *Journal of Criminal Law and Criminology and Police Science*, XLIII (May-June, 1952), 29-42; Cletus Dirksen, *Economic Factors in Delinquency* (Milwaukee: Bruce Publishing Co., 1948); Bernard Lander, *Juvenile Delinquency* (New York: Columbia University Press, 1954); J. B. Maller, "Juvenile Delinquency in New York City," *Journal of Psychology*, III (January, 1937), 1-25; Earl R. Moses, "Differentials in Crime Rates between Negroes and Whites in Comparisons of Four Socioeconomically Equated Areas," *American Sociological Review*, XII (August, 1947), 411-20; W. C. Reckless, *Vice in Chicago* (Chicago: University of Chicago Press, 1933); Clifford R. Shaw and H. D. McKay, *Juvenile Delinquency and Urban Areas* (Chicago: University of Chicago Press, 1942); T. Earl Sullenger, *Social Determinants in Juvenile Delinquency* (New York: John Wiley & Sons, 1936), 170-80; William W. Wattenberg and J. J. Balistreri, "Gang Membership and Juvenile Delinquency," *American Sociological Review*, XV (December, 1950), 744-52; Paul Wiers, *Economic Factors in Michigan Delinquency* (New York: Columbia University Press, 1944).

³ Clement S. Mihanovitch, "Who Is the Juvenile Delinquent?" *Social Science*, XXII (1947), 45-50; Sophia M. Robison, *Can Delinquency Be Measured?* (New York: Columbia University Press, 1936); Edward Schwartz, "A Community Experiment in the Measurement of Juvenile Delinquency," *National Probation Association Yearbook*, 1945, pp. 157-82; Thorsten Sellin, "The Basis of a Crime Index," *Journal of Criminal Law and Criminology and Police Science*, XXII (September, 1931), 335-56; Jeremiah Shalloo, "Youth and Crime," *Annals of the American Academy of Political and Social Science*, CXCIV (November, 1937), 79-86; Henry D. Sheldon, "Problems in Statistical Study of Juvenile Delinquency," quoted in Edwin H. Sutherland and Donald R. Cressey, *Principles of Criminology* (Chicago: J. B. Lippincott Co., 1955).

⁴ Austin L. Porterfield, *Youth in Trouble* (Fort Worth: Leo Potishman Foundation, 1946).

⁵ Fred J. Murphy, M. Shirley, and H. L. Witmer, "The Incidence of Hidden Delinquency," *American Journal of Orthopsychiatry*, XVI (October, 1946), 686-96.

one or more offenses.⁶ Short's research on criminal behavior in selected groups likewise bears testimony to the fact that delinquent and criminal behavior are by no means limited to the lower economic groups.⁷

Despite the abundance of criticism that has been leveled against the use of official records as an index of delinquent behavior in the various socioeconomic levels, recent studies continue either to confuse "official delinquency" with "delinquent behavior" or to equate the two phenomena. An example of the former is provided by Dirksen, who presents one of the more extreme positions in assessing the role of the eco-

Cohen, in one of the most recent treatments of juvenile delinquency, has said concerning the disparity between official delinquency rates and delinquent behavior rates in the general population: "If many delinquencies of upper-class children fail to find their way into the police and court records, the same is apparently true also of many delinquencies of working-class children, and conceivably even more true."¹⁰ Although Cohen indicates that the best *available* evidence supports the traditional and popular conception of the distribution of juvenile delinquency in the class structure, he calls for research that will make known the extent of delinquent behavior in

TABLE 1
PERCENTAGE DISTRIBUTION OF BOYS IN WESTERN HIGH SCHOOLS AND
STATE TRAINING SCHOOL SAMPLES BY SOCIOECONOMIC LEVEL

SOCIOECONOMIC LEVEL	WESTERN HIGH SCHOOLS		STATE TRAINING SCHOOLS	
	No.	Per Cent	No.	Per Cent
(High) 4.....	114	13.6	6	4.1
3.....	282	33.5	19	13.0
2.....	333	39.6	48	32.9
(Low) 1.....	112	13.3	73	50.0
Total.....	841	100.0	146	100.0

$$\chi^2 = 117.01 \quad P < .001 \quad \bar{C} = .45$$

nomic variable in juvenile delinquency.⁸ His study may indicate the class differential in alleged or official delinquency, but generalizations to delinquent behavior in the general population cannot be made from these results.

Other recent studies utilize such data as obtained from court files and other official records as an index of delinquency.⁹

⁶ James S. Wallerstein and C. J. Wyle, "Our Law-abiding Law-breakers," *Probation*, XXV (April, 1947), 107-12.

⁷ James F. Short, Jr., "A Report on the Incidence of Criminal Behavior, Arrests, and Convictions in Selected Groups," *Proceedings of the Pacific Sociological Society*, 1954, pp. 110-18 (published as Vol. XXII, No. 2, of "Research Studies of the State College of Washington [Pullman, Wash., 1954]).

⁸ Cletus Dirksen, *Economic Factors in Delinquency* (Milwaukee: Bruce Publishing Co., 1948).

the population not judged "delinquent."¹¹

The present analysis (Table 1) shows that, in one of the states from which a sample was drawn, the relationship between socioeconomic status and commitment to the state "training school" is similar to that shown by the studies quoted above. A disproportionate number of the official delinquents come from the lower socioeconomic categories.

After an examination of this literature, it is the opinion of the writers that the use of a measure of reported delinquent behavior

⁹ E.g., Bernard Lander, *Juvenile Delinquency* (New York: Columbia University Press, 1954).

¹⁰ Albert K. Cohen, *Delinquent Boys: The Culture of the Gang* (Glencoe, Ill.: Free Press, 1955), pp. 37-41.

¹¹ *Ibid.*, pp. 170-71.

rather than official records of delinquency will yield results somewhat different from those supporting the traditional conceptions of the status distribution of delinquency. We are not here concerned with etiology but with this question: Does delinquent behavior occur differentially by socioeconomic status?

The present study tests the null hypothesis that there is no significant difference in delinquent behavior of boys and girls in different socioeconomic strata.

Related to the higher growth rate is the lower average age and the smaller proportion of old people in these communities. From a state-wide sample drawn in another study¹² it was found, as would be expected, that horizontal mobility was greater in the present sample than for the state and is presumably greater than that for the country as a whole.

The midwestern data were gathered by a questionnaire soliciting comparable data from 250 boys and 265 girls in Grades IX

TABLE 2

SELECTED POPULATION CHARACTERISTICS FOR WESTERN COMMUNITIES FROM WHICH SAMPLE IS DRAWN, COMPARED WITH THOSE CHARACTERISTICS FOR WESTERN STATE AND THE UNITED STATES*

Selected Population Characteristics	Communities from Which Sample Is Drawn	Western State	United States
Median years of school (twenty-five years old or over):			
Male.....	11.1	11.3	10.0
Female.....	11.1	12.1	10.3
Median age.....	27.5	31.8	31.6
Per cent 65 and over.....	3.0	9.3	8.2
Per cent over fourteen years of age in labor force:			
Male.....	89.1	77.9	79.3
Female.....	32.1	31.6	33.2
Median income—families.....	\$4,515.00	\$3,755.00	\$3,431.00

* *United States Census* (1950). Urban statistics are used for western state and the United States. Comparable data are not available for midwestern communities.

THE RESPONDENTS

The sample.—In this study there are two principal sources of data—selected high-school groups in western and midwestern communities. In the western sample, data were gathered by questionnaire from 2,350 boys and girls in Grades IX through XII in the high schools of three western cities. These cities ranged in size from 10,000 to 25,000. They are thus clearly urban but not metropolitan.

The three western communities sampled in this study differ from the state in which they are located in two significant characteristics. They experienced a much higher growth from 1940 to 1950, and the average income level was higher. (See Table 2.)

through XII in the high schools of three midwestern communities. One of the high schools is located in a suburban residential town with a population of less than 2,500. The second is a rural town of less than 2,500 population. The third is a consolidated high school in a rural township. The population of these three communities has remained fairly stable since 1940.

No samples from large cities or from large non-Caucasian groups are included, and generalizations to such populations must await further research.

¹² LaMar T. Empey, "Relationship of Social Class and Family Authority Patterns to Occupational Choice of Washington High School Students" (unpublished Doctoral dissertation, State College of Washington, 1955).

MEASURE OF DELINQUENT BEHAVIOR

Delinquent behavior in the present study was measured by means of an anonymous delinquency check list administered to adolescents who are not adjudged delinquent¹³ and by a delinquency scale constructed from it. The list is designed to include a broad sampling of juvenile misconduct, though it does not include several of the more serious types of delinquency (e.g., rape, breaking and entering, and armed robbery).

MEASURE OF SOCIOECONOMIC LEVEL

The occupation of the father was utilized as an index of socioeconomic level of the respondent. A combination of the North-Hatt and Mapheus Smith scales was employed. These scales were combined and applied to data gathered from a sample of Washington State high-school students by Empey in a study of occupational aspiration and anticipation.¹⁴

Research in social stratification lends support to the use of occupation as a measure of socioeconomic status.¹⁵ It has the

¹³ The actual items in the delinquency check list are as follows: defied parents' authority, taken things you didn't want or need; "beat up" on kids that hadn't done anything to you; hurt or inflicted pain on someone just to see them squirm; purposely damaged property; taken things under \$2.00 in value; taken things between \$2.00 and \$50.00; taken things \$50.00 and over; driven recklessly in a car; bought or drank intoxicants; used or sold narcotics, homosexual relations; heterosexual relations; taken someone's car without asking; taken part in gang fights; run away from home; had a fist fight; and probation or expulsion from school.

¹⁴ Empey, *op. cit.*; see also LaMar T. Empey, "Social Class and Occupational Aspiration: A Comparison of Absolute and Relative Measurement," *American Sociological Review*, XXI (December, 1956), 703-9.

¹⁵ Raymond B. Cattell, "The Concept of Social Status," *Journal of Social Psychology*, XV (May, 1942), 293-308; Joseph A. Kahl and J. A. Davis, "A Comparison of Indexes of Socio-economic Status," *American Sociological Review*, XV (December, 1955), 317-25; National Opinion Research Center, "The Quarter's Polls—Occupations," *Public Opinion Quarterly*, XI (1947-48), 138-71.

following advantages: (1) Occupation correlates highly with other criteria of class and status position, such as subjective class affiliation, income, educational level, subjective class ratings, and others. (2) Occupation is related not only to income but to values, attitudes, and goals; to a certain extent it determines the social relations among societal members. (3) The use of occupation as a criterion of socioeconomic status makes it possible to correlate a child's delinquent behavior with the socioeconomic level of his immediate family rather than with the demographic area in which he lives. (4) In addition, data on the occupation of the father are generally obtained more accurately from adolescents than income, years of schooling of the parents, value of the home, rental, and other items with which the adolescent may not be familiar.

Comparison of the percentage distribution by major occupational groups for the western sample and the midwestern sample indicated that the differences between the proportions of each sample falling in the several socioeconomic levels were not significant. The two samples are not significantly different as to range or distribution of occupations. The combined occupational prestige scale contains ten categories of occupations, each representing a range of occupations within the total scale. In the present study these ten categories were combined into four status groupings which include the following types of occupations: (1) unskilled and semiskilled labor (e.g., migratory worker to restaurant cook); (2) skilled labor and craftsmen (e.g., housepainter to linotype operator); (3) white collar and small business (e.g., newspaper columnist to owner-operator of a mine); and (4) professional and large business (e.g., interior decorator to United States Supreme Court justice). No attempt was made to classify adolescents who live in families in which there was no adult male. Elimination of this small group (108 out of a total of approximately 2,350 cases) does

not seriously influence the findings of the study. Separate analysis of this group indicates that its members tend to be more delinquent than all others; but, when status is held constant by two independent measures (education of mother and comparison of income with that of "others"), no significant relationship between status and delinquency is found.

FINDINGS

The data were subjected to five tests in an attempt to locate significant differences in delinquent behavior by socioeconomic status. First, four-by-four tables were constructed in which social status was categorized as described above. Delinquent behavior on each item was divided into four categories. Typically these were: (1) did not commit the act; (2) committed the act once or twice; (3) committed the act several times; (4) committed the act very often. The distribution of each delinquent act by social status was tested separately by the chi-square test. Tests were computed separately for boys' and girls' samples, followed by tests in which the boys' and girls' samples were combined for twenty-one delinquency items. This was done separately for the western and midwestern samples. In all, 126 chi-square tests were made with the western and midwestern data.

In the western samples two significant differences were found. Since 63 chi-squares were computed from the western data, three differences significant above the 5 per cent level might be expected to occur by chance.¹⁶ These two differences did not follow any consistent pattern. "Heterosexual relations" were committed most frequently by lower-class boys, but "purposely damaged or destroyed property" was committed most frequently by upper-class boys

¹⁶ This assumes independence of variables and of each of the tests. The assumption in this case is valid except where boys and girls are combined following tests performed on data for boys and girls separately. For a summary of significant differences see Table 3 and comment below.

and girls. In the midwestern samples three significant differences were found. Since 63 chi-squares were computed from the midwestern data, three differences significant above the 5 per cent level might be expected to occur by chance. Furthermore, the three do not follow any consistent pattern. "Taking a car without permission" was committed most frequently by lower-class boys; "running away from home," most frequently by upper-class girls. It was concluded, therefore, on the basis of the chi-square test of the four-by-four tables that there is no reason to reject the null hypothesis.

However, it was possible that the differences in delinquent behavior might be cumulative and that a simple dichotomy of "committed" or "did not commit" the act might reveal differences not apparent in the more detailed analysis.¹⁷ Thus a second test was made in which all delinquent behavior items were dichotomized and in which the relationship of delinquent behavior to socioeconomic status was again tested by the use of chi square. For the western sample seven items were found to differ significantly by socioeconomic status. "Skipped school" and "taken a car for a ride" were most frequently admitted in the lowest socioeconomic category, both in the boys' and in the combined boys' and girls' samples. "Purposely damaged or destroyed property" was most frequently admitted in the highest socioeconomic category, both in the girls' and in the combined boys' and girls' samples. "Heterosexual relations" were most frequently admitted by the boys in the lowest socioeconomic category. In all, 63 chi-square tests were made of the dichotomized delinquency items—21 for girls, 21

¹⁷ The combination of table cells serves to increase the n within each cell. If the distribution of responses of the two samples is similar in adjacent cells, the combination of such cells increases the n per cell without diminishing percentage differences between samples. As a result, differences previously not significant become significant. Total sample n is not increased, but, by decreasing the degrees of freedom, the end result is somewhat similar.

for boys, and 21 for the combined western samples (boys and girls). Seven significant differences are somewhat in excess of the number expected by chance, but not all are consistent. Similarly, in the midwestern sample 63 tests were made with none significant at the 5 per cent level. Of the 126 tests made in the two samples, 7 are significant, which is little if any in excess of the number that might occur by chance.

The possibility still remained that there were significant differences between some of the socioeconomic categories in their degree of delinquent involvement. A third test was therefore made. Percentages were computed from the dichotomized delinquency items, and significance of differences between proportions was computed.¹⁸ Since with four socioeconomic categories, six comparisons are possible, for each item the total number of comparisons is 6×21 , or 126 in each sample.

In the western boys' sample, of the 126 tests of significance of differences between proportions, 6 were found significant. For three delinquency items the offense was committed most frequently by the lowest socioeconomic category; for three, by the middle category. For the western girls' sample, five significant differences were found. Four of these delinquencies were committed most frequently by upper socio-

economic girls and one by lower socioeconomic girls. In the combined (boys' and girls') western sample, eight significant differences were found. Four were committed most frequently by upper and four by lower socioeconomic categories. For the western samples, 378 tests were possible, of which 19 (or 5 per cent) proved significant. At the 5 per cent level of significance this is about the percentage of differences that should appear significant by chance.

Since one category of the midwestern sample is very small, the analysis by significance of differences between categories was completed for the combined sample only. This test of 126 comparisons found two significant differences, one offense being committed most frequently by the lower and one by a middle category.

The third test involved a total of 504 possible tests of significance, of which 21 were found to be significant. At the 5 per cent level of significance this number might be expected to occur by chance. Furthermore, in only one-third of the cases was the act most frequently committed by the lowest socioeconomic status category. On the basis of this third test it must again be concluded that the evidence does not permit rejection of the null hypothesis.

Some patterning of significant differences did occur. In all the tests made for the individual items, a total of 33 significant differences were found, as can be seen from Table 3. These differences were concentrated chiefly in the lower- and upper-class groups, and 26 of the 33 differences concerned five offenses. These offenses and the class groups reporting their higher incidence were: truancy (lower-class children); heterosexual relations (lower-class boys); car theft (lower-class boys); destroying property (upper-class boys); and gang fights (upper-class children). It seems likely that some of these differences are also spurious.

In addition to the tests of individual delinquent behavior items, the distribution of delinquency scale types by socioeconomic

¹⁸ Hovland and Linquest have cautioned against detailed tests following a general test which proved not significant. In each case, however, the caution appears to be directed against using such double tests in a search for support of a positive hypothesis. Hovland is particularly concerned about possible "false validities." We have guarded against false validities by discounting significant differences to the number of 5 per cent of the total tests made. As the tests are employed here, the analysis goes considerably beyond usual practice in exhausting every possibility for disproving our null hypothesis and providing support for the opposed traditional theory. For Hovland's and Linquest's discussions see Carl Hovland, A. A. Lumsdaine, and F. D. Sheffield, *Studies in Social Psychology in World War II*, Vol. III: *Experiments on Mass Communication* (Princeton, N.J.: Princeton University Press, 1949), p. 297, and E. F. Linquest, *Statistical Analysis in Education Research* (Boston: Houghton Mifflin Co., 1940), pp. 296-97.

status was tested for significant differences.¹⁹ Ten tests were made of distribution of scale types by socioeconomic status. Four samples were tested in both the western and the midwestern high-school populations: girls fifteen and younger, girls sixteen and older, boys fifteen and younger, and boys sixteen and older. One test was made in the boys' and girls' training

omic strata, a further test was considered necessary. In the western communities children are required to attend school until sixteen years of age. A check of census data in these towns shows that, in 1950, 97.5 per cent of children aged fourteen and fifteen were attending school. Of those not attending, there were a number with extreme physical and mental handicaps presumably

TABLE 3

SUMMARY OF TESTS OF SIGNIFICANCE BETWEEN SOCIOECONOMIC STATUS AND DELINQUENT BEHAVIOR OF BOYS, GIRLS, AND COMBINED SAMPLES OF THREE WESTERN AND THREE MIDWESTERN TOWNS

SAMPLE AND TEST	No. OF POSSIBLE DIFFER- ENCES	No. OF DIFFER- ENCES SIG- NIFICANT	SOCIOECONOMIC STATUS IN WHICH HIGHEST PROPORTION COMMITTED DELINQUENT ACT*		
			Lower	Middle	Upper
4-4 Table χ^2 :					
Midwestern boys.....	21	2	1	0	1
Midwestern girls.....	21	1	0	0	1
Midwestern combined.....	21	0	0	0	0
Western boys.....	21	1	1	0	0
Western girls.....	21	0	0	0	0
Western combined.....	21	1	0	0	1
2-4 Table χ^2 :					
Midwestern boys, girls, and com- bined.....	63	0	0	0	0
Western boys.....	21	3	3	0	0
Western girls.....	21	1	0	0	1
Western combined.....	21	3	2	0	1
Significance of difference between proportions (<i>t</i> score):					
Western boys.....	126	6	3	3	0
Western girls.....	126	5	1	0	4
Western combined.....	126	8	4	0	4
Midwestern combined.....	126	1	1	1	0
Total.....	756	33	16	4	13

* Act that was significantly different by socioeconomic category.

schools of students sixteen and older. Of these 10 tests, 9 proved non-significant, with $P > .20$ in each case. (See Table 4.) The tenth test (western boys sixteen and over) was significant at the 5 per cent level.

SELECTIVE FACTORS

Not all adolescents are in school. Since the "drop-outs" may be more delinquent than those in school and may be disproportionately recruited from the lower socioeco-

unrelated to the present analysis. "Drop-outs" were, therefore, so few proportionately that they could not affect (considerably) the findings in these age categories. The relationship of socioeconomic status to delinquent behavior was tested within this younger age group separately, and six significant differences were found. Two acts were committed less frequently by the upper-status group, three by the lowest, and one by the middle groups.

THE NON-CLASSIFIED CATEGORY

Not all respondents could be classified in terms of socioeconomic level (28 per cent

¹⁹ For a description of the construction of the delinquency scale see F. Ivan Nye and James F. Short, Jr., "Scaling Delinquent Behavior," *American Sociological Review*, XXII (June, 1957), 326-31.

of boys were "non-classifiable"). For example, "works on a newspaper" was deemed insufficient information for assignment to a particular category. These cases were necessarily omitted from the above analysis. It was considered desirable to know whether this non-classifiable group differed markedly from the group analyzed. A second measure of socioeconomic status—the education level of the father—was employed. Differences in education level of the father in the classified and non-classified groups were tested and found not significant. The two

delinquent behavior scale. Data were gathered anonymously by questionnaire under classroom conditions. Socioeconomic status was determined by the father's occupation, using a combination of the North-Hatt and Mapheus Smith occupational prestige scale.

The data were put to five tests: (1) The chi-square test was applied to the data in four-by-four tables for boys and girls separately and combined. (2) Delinquent behavior categories were dichotomized, and the chi-square test was applied to the data in two-by-four tables. (3) A test of signifi-

TABLE 4
DISTRIBUTION OF DELINQUENT BEHAVIOR SCALE TYPES FOR WESTERN
BOYS TWELVE TO FIFTEEN YEARS OLD BY SOCIOECONOMIC STATUS

SOCIOECONOMIC LEVEL	N	Low*	SCALE TYPES Intermediate†		High‡	
		Per Cent	N	Per Cent	N	Per Cent
(High) 4.	25	18	7	7	16	16
3.	48	34	35	35	28	27
2.	54	39	42	42	40	39
(Low) 1.	13	9	16	16	18	18
Total.	140	100	100	100	102	100
		$\chi^2 = 9.99$	$P < .20$			

* Scale Type No. 1 = no admitted offense.

Scale Type No. 2 = admitted driving without driver's license.

† Scale Type No. 3 = admitted the above items plus defied parents' authority openly.

Scale Type No. 4 = both the above plus petty larceny.

Scale Type No. 5 = all the above plus taking automobile without permission.

‡ Scale Type No. 6 = all the above plus drank alcoholic beverages.

Scale Type No. 7 = all the above plus heterosexual relations.

Scale Types No. 8-15 = all the above plus some or all more than once or twice.

groups were then compared by delinquent behavior scale types, and again differences were found to be not significant. It was concluded, therefore, that the findings were not biased by the exclusion of the non-classified group.

SUMMARY

The null hypothesis was tested that there is no significant difference in delinquent behavior of boys and girls in different socioeconomic strata. The study was conducted in three western communities and three midwestern communities. The population included all pupils in Grades IX through XII. Delinquent behavior was measured by means of a delinquency check list and a de-

licance of difference between proportions was applied to subgroups showing marked differences for the two-by-four tables. (4) A test was made of the distribution of delinquency scale types by socioeconomic status. (5) A separate test was made with adolescents of fourteen and fifteen years of age to minimize the effect of school "drop-outs." The tests employed failed to uncover enough significant differences to reject the null hypothesis.

This study does not attempt to explain the etiology of delinquent behavior, but the findings have implications for those etiological studies which rely upon the assumed class differential in delinquent behavior as a basis for a delinquency theory.

Our data suggest that several *single* measures of socioeconomic status and delinquency are not highly correlated in rural areas and in small towns and cities.

Although present findings are negative, attention is called to the seemingly non-random distribution of significant differences in Table 3. The two middle socioeconomic categories are highest on delinquent

behavior on only 4 tests in contrast to 29 for the highest and lowest categories combined. These seemingly non-random differences may be caused by underreporting of delinquent behavior by the middle classes or by slightly more effective social control and socialization by middle-class parents.

STATE COLLEGE OF WASHINGTON

THE PRESTIGE EVALUATION OF OCCUPATIONS IN AN UNDER-DEVELOPED COUNTRY: THE PHILIPPINES

EDWARD A. TIRYAKIAN

ABSTRACT

A sample of urban and rural Filipino respondents was asked to rank a list of thirty occupations. A high level of agreement in the ranking was noted regardless of the indexes used to differentiate the respondents. Evaluation of occupations in this underdeveloped country was found to be similar to that of industrialized nations. Attention was also given to the frames of reference used in ranking occupations.

Empirical studies of occupational stratification have mushroomed in recent years. Prior to World War II, investigations in this field were pioneered in the United States,¹ but in the postwar years large-scale studies concerning the hierarchical rating of occupations have been conducted not only in America² but also in Great Britain,³ New Zealand,⁴ Australia,⁵ Japan,⁶ and Germany.⁷

¹ G. S. Counts, "The Social Status of Occupations," *School Review*, XXXIII (January, 1925), 16-27; E. S. Bogardus, "Occupational Distance," *Sociology and Social Research*, XIII (1928), 73-81; W. Coutu, "The Relative Prestige of Twenty Professions," *Social Forces*, XIV (May, 1936), 522-29; J. A. Nietz, "The Depression and the Social Status of Occupations," *Elementary School Journal*, XXXV (February, 1935), 454-61.

² National Opinion Research Center, "Jobs and Occupations: A Popular Evaluation," *Opinion News*, IX (September, 1947), 3-13.

³ John Hall and D. Caradog Jones, "The Social Grading of Occupations," *British Journal of Sociology*, I (March, 1950), 31-55.

⁴ A. A. Congalton, "The Social Grading of Occupations in New Zealand," *British Journal of Sociology*, IV (March, 1953), 45-60.

⁵ Ronald Taft, "The Social Grading of Occupations in Australia," *British Journal of Sociology*, IV (June, 1953), 181-88.

⁶ Japan Sociological Society, *Report of a Sample Survey of Social Stratification and Mobility in the Six Large Cities of Japan* (Tokyo, 1952).

⁷ The German study was conducted in Schleswig-Holstein and published in an article in *Der Spiegel*, June 30, 1954. The findings have been summarized in English by Alex Inkeles and Peter Rossi, "Cross-National Comparisons of Occupational Ratings," *American Journal of Sociology*, LXI (January, 1956), 329-39.

Although it is not the purpose of this paper to give a critical résumé of previous studies,⁸ it may be worthwhile to indicate briefly the general findings of these works:

1. There is a very high level of agreement within a country as to the prestige hierarchy of occupations. With minor variations, the agreement holds regardless of indexes (e.g., age, sex, socioeconomic status, etc.) used to differentiate *groups* of respondents, although within a given group individual differences have been noted. Furthermore, at least in the United States,⁹ the prestige ranking appears to be quite stable over time.

2. Respondents of different nations also have a high level of agreement: a detailed cross-national comparison of occupational evaluations in six modern industrialized countries pointed out that there is an extremely high level of agreement, going far beyond chance expectancy, as to the relative prestige of a wide range of particular occupations despite the variety of socio-cultural settings in which they are found.¹⁰

3. The professions, high government posts, and the top positions of the industrial system stand at the summit of the prestige hierarchy; semiskilled and un-

⁸ A. F. Davies, "Prestige of Occupations," *British Journal of Sociology*, III (1952), 134-47; Donald G. MacRae, "Social Stratification," *Current Sociology*, II (1953-54), 20-23.

⁹ Maethel E. Deeg and Donald G. Paterson, "Changes in Social Status of Occupations," *Occupations*, XXV (1947), 205-8.

¹⁰ Inkeles and Rossi, *op. cit.*, p. 339.

skilled occupations are consistently rated at the bottom.

Despite these comprehensive results, the present study was undertaken with the view that there remained some lacunae in empirical sociological knowledge concerning occupational stratification. The previous studies had been limited to countries sharing a common tradition of Anglo-Saxon culture or a relatively high degree of industrialization, or both. We asked what the prestige hierarchy would be in a society not falling into one of these categories. Again, little systematic information has been available regarding the subjective dispositions of respondents in ranking occupations. What frames of reference do respondents employ? Do respondents use the same frames of reference in rating occupations at the top of the prestige hierarchy as they do in placing occupations at the bottom?

The Philippines was chosen as the site for this research because it did not resemble areas previously studied. Essentially an underdeveloped country, with approximately 80 per cent of the people engaged in agriculture, the maintenance of a high birth rate and the reduction of the death rate in the Philippines have created a rapidly expanding population doubling itself every generation. Furthermore, the Philippines has a variety of existent cultural influences: an American administration which lasted for nearly fifty years (1898-1946, with the interruption of the Japanese occupation during World War II); the Hispano-Catholic culture, which took deep roots during three centuries of Spanish occupation; and the older Indonesian and Malay cultures. Data obtained on the evaluation of occupations in such a country would be of value, we felt, in testing the generalizations obtained in previous studies.

The research was done on the island of Luzon, where almost half of the total Filipino population live. Five communities, at varying distances from the capital city of Manila, were chosen for the study. The

first was San Juan del Monte, a municipality located in the immediate suburbs of Manila and having a population of about 32,000 according to the 1948 Census. It is a fairly prosperous town, with a diversity of occupational roles; many of its residents work in Manila, to which it is connected by various means of transportation. The town has had a long history; culturally, it is a Tagalog-Catholic community:¹¹ nearly 100 per cent of the inhabitants are reported able to speak Tagalog and 94 per cent classify themselves as Catholics.

For purpose of comparison four rural communities (barrios)¹² were also selected: Nangka, La Paz, Amacalan, and Santo Tomas. The first three are Tagalog communities; the last is a Pampango-speaking barrio. All four are situated at distances ranging from thirteen to about ninety miles away from Manila, and all four agrarian communities are fairly representative of central Luzon. Rice or sugar-cane cultivation forms the chief basis of livelihood, with tenancy rates (85-90 per cent) being slightly higher than average in this area. Each of the barrios has had either a sharp natural increase in population since World War II or else a high growth potential. None is completely insulated from urban centers, but in each a rural ethos still predominates. Finally, the centripetal attraction of the metropolis—Manila—makes itself felt in all parts of Luzon, and none of the agrarian sites chosen for this study is completely outside this pull.¹³

¹¹ The largest of the Malay groups in Luzon. The Tagalog language has been designated as the official Filipino language, although English is the *lingua franca*.

¹² A barrio is the smallest recognized administrative unit in the Philippines.

¹³ How much this phenomenon is typical in Southeast Asia has been discussed by Norton S. Ginsburg, "The Great City in Southeast Asia," *American Journal of Sociology*, LX (1955), 455-62. For a general treatment of the influence of the city upon agrarian milieus see Robert Redfield, *The Primitive World and Its Transformations* (Ithaca, N.Y.: Cornell University Press, 1953), esp. p. 31.

PROCEDURES

In San Juan del Monte a random sample of electoral precincts was chosen, with a sampling ratio of about 4 to 10. Interviewers selected were college students, bilingual in English and Tagalog, from the University of the Philippines. They were first trained in questionnaire administration and then assigned to a particular precinct. Here they spent some time familiarizing themselves with the social environment. They were then instructed to get as equal a number as possible of respondents living in what would be considered, in such an urban community, as above-average, average, and below-average dwellings. Furthermore, they were to administer only one schedule per household and to interview a person who was or had been gainfully employed, preferably the household head.

Choosing interviewers in the rural areas constituted a problem akin to that in anthropology: finding an informant who is not marginal or deviant to his cultural group. Not only did the interviewers have to have an intimate knowledge of their community in order to find a representative sample and be able to comprehend the purpose of the survey so as to administer the questionnaires but also they could not be objects of suspicion or hostility in their community.

The majority of interviewers finally selected were schoolteachers; in Nangka, however, interviewers were members of the Philippine Rural Reconstruction Movement, a civilian organization dedicated to the improvement of the standard of living in rural areas. Each interviewer answered a sample questionnaire himself and conducted practice interviews which were reviewed by the writer. The selection of respondents was left to the interviewers, but they were asked to obtain, in their assigned quotas of schedules, a sample which would be fairly representative of the occupational distribution of persons in their community.

The selection of respondents was left to the interviewers for several reasons. Prior

to the survey (November, 1954–February, 1955) postwar census data were not yet published. Probability sampling was not undertaken because (1) census tracts are not used in the areas studied, (2) town lists of the inhabitants or of dwelling units were not available, and (3) limited research funds and lack of personnel made an enumeration of household units too expensive and time-consuming.

The questionnaire used was designed to take about one hour in its administration. A list of thirty occupations had been selected which was designed to approximate the following criteria: (1) the occupations should be familiar ones; (2) they should represent a cross-section of the occupational structure of the Philippines, and (3) as many as possible should be the same as, or similar to, occupations evaluated in previous studies. The final list consisted of the following:

- | | |
|----------------------|-----------------------------|
| 1. Barber | 16. Manager of a |
| 2. Bus or jeepney | business company |
| driver | 17. Midwife |
| 3. Carpenter | 18. Office clerk |
| 4. Congressman | 19. Officer in the |
| 5. Construction | armed forces |
| worker | 20. Owner of a <i>sari-</i> |
| 6. Domestic servant | <i>sari</i> store |
| 7. Engineer | 21. Physician |
| 8. Enlisted man in | 22. Policeman |
| the armed forces | 23. Priest |
| 9. Factory worker | 24. Produce peddler |
| 10. Farm tenant | 25. Professional artist |
| 11. Farmer | 26. Road repairman |
| 12. Fisherman | 27. Salesman |
| 13. Gasoline-station | 28. Sugar-cane plan- |
| attendant | tation worker |
| 14. Intermediate | 29. Tailor |
| schoolteacher | 30. University pro- |
| 15. Lawyer | fessor |

A few words might be said about two of the above occupations, which may not be familiar to persons outside the Philippines. Busses and jeepneys are the most widely used forms of land transportation in central Luzon. Most jeepneys are surplus American jeeps used during the military occupation of 1944–45; they have been

converted into taxis which can accommodate as many as eight people. Common in the urban centers, jeepneys are also a frequent means of transportation in traveling from one barrio to another.

Sari-saris are small, independently owned, retail stores found throughout the Philippines; they sell canned and baked goods, soft drinks, and a host of other household items. Since it takes relatively little capital to operate one, many self-employed shopkeepers are *sari-sari* store owners.

For the evaluation of these occupations, urban respondents were presented with a set of thirty 3×5 cards. On the front of each card the occupational title was written in Tagalog; on the back the English equivalent was given.¹⁴ The instructions read:

We have here some cards, each of which has an occupation written on it. If you think these occupations are of different social standing (do not have the same social evaluation, do not have the same social rank or prestige), please arrange them in order of decreasing social standing with the highest rated occupation on top, then the next highest, and so on. Place aside those occupations that you cannot arrange in this sort of order. Please remember that there is no correct way to do this arranging and that we are only interested in your judgment.

Since a major objective of the survey was to seek the frames of reference by which individuals evaluate occupations, each respondent was also asked to state why he had ranked each of the top three occupations and each of the bottom three occupations as he did.

In administering the questionnaire in rural areas, we anticipated that a large number of the respondents might be illiterate and hence unable to sort a deck of cards with written stimuli. To circumvent

this, the following was done: for each occupation a photograph or picture of a typical work situation was clipped on a 3×5 or 4×6 card, on the back of which was written the occupational title. Undoubtedly, this is a somewhat crude procedure requiring greater standardization; however, it proved facile to administer, and interviewers reported not only that respondents were easily able to recognize the occupational role on each picture but also that the pictures added to the enjoyment of the respondent in answering the questionnaire.

FINDINGS

The composition of the sample, by locality and sex of respondents, is shown in

TABLE 1
DISTRIBUTION OF THE SAMPLE
BY LOCALITY AND SEX

Locality	Males	Females	<i>n</i>
San Juan.....	448	79	527
Nangka.....	48	4	52
La Paz.....	21	1	22
Amacalan.....	20	1	21
Santo Tomas....	19	..	19
Total.....	556	85	641
Per cent of total	87	13	100

Table 1. Since the rural areas had a much greater degree of homogeneity than the urban community in such matters as standard of living, dwelling units, and forms of employment, it was not thought necessary to have as large a rural sample as the urban one.

Respondents were grouped according to the number of occupations ranked. The frequency distribution is given in Table 2.

In all, 89 per cent of respondents evaluated all thirty occupations; 92 per cent of all respondents evaluated at least twenty-six occupations, and but 2 per cent could not rank more than five occupations. Of the urban sample, 92 per cent were able to rank all the occupations on the list, against 76 per cent of the combined rural sample. Among both rural and urban respondents there was no marked discrepancy between

¹⁴ The teaching of Spanish was not encouraged during the Spanish occupation. Consequently, very few Filipinos today speak that language, but almost all can readily converse in either Tagalog or English. Hence the schedules were written in only two forms, English and Tagalog.

the socioeconomic status of those unable to evaluate all thirty occupations and those who could do so.

In view of the fact that an overwhelming majority of all respondents was able to rank all occupations, it was thought feasible to concentrate on this group for purposes of data analysis; hence, unless otherwise noted, we have based our ensuing calculations on this category of respondents. Of course, if it were the case that those who did not rank all thirty occupations perceived the occupational structure differently from those who did, then a severe bias would be introduced in the results. However, the rank correlation coefficient (ρ)¹⁵ between those who ranked eleven to twenty-nine occupations and those who ranked all thirty was, for all occupations,

+ .98, showing a very high degree of agreement.

Table 3 shows the rank evaluation of occupations for all respondents ($N = 606$) who ranked a minimum of eleven occupations. This evaluation is practically identical with the ranking of occupations done by respondents who rated *all* the items. The latter ranking showed no ties: "Enlisted man" rated higher than "Owner of a *sari-sari*," and "Barber" higher than "Bus or jeepney driver."

¹⁵ Spearman's rho obtained by the formula

$$\rho = 1 - \frac{6\sum d^2}{n^3 - n},$$

where n is the number of items ranked and d is the difference in ranks assigned to a given item.

TABLE 2
FREQUENCY DISTRIBUTION, NUMBER OF OCCUPATIONS RANKED,
URBAN AND RURAL RESPONDENTS

RESPONDENT	No. of Occupations Ranked						
	0	1-5	6-10	11-15	16-20	21-25	26-29
Urban.....	6	4	4	5	1	8	13
Rural.....	..	4	10	6	1	1	5
Total.....	6	8	14	11	2	9	18
							566

TABLE 3
RANKING OF OCCUPATIONS, ALL RESPONDENTS EVALUATING
ELEVEN TO THIRTY OCCUPATIONS

Occupation	Final Rank*	Occupation	Final Rank*
Physician.....	1	Owner of a <i>sari-sari</i> store.....	15.5
Congressman.....	2	Salesman.....	17
Lawyer.....	3	Tailor.....	18
Engineer.....	4	Fisherman.....	19
University professor.....	5	Carpenter.....	20
Priest.....	6	Farm tenant.....	21
Manager of a business company.....	7	Construction worker.....	22
Officer in the armed forces.....	8	Factory worker.....	23
Intermediate schoolteacher.....	9	Sugar-cane plantation worker....	24
Professional artist.....	10	Barber.....	25.5
Farmer.....	11	Bus or jeepney driver.....	25.5
Midwife.....	12	Gasoline-station attendant.....	27
Office clerk.....	13	Road repairman.....	28
Policeman.....	14	Produce peddler.....	29
Enlisted man in the armed forces	15.5	Domestic servant.....	30

* To obtain the mean rank value of a given occupation, the total sum of ranks was divided by the number of respondents rating the occupation; the final rank represents the position of the mean rank of a given occupation relative to all other mean ranks.

For purpose of computation, the rankings of respondents who evaluated eleven to twenty-nine occupations were transformed by uniform stretching of the ranking scales; this was accomplished by multiplying a particular rank by the factor $30/n$, where n represents the number of items ranked by a given individual. Thus, an occupation given a rank of 20 by a respondent who ranked only twenty occupations would have a converted value of $20 \times 30/20 = 30$.

Rank correlation coefficients of the occupational ratings were computed for various characteristics of the respondents, with the following results:

1. *Age*. Between respondents under forty years of age and those over forty, the rank correlation coefficient of the occupational rankings was $+.99$. The greatest rank difference was in the case of "Farmer" and "Office clerk": respondents under forty rated "Office clerk" three ranks higher (than respondents over forty) and "Farmer" three ranks lower.

2. *Sex*.—Men and women showed an agreement of $+.98$. Women rated "University professor" three ranks higher than men, "Construction worker" four ranks higher, but "Congressman" three ranks lower.

3. *Locality*.—The urban and rural samples had a correlation of $.96$. Urban respondents rated "Fisherman" seven ranks higher and "Factory worker" five ranks lower than did rural respondents. These were the highest rank differences.

4. *Religion*.—Respondents were grouped in terms of four categories: (I) "Roman Catholic," (II) "Aglipayan" and "Iglesia ni Kristo,"¹⁶ (III) "Protestant," and (IV) "Other or none." The average correlation for all six pairs involved was $+.96$; the highest correlation in occupational rankings was between Groups I and II ($.98$), and the lowest between Groups III and IV ($.94$). Priest was ranked fifth by Protestants and sixth by Catholics; its lowest rating came from members of Aglipayan and Iglesia ni Kristo, who gave it a rating of ninth.

5. *Income*.—Respondents were divided into four categories in terms of their monthly household income: (I) ₱150 or

less,¹⁷ (II) ₱151–₱300, (III) ₱301–₱600, and (IV) ₱601 or more. The rank correlation coefficients showed an amazingly narrow range: from $.98$ to $.99$! "Manager" received its highest rating (third) from those earning ₱601 and over and its lowest (seventh) from those in the ₱151–₱300 category.

6. *Education*.—Contrary to what might be expected, this characteristic showed the least variation among groups of respondents. The sample was divided into those who had not gone beyond primary school, those who had done so but not beyond high school, and those who had gone beyond high school. For each of the three possible pairs the rank correlation coefficient was $.99$.

7. *Occupational status*.—First, a division was made between blue-collar and white-collar workers, which yielded a correlation coefficient of $.99$. Next, correlation coefficients were obtained for pairs of specific occupational groups; these are shown in Table 4.

It may be noted that Group X (farm tenants) had the lowest correlation with all other groups, its average correlation being $.94$; the highest correlation for this group was with Group III (farmers and farmowners). The average rank correlation coefficient for all forty-five pairs was $.96$.

8. *Language*.—Respondents were placed into four categories based on language spoken at home: (I) Tagalog; (II) Tagalog and Spanish or Tagalog and English; (III) native languages other than Tagalog; and (IV) Spanish or English.¹⁸ The average ρ was $.95$. The highest correlation in occupational rankings, $.99$, was between Groups I and II; the lowest correlation, $.92$, occurred between Groups III and IV.

To summarize, language and religion appear to be the two indexes which, relative to other indexes used, best discrimi-

¹⁷ At the official rate of exchange the peso (₱) = \$0.50.

¹⁶ These are two native independent churches. The 1939 Census listed about one and a half million persons belonging to the Aglipayan faith in the Philippines. Membership figures of the Iglesia ni Kristo church are not available, but the total is probably not more than 100,000. Although the headquarters of Iglesia ni Kristo are located in San Juan del Monte, not more than 1 per cent of the inhabitants in San Juan belong to that faith.

¹⁸ It was necessary to limit the number of categories in order to have sufficient cases in each group for the analysis of data.

nate respondents in their evaluation of occupations. However, it must be borne in mind that the level of agreement for all indexes is quite substantial in absolute terms.

Regardless of the criterion used to differentiate the sample, "Physician" always had an average rank of 1 (first) and "Domestic servant" an average rank of 30 (last). Theoretically, this might be due to the following reasons: (1) "Physician" received more first-place rankings and "Domestic servant" more last-place rankings than other occupations and (2) the amount of dispersion or scatter in the rankings of

it below "Physician." Likewise, although "Domestic servant" had the lowest average rank of all, six other occupations were ranked last more often.¹⁹

Respondents having ranked all thirty occupations were then divided into two groups, those under forty years of age and those over forty. The standard deviation of the ranks assigned to a given occupation was calculated for both groups. The rank correlation coefficient between the two sets of standard deviations was $+.77$, showing an essential agreement between the two age groups as to the variation in the ranks as-

TABLE 4
CORRELATION COEFFICIENTS OF OCCUPATIONAL RANKINGS,
BY OCCUPATIONAL GROUPS*

GROUP	I	II	III	IV	V	GROUP VI	VII	VIII	IX	X
I.....	—	.97	.96	.98	.99	.99	.98	.96	.99	.94
II.....		—	.95	.97	.96	.96	.96	.92	.96	.92
III.....			—	.97	.95	.96	.97	.94	.97	.97
IV.....				—	.98	.98	.98	.96	.98	.92
V.....					—	.99	.99	.96	.98	.92
VI.....						—	.99	.97	.98	.94
VII.....							—	.97	.99	.94
VIII.....								—	.97	.93
IX.....									—	.94
X.....										—

* Roman numerals designate the following: I, professionals and semiprofessionals; II, managerial, executive, and officials, other than farm; III, farmowners, farm managers, and independent farmers; IV, small business owners, shopkeepers, and other self-employed; V, clerical and sales workers; VI, foremen, craftsmen, and other skilled workers; VII, machine operatives, semiskilled workers; VIII, laborers (including farm), unskilled workers; IX, public service workers (police, fire, armed services, etc.); X, farm tenants.

these occupations might be less than that of other occupations whose average rank placed them near the top or bottom of the list. To the writer's knowledge this problem had not been systematically treated in previous studies. To investigate this, we first considered the frequencies of those occupations most often placed first or last. The results can be summarized as follows: (a) on the whole, occupations most frequently ranked first or last are the same as those whose average rank is near the top or the bottom, as the case may be, but (b) there is no 1:1 correspondence between the number of times an occupation was placed first (or last) and its average rank. Thus, although "Congressman" was ranked first by more persons, its average rank placed

signed to particular occupations.

"Physician" had the lowest standard deviation in both groups, 3.88 and 4.02, respectively. On the other hand, "Farmer" showed the highest standard deviation in each group, 7.45 and 7.13. "Enlisted man," "Farm tenant," and "Congressman" had large standard deviations in both age groups, whereas "Engineer," "Barber," and "Tailor" had relatively low ones. On the whole, the predominantly agrarian occupations ("Farmer," "Farm tenant," "Fisher-man") showed the greatest spread in their respective rankings. This might well be a

¹⁹ "Road repairman," "Produce peddler," "Barber," "Gasoline-station attendant," "Bus or jeepney driver," and "Sugar-cane plantation worker" (in order of decreasing frequencies).

common feature in societies in the process of transition from an agrarian-based economy into an industrial one: the low purchasing power of these occupations, on the one hand, and their traditional status as the "backbone" of the country, on the other, help to make their prestige ambiguous. The greater the ambiguity, the greater will be the spread in the prestige ranking of a given occupation. Thus, "Congressman" has a more ambiguous prestige position than "Physician": to some people a congressman is a great source of power and influence, a sort of collective representation of the society; but to other respondents he is a man subject to graft, corruption, and idle talk.

What appears to be of significant interest is that the average standard deviation of the ranks of an occupation is rather high for both age groups. Previous studies have tended to emphasize the very high agreement in the over-all rank or score of occupational roles; our data on the standard deviations suggest that there is also noticeable variation among individual respondents as to the relative position of any given occupation. To account for this, it seems plausible that respondents (at least in the Philippines) have a plurality of prestige reference systems, indicative of a plurality of value orientations. If everyone shared the same prestige frame of reference, there should have been for each occupation a relatively small standard deviation (as a measure of dispersion); this, however, does not appear to be the case.

To see whether, in fact, different respondents do use different frames of reference in evaluating occupations, the questionnaire asked, as we have said, for the reasons why the top three and the bottom three occupations were placed in those positions. It was possible to group the various reasons into several general descriptive categories, which can be considered as frames of reference. The results are shown in Table 5, which shows the percentage distribution of reasons given for occupations ranked first.

The "power" (or "influence") of a "Congressman" (by far the most "popular" first-place choice) was mentioned more than any other reason by rural respondents ranking it first; urban respondents, however, who ranked that occupation at the top, mentioned "service to the country," etc. "Service to the country" was also cited most frequently among both rural and urban respondents ranking the following occupations first: "Physician," "Farmer,"

TABLE 5

PERCENTAGE DISTRIBUTION OF FRAMES OF REFERENCE FOR OCCUPATIONS RANKED FIRST

Descriptive Category	Per Cent of Answers
Service to the country, the community, or to mankind in general.....	25
Income, economic security, standard of living.....	18
Social prestige in the community.....	14
Education and other requirements needed.....	11
Power or influence of the occupation...	5
Nobility of the work.....	4
Service to others (to individuals or specific groups).....	4
Reference group (respondent is, has been, or expects to be in the occupation or knows someone in it).....	3
Opportunities for advancement, springboard to other occupations.....	2
Independence from other persons.....	1
Moral character of persons in the occupation.....	1
Other reasons or reasons not classifiable	10
"Don't know" or no reason given.....	2

"Officer," or "Priest." For "University professor" and "Lawyer" the frame of reference most commonly cited was "education and other requirements needed"; finally, the economic frame of reference ("income," etc.) had the highest frequency of responses for those rating "Manager" or "Engineer" at the top of the prestige hierarchy.

At the other extreme of the prestige hierarchy we find the frames of reference used by respondents in ranking an occupation last (see Table 6). Essentially, these are the converse of those used in ranking

occupations first: "low income," "degradation of work" (instead of "nobility of the work"), and so on. However, there are some differences in the frequencies with which these frames are used. The economic frame of reference was used more than any other by persons ranking last the follow-

ments, etc.) was used more than any other.

It is interesting to note that "service to the country," etc.—the most cited frame of reference in rating occupations first—was not paramount in ranking an occupation last; 25 per cent of the respondents used it in the first instance, but only 7 per cent in the second. On the other hand, "ignobility of the work" was the second most common frame of reference in ranking occupations last, whereas "nobility of the work" was only the sixth most frequent frame in evaluating occupations first.

It also appears that a large number of respondents employ various frames of reference in evaluating occupations. In ranking the top three and the bottom three occupations, many persons gave as many as four or five separate reasons. A frequent pattern was this: An individual placed three occupations at the top of the hierarchy on the basis of their service to the country (or to the community or to mankind in general), but he selected the bottom three occupations in terms of their economic status. In only very few cases did respondents use only one frame of reference in evaluating the whole prestige hierarchy.

TABLE 6

PERCENTAGE DISTRIBUTION OF FRAMES OF REFERENCE FOR OCCUPATIONS RANKED LAST

Descriptive Category	Per Cent of Answers
Low income, economic insecurity, low standard of living.....	24
Degradation or ignobility of the work..	14
Social prestige in the community.....	11
Absence of education and other requirements.....	9
Dependence on others; no freedom of expression.....	9
Poor working conditions.....	7
Little service to the country, etc.; dispensable work.....	5
No opportunities for advancement.....	5
Moral character of persons in the occupation.....	3
Little service to others.....	1
Personal dislike for persons in that occupation.....	1
Other reasons or reasons not classifiable "Don't know" or no reason given.....	10
	2

TABLE 7

	United States	Great Britain	New Zealand	Japan	Germany
Philippines.....	18	10	10	14	13
Total number of occupations in reported study.....	90	30	30	30	38

TABLE 8

	United States	Great Britain	New Zealand	Japan	Germany
Philippines.....	.96	.96	.96	.93	.83

ing: "Produce peddler," "Gasoline-station attendant," "Bus or jeepney driver," "Barber," "Factory worker," "Fisherman," and "Sugar-cane plantation worker." Among respondents ranking last either "Road repairman," "Construction worker," or "Enlisted man" the factor of "poor working conditions" (hazards, exposure to the ele-

Our final consideration is to relate the Filipino data to that of previously reported studies. In the present study we chose those occupations deemed most similar or identical to those used in the United States (the NORC study), Great Britain, New Zealand, Japan, and Germany. In Table 7 is shown the number of similar or

identical occupations rated between the Philippines and these countries.

Since the Philippine study used *ranks* and the others (with the exception of Germany) *scores*, the scores were converted into ranks in order to make comparison possible. Rank correlation coefficients were computed for the comparable occupations, with the results shown in Table 8.

The average correlation is .94, indicating a marked agreement between the Philippines and other countries in prestige evaluation of occupations. This high agreement is all the more interesting, since the Philippines is essentially a non-industrial, non-Anglo-Saxon country. Even if the Filipino rural and urban samples are compared separately with such an industrialized so-

ciety as the United States, their respective correlations are still very high: .93 and .955.

One may conclude from this that the Philippines, primarily an agricultural-underdeveloped area, shares the same pattern of occupational evaluation as urban industrial and/or Anglo-Saxon countries: The professional group is given the highest over-all rating, followed by the executive roles of the industrial system; in the middle of the hierarchy are to be found clerical and sales workers, small business owners, agrarian and skilled occupations; finally, at the bottom of this stratification of occupational roles are to be found the semiskilled and unskilled occupations, in that order.

PRINCETON UNIVERSITY

MAX WEBER'S TWO CONCEPTIONS OF BUREAUCRACY¹

HELEN CONSTAS

ABSTRACT

Weber operated with two conceptions of bureaucracy—the legal-rational and the charismatic—which he never fully clarified. Consequently, he failed to see that a charismatic bureaucracy could be a ruling class, as in pharaonic Egypt, Incan Peru, and Soviet Russia. An analysis of the historical evidence suggests that charismatic bureaucracies do not ultimately transform themselves, as Weber assumed, into legal-rational types but that they remain totalitarian structures, deviating significantly from the ideal type of bureaucracy. Charismatic bureaucracies are ends themselves and irresponsible; legal-rational bureaucracies, however, can be democratically controlled and are more rational.

This paper is part of a larger study of bureaucratic societies, that is, a study of societies in which a bureaucracy is the ruling class. The investigation of these societies led the author to analyze such historical examples as pharaonic Egypt and Incan Peru as well as the most important modern example of bureaucratic society—the Soviet Union. In all these cases certain institutional fundamentals were found to exist: the state exercised total control over social, political, and economic life; private property was either weak or absent; and the economy was administered by a bureaucratic ruling class which, through its monopolistic control of the state, was dominant over all other sectors of society and controlled them in classical totalitarian fashion.

Investigation of these historical examples led to the conclusion that here was a quite different type of social structure—one that could not be classed as slavery, feudalism, or capitalism but which constituted rather a type of its own, a type termed best "bureaucratic society," one that contained identifying structural features differentiating it profoundly from the classical social structures familiar to the West.

Naturally, any consideration of bureaucratic society must sooner or later examine the concept of bureaucracy as it has been developed in sociology to date. This means, in practice, consideration of the contribu-

tion of Max Weber to our understanding of the phenomenon of bureaucracy.

Max Weber is well known, among other reasons, for his pioneer work on the problem of bureaucracy. To this day Weber's analysis of bureaucracy remains the theoretical framework within which most empirical research on this subject is pursued. In addition to its profound effect upon subsequent investigations in this field, Weber's analysis is important in its own right for the understanding it offers of Weber's system of thought—a system in which bureaucracy is a key concept. Thus, from a number of points of view, both theoretical and practical, Weber's ideas about the nature of bureaucracy bear close review.

It is the thesis of this paper that Weber actually operated with two different conceptions of bureaucracy, never clearly distinguished from each other. He did realize the difference between them and, at times, tried to explain it, but his own presuppositions made this differentiation difficult, and death intervened before he succeeded in clarifying his thought in this area. The penalty was that Weber was precluded from seeing bureaucracy as a ruling class and was limited rather to the view that bureaucracy was always a tool in someone else's hands, always a means and never an end in itself. We believe that, if bureaucracy is clarified on the basis of historical genesis (a task Weber began but did not complete), it will be seen to fall into two fundamentally different types: (1) a legal-rational staff func-

¹ Paper delivered at the Thirty-fourth Annual Institute, Society for Social Research, University of Chicago, May 30, 1957.

tioning within a pluralistic power structure (bureaucracy as a means, that is, democratic bureaucracy) and (2) a totalitarian organization resulting from the institutionalization of charisma in a bureaucratic direction (bureaucracy as an end in itself, that is, totalitarian bureaucracy). In the latter case, bureaucracy has "come into its own" and constitutes a ruling class with a monopoly of power. In our own day the social structure of Soviet Russia presents the clearest example of this. In Russia the social pyramid is capped by a bureaucratic ruling class which came to power through the routinization of charisma (in this case, the Bolshevik movement) in a bureaucratic direction. Such a bureaucracy is fundamentally different from the legal-rational staff of a state (i.e., a civil service) or the industrial bureaucracy required by corporations under conditions of modern technology. Weber, however, who was influenced by liberal, unilinear historical constructions,² assumed that in the long run a charismatic bureaucracy would transform itself into the legal-rational type. This paper will try to show, through an analysis of the historical evidence Weber himself presented, that there is little basis for this assumption of transformation and that, in that case, these two types of bureaucracy should be seen as radically different historical forms.

WEBER'S EVOLUTIONARY BIAS

To understand Weber's ideas about bureaucracy, it is necessary to begin with the framework of his political sociology in which the concept of bureaucracy finds its place. Weber felt that all power requires a belief in its legitimacy if it is to become stabilized. In Section III of *Wirtschaft und Gesellschaft*, accordingly, he set up his famous typology of the grounds on which a claim to legitimacy may be based. The first is the legal-rational basis, in which legitimacy rests on "a belief in the 'legality' of

patterns of normative rules and the right of those elevated to authority under such rules to issue commands. . . . Obedience is owed to the legally established impersonal order."³ The staff typically related to such a legal-rational order is a bureaucracy. The second of the possible bases for legitimacy is traditional. Here legitimacy rests on an "established belief in the sanctity of immemorial traditions and the legitimacy of the status of those exercising authority under them. . . . Obedience is owed to the person of the chief who occupies the traditionally sanctioned position of authority and who is (within its sphere) bound by tradition."⁴ The staff logically related to such an order is a feudal staff, either in its purely patriarchal form or in the more developed form of a patrimonial staff (i.e., hereditary). The third possible basis for a belief in the legitimacy of an order is charismatic. Here legitimacy rests on the devotion to the specific and exceptional sanctity or heroism of an individual person and of the normative patterns or order revealed or ordained by him.

Charisma is, of course, another key concept in Weber's sociological system and probably one of his best-known ideas. Being unstable in the extreme, charisma must necessarily transform itself ("routinize itself," as Weber said) in order to serve as a durable basis for a political order. The routinization or institutionalization of charisma may proceed in either of two directions: (1) the hereditary line, in which charisma transfers itself from the original person in whom it was believed to inhere to someone designated as the charismatic leader's successor, typically a hereditary successor, or (2) charisma may attach itself to the office and not the person. This latter road, according to Weber, leads into a bureaucratic order on an (eventual) legal-rational basis.

² See Hans Gerth and C. Wright Mills, *From Max Weber: Essays in Sociology* (New York: Oxford University Press, 1946), Preface, p. 51.

³ Max Weber, *The Theory of Social and Economic Organization* (New York: Oxford University Press, 1947), p. 328.

⁴ *Ibid.*, p. 328.

Thus in Weber's view, bureaucracy may come about by either of two routes. Either a bureaucracy is built up as the logical staff of a legal-rational order (with all this implies about impersonality, rationality, technical competence, etc.), or it comes about as the result of the institutionalization of charisma in a bureaucratic direction rather than a hereditary one. The original charismatic "staff" (hardly a staff in the proper sense of the term, since it consists of disciples with no status or office) is transformed into a hierarchy of charismatic offices. The Catholic church provided the most famous example of this latter possibility up until Weber's day.

The problem arises whether these two types of bureaucracy, each originating by its own route and resting originally on quite different grounds of legitimacy, are equivalent. But one has only to compare a corporate board of directors with the Catholic College of Cardinals or with the Communist Presidium (formerly Politburo) to appreciate the enormous differences between charismatic and non-charismatic bureaucracy. To put the matter more sharply, offices under a legal-rational order are typically not charismatic, and claims to moral authority are limited; offices resulting from the routinization of charisma in a bureaucratic direction, however, are immediately charismatic, and moral elements are therefore always immediately present. (To be sure, even a legal-rational bureaucracy rests, in the last analysis, on charismatic grounds, but the charismatic elements, while ultimately present, do not form the immediate core; rational considerations and procedures do.)⁵

In the case of a charismatic bureaucracy, however, with its hierarchy of immediately charismatic offices, charismatic elements

(i.e., non-rational elements) overshadow and sharply limit the use or area of rationality. This has a number of very important consequences, and I would like to call attention to at least two which seem particularly crucial for the bureaucratic structure itself.

The first consequence involves the fate of science. The development of science is surely an important adjunct of rationality, since rationality rests on scientific understanding. But science cannot develop as fully in a charismatic situation as it can under conditions of greater intellectual freedom (i.e., within a pluralistic power structure where scientific work may be pursued as an end in itself). The development of the Soviet *sputnik* does not negate but rather confirms this view, for it underscores that Soviet science does not enjoy autonomy but that its progress is dependent upon political and ideological factors. Science-as-an-end-in-itself is impossible within any immediately charismatic structure. The aims of science in such a case are necessarily subordinate to charismatic claims to the truth (i.e., to ideological elements). This is to be expected, since, after all, charisma is the antithesis of rationality and the irrational force par excellence.

A second consequence of the sharp limitation of the area of rationality in a charismatic bureaucracy centers on the problem of bureaucratic recruitment. Just as the full range of functional requirements for scientific work is not present in charismatic bureaucracies, so, too, the prerequisites for strictly bureaucratic recruitment are impaired. To the degree that charismatic elements are present, ideological commitment, as well as technical competence, must necessarily figure in bureaucratic recruitment. It may even supersede it. Hence, purges, orthodoxy, and hewing to the party line will inevitably arise at every level in a charismatic bureaucracy and in the most diverse fields of work.

To the degree, then, that a hierarchy of offices stems from the institutionalization of charisma rather than from a legal-rational

⁵ "Indeed, Weber's fullest treatment of legitimacy leaves no doubt that there is no *legitimate* order without a charismatic element. . . . Similarly, in a rational bureaucratic structure there must always be a source of the legality of its order which is, in the last analysis, charismatic" (Talcott Parsons, *The Structure of Social Action* [New York: McGraw-Hill Book Co., 1937], p. 665).

set of rules governing the use and exercise of political power, to that degree is its ability to act primarily or purely on the basis of rational considerations impaired. Charisma does not disappear in the course of its routinization. It merely transforms itself. The question then becomes: How far is it transformed and in what direction?

In this light let us consider the history of the Catholic church, the most outstanding example of the possibility of the transformation of charisma into bureaucratic organization known to Weber, for he died before the Soviet bureaucracy had become fully developed (although he sensed the tremendous importance of Bolshevik power and followed its course with the greatest interest up to his death in 1920). The degree to which rationality is effective in the total structure of the Catholic church is limited by the inherent and immediately charismatic nature of its offices—a charisma still immediately present, let us note, after almost two thousand years! Thus, the Pope can and does promulgate new dogma, speaking, of course, in his charismatic office as Pope and not as a private person. But the charismatic character of the church's offices is not confined to the bureaucratic apex alone. It operates at every level. The church itself long ago established the charismatic nature of its offices, not its personnel, in the fight against the Donatists, in which the church successfully maintained that the legitimacy of a sacrament has nothing to do with the character of the individual administering it. It is rather a function of the office held. The office of a priest, which qualifies him and not the layman to say Mass, is obviously a charismatic one. Thus, the Catholic church is to be seen as a vast charismatic bureaucracy monopolizing the means of grace.

Although Weber discussed two alternative genetic bases for the growth of bureaucratic structures, and what would seem to be, therefore, two different types of bureaucracy, he himself did not view them as essentially different at all. Instead he saw them in terms of a system of historical continu-

ity; or specifically, he saw them from an evolutionary standpoint, and, therefore, as two examples—primitive and advanced—of the same basic phenomenon: bureaucracy. The ideal-type construct of bureaucracy created by Weber was one in which the staff was based on a legal-rational order, and the typical features of Weber's bureaucratic model were held to be most nearly approximated by historical cases with such a basis. This ideal type has been followed by subsequent researchers almost without question. The result is that Weber's criteria for the choice of officials and their bureaucratic functioning have become standard in all discussions on this topic. Let us review these bureaucratic criteria, making a mental note as we do where and to what degree they are inapplicable in the case of charismatic bureaucracy. His criteria are stated as follows:

1. They are personally free and subject to authority only with respect to their impersonal official obligations.
2. They are organized in a clearly defined hierarchy of offices.
3. Each office has a clearly defined sphere of competence in the legal sense.
4. The office is filled by free contractual relationship. Thus, in principle, there is free selection.
5. Candidates are selected on the basis of technical qualifications. In the most rational case, this is tested by examination or guaranteed by diplomas certifying technical training, or both. They are appointed, not elected.
6. They are remunerated by fixed salaries in money, for the most part with a right to pensions. Only under certain circumstances does the employing authority, especially in private organizations, have the right to terminate the appointment, but the official is always free to resign. The salary scale is primarily graded according to rank in the hierarchy; but in addition to this criterion, the responsibility of the position and the requirements of the incumbent's social status may be taken into account.
7. The office is treated as the sole, or at least the primary, occupation of the incumbent.
8. It [the office] constitutes a career. There is a system of "promotion" according to sen-

iority or to achievement, or both. Promotion is dependent on the judgment of superiors.

9. The official works entirely separated from ownership of the means of administration and without appropriation of his position.
10. He is subject to strict and systematic discipline and control in the conduct of the office.⁶

According to Weber, this bureaucratic type of organization is, in principle, applicable to a great many areas of social, political, economic, military, and religious life. It may be found in profit-making enterprises as well as in charitable institutions. Bureaucracy—and Weber does not qualify the term in any way—is the organizational form of the Catholic church and, equally, the staff of the modern capitalist corporation. Inasmuch as Weber believed, or more correctly assumed, that a charismatic bureaucracy would eventually lead into a bureaucratic order on a legal-rational basis, he did not elaborate on any fundamental differences that might exist between these two types of bureaucracy. Weber simply took it for granted that a charismatic bureaucracy is merely a less developed form of bureaucratic organization. This is one of the few remaining elements of evolutionary, “progressivist” thought in his system, but it is a rather crucial one.

Actually, Weber’s assumption as to the eventual historical transformation of charismatic bureaucracies into democratic bureaucracies derives from an even more fundamental assumption on Weber’s part—that historical life exhibits a growing rationality, the famous “disenchantment of the world.” The growing rationality of life, which Weber believed to be an irreversible historical trend, is again merely another of those evolutionary biases to which Weber was still subject, although he did so much to destroy any unilinear, evolutionary schema of history. At any rate, in these days no one will easily speak of the growing rationality of the world. Paradoxically, one of the most

important of Weber’s contributions to sociology has been his insistence on the fundamental and inescapable area of irrationality in social life at all times.

HISTORICAL EXAMPLES OF BUREAUCRACY

It was decisive for Weber’s approach that he prove two things: (1) that his approach to bureaucracy could account for the significant deviations from the ideal type which charismatic bureaucracies so clearly exhibit and (2) that historical transformations of charismatic bureaucracy into legal-rational bureaucracy can and do in fact occur. It is my contention that Weber’s approach to these two problems is inadequate. Let us look at the historical data which Weber discussed and analyzed in this connection. Here, for example, is his list of the “historical examples of rather distinctly developed and quantitatively large bureaucracies”:

(a) Egypt, during the period of the New Empire, which, however, contained strong patrimonial elements; (b) the later Roman Principate, and especially the Diocletian monarchy and the Byzantine polity which developed out of it and yet retained strong feudal and patrimonial elements; (c) the Roman Catholic Church, increasingly so since the end of the 13th Century; (d) China, from the time of Shi Hwangti⁷ until the present, but with strong patrimonial and prebendal elements; (e) in ever purer forms, the modern European states, and, increasingly, all public corporations since the time of princely absolutism; (f) the large modern capitalist enterprise, the more so as it becomes greater and more complicated.⁸

Weber himself makes a distinction among these various historical examples, classing the first four together and distinguishing them from the last two. Investigation of his treatment of these two groups of examples will show, however, that, although he is aware that a difference exists, on the basis of his assumptions, he cannot really locate

⁷ “Shih Huang-ti” in the Wade system of transliteration more generally followed in England and America.

⁸ Gerth and Mills, *op. cit.*, p. 204.

⁶ Weber, *op. cit.*, pp 333–34.

these differences, and he is unable to explain them adequately.

Weber considers pharaonic Egypt, the late Roman Empire, the Catholic church, and the long course of Chinese history as examples of more primitive bureaucratic structures, and he counts the modern state and modern capitalist enterprises as examples of more developed or advanced bureaucracy. The reason Weber gives for this difference—not of type but merely of degree, let us note—is surprisingly a rather technical one having to do with the form of compensation of the officials in each group. Officials of the first group tend to be compensated in kind, not money, and such a form of payment to officials tends to transform a rational, impersonal, efficient bureaucratic structure into one dominated by private, personal, and even hereditary claims. "The allocation of fixed incomes in kind, from the magazines of the lord, or from his current intake, to the officials easily means a first step toward the appropriation of the sources of taxation and their exploitation as private property."⁹ Payment in kind, so Weber's argument runs, opens the way to the private appropriation of the sources of taxation and their exploitation as private property. And he views the development of private property as inimical to the development of advanced bureaucracy. But an examination of the historical evidence suggests that the direction of causality is rather the reverse: that is, the growth of large-scale bureaucracy is inimical to the development of private property, and, in every instance, large-scale bureaucratic organization has inhibited all but the weakest development of private property.¹⁰

Let us state here that we quite agree with Weber that the first four historical examples form one group and the last two an-

⁹ *Ibid.*, p. 205.

¹⁰ For a detailed and masterful discussion of the relation between bureaucracy and property see Karl Wittfogel, *Oriental Despotism* (New Haven, Conn.: Yale University Press, 1957), esp. chap. vii, "Patterns of Proprietary Complexity in Hydraulic Society."

other. The problem is to discover why there are two kinds of bureaucratic structures and what accounts for the difference between these two groups. Weber treats the difference in the light of the influence of the development of a money economy. We suggest, however, that the difference really derives from the fact that the first four historical examples (Egypt, China, Rome, and the Catholic church) are bureaucracies which developed from the routinization of charisma in a bureaucratic direction, while the last two (the modern state and the modern capitalist corporation) are both examples of bureaucracies which developed as staffs for legal-rational orders and not from charisma. In addition, the first four examples presented by Weber never became related to legal-rational orders but continued to remain charismatic bureaucracies throughout the course of their historical lives. In other words, the first groups of bureaucracies were structures which contained immediately predominating charismatic elements. It is interesting to note, too, that at least three of these charismatic bureaucracies (the Catholic church, Egypt, and China) were of extremely long duration, persisting through numerous centuries, so that any transformation likely to occur certainly had sufficient opportunity to do so.

Of the group classed as primitive bureaucracies, Weber particularly singled out pharaonic Egypt as typical, for in ancient Egypt we have a clear case of a bureaucracy compensated in kind. The historical records attest to that fact abundantly. However, if we go on to ask whether Egypt, on that account, ever took the first step toward the development of private property, as Weber thought both likely and easy, we do not find any indications of such a trend. Payment in kind continued through dynasty after dynasty without ever leading to the emergence of private property to any significant degree in ancient Egypt. Weber's assumption about the inner connection between payment in kind and the growth of private property is simply not borne out by

the results of Egyptological research. On the contrary, the publication of the Wilbour Papyrus in 1948 provides documentary proof that the pharaonic state, by both direct and indirect means, "owned" or controlled the land and other economic activities of Egypt.¹¹ Real private property in land never developed; the state remained supreme in all social and economic affairs, and, in the last analysis, legal claim to all the land of Egypt rested with the Pharaoh. Land could be rented from the state or its temples, but it could never be bought. There are no documents of sale, mortgages, etc. In short, there was no real private property in land. There also was no urban mercantile class, because Egypt was, like the Soviet Union, a statist or nationalized economy, and economic affairs were in the hands of the state. The only private property which existed lay in petty household goods (pots, linens, boxes, slaves who were household servants, etc.), not in landed property upon which the taxation system was built. Thus, Weber's assumption that the payment of officials in kind rather than money will lead to the development of private property, through the private appropriation of the sources of taxation, is not supported by the historical facts. There is simply no inherent connection between payments in kind and the genesis of private property. This is made even clearer when we turn our attention to the other three examples in our first group of four.

Without going into all the details of the matter, the following points may be made. Both the Diocletian monarchy and the Byzantine polity had an excellent coinage system. For over a thousand years imperial Byzantine coins were minted, and, if payment was made to officials in kind, it could not have been from any technical inability to pay in money, for in Byzantium, unlike pharaonic Egypt, state coinage was known and practiced. (Egypt, however, had to wait

for the advent of the Greeks to know a true coinage system.)

It would not seem, therefore, that Weber has made a convincing case or that his explanations are adequate, even though they may be relevant. His explanations for the different types of bureaucracy, although they may be part of the picture, do not seem to give us the basic cause and suggest an inadequate grasp of the phenomena. It is more than likely, for example, that the presence or absence of coinage is a social effect at least as much as it may be a cause. That is, if a people need a coinage (and are already familiar with metal-working techniques), they will develop (or borrow) one. Egypt had no need for coinage. The other three cases of the first group had a coinage system. Clearly, the presence or absence of coinage (as against payments in kind) would seem to be beside the point. One must look elsewhere for an explanation of why these two groups of bureaucracies differ so.

Simple reflection on these historical cases suggests that something quite different from the development of money is at the root of the dissimilarity of the two bureaucratic groups. The simple fact is that the first four examples are charismatic in origin, while the last two are related to a legal-rational order from their inception. Let us briefly review the charismatic bases of the members of the first group. Egypt's political basis is well known—the Pharaoh was a god. The Roman Principate marked precisely the end of the legal-rational order and the introduction of charismatic elements; Caesar worship had begun (not to mention the religious charismatic elements on the scene); Augustus was apotheosized, and henceforth the emperor required worship as a divinity. Weber himself analyzed the Roman Catholic church as a charismatic bureaucracy, able to make claims of a sort that no board of directors or president of a capitalist corporation could or would. In China the theory of divine kingship prevailed; the emperor was the "Son of Heaven" and ruled with the mandate of heaven. Talcott Parsons has

¹¹ Alan Gardiner, *The Wilbour Papyrus* (3 vols.; New York: Oxford University Press [for the Brooklyn Museum], 1948).

given an admirable exposition of Weber's view of China:

The Chinese imperial "state" had two main aspects. On the one hand, it was a theocracy, in a sense differentiating it radically from any Christian political structure. The Emperor was the "Son of Heaven" and was conceived of as the principal intermediary between the divine order of things and that of human society. A break in the harmony of the latter could be laid to his ritual inadequacy. Thus the Emperor formed the center of the ritual interests of China. But this religious aspect did not lead, as it might have, to the placing of political power in the hands of a hereditary priesthood of which he was the head. Under the Emperor stood a special class of bureaucratic administrators, the mandarins. In certain respects the Chinese political system carried bureaucratic principles through to a point scarcely reached anywhere else, but in others it differed radically from the type important for bourgeois capitalism.¹²

Here, again, we have a case of charisma institutionalizing itself along the bureaucratic path and creating a class of administrators—the mandarins—as a result. Yet the whole structure differs in key ways from the classical type of bureaucracy envisaged by Weber as specifically modern. In the Chinese case the charismatic elements were never fully rationalized, although the process of bureaucratization occurred. Why? Paper money is a Chinese invention, so we shall hardly expect to find any monetary reason in this case. Weber's answer is that charisma became traditionalized instead. To crack the traditional mold, some new form of charismatic breakthrough (some new charismatic irruption) would have been required, and this could not, in the nature of the case, be provided by Confucianism. To say the least, this is a rather circular bit of reasoning on Weber's part. Why was not charisma able to develop in a more rational direction in the first place instead of in a traditional one?

At this point we come up against another

of Weber's fundamental assumptions. Quoting Parsons again:

Traditionalism is everywhere the rule in the earlier stages of a given social development. It is so powerful that it requires forces of exceptional strength to break through it even appreciably, and only when that has happened are certain kinds of social development, like that of rational bourgeois capitalism, possible.¹³

Weber assumes that the historically newer or more recent irruptions of charisma will somehow be increasingly institutionalized along legal-rational lines, because this is a historical trend! Social life proceeds from the more traditional to the more legal-rational via irruptions of charisma which break up the old traditionalism and institutionalize along legal-rational lines. Such is Weber's view of the long-term course of history. This may be the case in fact, but it must be pointed out that Weber's sociological scheme does not explain this at all; it merely assumes it. Here is a point which bears further investigation, particularly in terms of the relation of charisma (irrational in the extreme and strongly antieconomic due to its extraordinary character) and the requirements of economic life as the latter develops, with its routine, mundane orientations.

As Weber viewed the matter, bureaucracy and rationality necessarily went hand in hand. The modern world was destined to see the irresistible growth of both bureaucracy and rationality, inasmuch as they are interrelated. No wonder, then, that Weber, in contrast to Marx, was led to stress the similarity of capitalism and socialism, viewing them as continuities precisely through the process of bureaucratization. As Parsons comments, "Not only would socialistic organization leave the central fact of bureaucracy untouched, it would greatly accentuate its importance."¹⁴

If, however, bureaucracy is not necessarily connected with rationality but is the re-

¹³ *Ibid.*, p. 549.

¹⁴ *Ibid.*, p. 509.

¹² Parsons, *op. cit.*, p. 543.

sult rather of the institutionalization of charisma, then one gets a very different picture from that given by Weber. Far from stressing the continuities between capitalism and "socialism" (in concrete terms, Soviet Russia, although one must remain fully aware that Soviet Russia does not represent socialism in the doctrinaire sense of this utopian term, but, then, nothing could) we would rather stress the radical discontinuity. A social system having a charismatic bureaucracy as its ruling class is not merely a more rationalized form of capitalism but a quite different social order, in radical opposition to capitalism. We are dealing here not with two genera of the same species but rather with two distinct families. What might be the relation between capitalism and democratic bureaucracy is another question.

The difficulties that attend Weber's approach to bureaucracy show up most clearly when one attempts to apply his ideas to the problem of the nature of the Soviet Union. Using Weber's approach, one will be forced into one of two paths of explanation. Either the Soviet Union contains all sorts of deviations from the ideal-type construct of bureaucracy, and, if this is so, one must ask: Why? (On the basis of Weber's belief in the increasing rationality of the world, these deviations are unexpected and have to be accounted for.) Or, on the other hand, one will entertain the expectation that a legal-rational order must eventually come to exist in the Soviet Union, although there is no substantial evidence for this. Such a view perpetuates illusory hopes about the future peaceful development of democratic social relations in the Soviet Union and is too optimistic about the possibility that the Communist social structure may basically transform itself, despite the weight of historical evidence to the contrary.

There is much to indicate, however, that bureaucracy and rationality do not necessarily go hand in hand, although under certain conditions, they may. It may be, too, that (contrary to Weber) the long-run historical

direction is a change from a legal-rational basis to a charismatic one, so that a bureaucracy can free itself from any outside control and "come into its own" as a new ruling class. To do so, it must raise claims to power and refuse to be merely a technical instrument in someone else's hands. Such a course, envisaged by the technocrats, Burnham's "managerial revolution," and Veblen's "Soviet of Engineers" (although misunderstood by all, since they stressed technological indispensability and overlooked the ideological elements necessary for such a transformation) remains, however, only in the realm of speculative possibility. At this point we do not know what the long-run historical direction, if any, may be. At any rate, the bureaucratic ruling class of Soviet Russia did not come to power by any "managerial revolution" based on their technical indispensability. The bureaucracy was elaborated after the Revolution in which the Bolsheviks seized power and represents the institutionalization of that power, step by step, in a series of changes by no means complete as yet.

The usual bureaucracies of the West, whether labor, industrial, or state, do not have their foundations in such charismatic movements as bolshevism represents but are rather related to legal-rational orders. What modern man is quite justifiably afraid of is charismatic bureaucracy, which is not, and in the nature of the case cannot be, responsible to any legal-rational order of which he may feel a constituent part. Legal-rational bureaucracy may be annoying, stupid, slow-moving, too complex, ad infinitum, but it does not inspire fear, because it cannot make total claims of a moral sort. It is necessarily a limited structure, circumscribed in power, and subject, on principle, to control from other power centers in a pluralistic universe.

Charismatic bureaucracies are irresponsible and totalitarian. Hierarchical order, embracing moral claims, divisions into the orthodox and the heterodox, and the sense of mission and salvation of mankind mark them all. While charismatic bureaucracy

undergoes change and adaptation and may, indeed, prove to be highly flexible (if we take the Catholic church as an example), certain fundamental dogmas can never be given up. The bureaucratic organization is fundamentally committed to these dogmas and exists in relation to them. Charismatic bureaucracies cannot simply be turned to any purpose, as Weber conceived of bureaucracy. Charismatic bureaucracy cannot, by definition, be value neutral.

SUMMARY

The conception of bureaucracy developed by Weber contained elements requiring an ambivalent attitude, as indeed Weber himself was ambivalent when facing this phenomenon. Inasmuch as Weber was the founder of the serious study of bureaucracy, the areas of confusion that he left behind (very likely because his work remained incomplete) have been multiplied by the uncritical adoption of his ideas on this subject. Clarity requires the separation of the two types of bureaucracy and the careful study of the historical examples of each form.

For Weber, a bureaucratic structure, being legally-rationally based, is always a means and is controlled from some point outside itself. A bureaucracy is purely a

technical instrument in someone else's hands. But the phenomenon of charismatic bureaucracy, embodied in such historical cases as pharaonic Egypt, Incan Peru, and the Soviet Union, shows instead that a bureaucracy may be a ruling class itself and not a means in someone else's hands. Under such circumstances, the bureaucracy functions essentially for itself. Bureaucracy is an end and not subject to any outside control, for there is none. The routinization of charisma in a bureaucratic direction has resulted in a totalitarian order, and, conversely, a totalitarian order is an example of the routinization of charisma in a bureaucratic direction.

In our view, the prospects for a responsible (i.e., democratically controlled) bureaucracy depend upon the realization that a theoretical distinction must be made between charismatic and legal-rational bureaucracies. The former are basically totalitarian structures, ends unto themselves, and therefore incapable of responsibility to anything outside themselves (except under duress). Only if a bureaucracy is rooted in a legal-rational order can it remain entirely a technical instrument and, hence, a responsible bureaucracy.

JUILLIARD SCHOOL OF MUSIC

CONTINGENCIES OF PROFESSIONAL DIFFERENTIATION

HARVEY L. SMITH

ABSTRACT

Social differentiation within modern professions creates differential sensitivity to developments within a profession and outside it. The various areas of professional activity and levels of professional organization respond to different contingencies or respond differently to the "same" contingencies. This process is highlighted during professional change. The professional association represents selected interests within a profession; the resistances of environing professions permit only certain areas of professional expansion; institutional inertia within a profession provides brakes on certain forms of change; professional fictions select out certain stereotypes at the expense of others; the public image similarly rewards certain activities and ignores others; variations in skills and their distribution make for variations in professional independence. Professional technology can be understood only as rooted in these problem areas of professional integration and adaptation.

The modern professions are complex social institutions which select people of varied skills, often from several social strata, and organize them into different levels of operation and diverse interest groups. Each level and group may be sensitive to contingencies not shared by the profession as a whole. Thus different parts of the profession may "metabolize" at different rates, and any single action may have many, diverse, and often conflicting effects within the professional institution. In addition, our complex modern professions have multiple relations, directly and, through their professional associations, with other occupational groups and with the public. Here, too, there are important problems of differential sensitivity. Such problems are highlighted during periods of professional change. We will review these problems as they involve the following: professional associations, environing professions, resistances within the profession, professional fictions, the role of the public, and professional skills.

THE PROFESSIONAL ASSOCIATION

Within the profession there may be wide divergence of aims between rank-and-file members and their professional associations. The personnel of such associations may be selected from among the *avant-garde*, the future-oriented members of the profession. They may demand changes in the profes-

sion's status and remain insensitive to those of their fellow members whose security systems are rooted in the status quo. Or, like other human institutions, the professional associations, regardless of the need that they arose to fulfil, often develop a desire to perpetuate their own existence and behave in ways not necessarily those of the profession they represent.

Another problem may arise when the professional association tends to represent one interest group within the profession rather than another—the teaching group, for example, rather than research or service. In such a case important components of a profession may remain effectively disenfranchised at the level of professional politics.

RESISTANCE OF ENVIRONING PROFESSIONS

The rank-and-file members of a profession may be caught in the cross-fire when the directives of their associations are too greatly at variance with the aims of other professions in a shared working situation.

It is, for example, an excellent thing for a society of pathologists to insist that hospital pathologists are clinicians and should have ready access to the bedsides of living patients. Current medical understanding of the role of pathologist makes the complete acceptance of such definition highly unlikely, certainly in the immediate future. Many medical colleagues continue to define the pathologist's role as the scientist of cadav-

ers and body tissues. The bedside aspirations of some pathologists have been met with the jeering appellation "clinical mortician." The result renders the position of the pathologist somewhat unstable: he is prodded to become something which his working situation will not yet permit.

There are other forms of this problem. The physician-anesthesiologist often encounters a medical situation defined for a nurse-anesthetist.¹ The radiologist's fee arrangements with hospitals are often at variance with those desired by his professional associations.²

Among nurses, changes advocated by their professional associations frequently meet resistance from physicians whose work would be involved in the changes advocated. Many physicians are resisting the technical professionalization of nursing in favor of a more specifically "womanly" role which they see as proper to nursing.

The professionalizing occupation may also be involved in competition with other occupations, and this may lead to organizational strain within its working institutions. For example, one may look upon a hospital as a set of specified functions which are parceled out among the several occupations and professions concerned with the care of patients. In such a situation occupational mobility upward is achieved by taking over certain functions formerly limited to the next higher personnel category or by sloughing off certain unwanted or low-prestige functions to the next lower category, or both means may be used. The group above may resent the intrusion. The group below may resent taking over the unwanted or degraded tasks. Considerable tension may result.³ Nursing, for example, seems to be caught, in this way, between

resentful physicians and resentful aides.

Decisions made within a profession may be abrogated by forces or values mustered by the other professions. For example, in several states psychological associations have attempted to secure the licensing of psychologists in the practice of psychotherapy. In some cases this has met head-on the resistance of organized medicine and has been summarily rejected. Similarly, nursing considers itself a competent guide to its own professional fate. Yet it meets opposition from medicine, which often considers nursing a dependent profession, with skills ancillary to those of medicine. Until physicians consult and communicate freely with nurses, they can never share the nurses' desires for change and will continue to oppose them. Yet, if the physicians *are* consulted, they may impose criteria objectionable to nursing.

These dilemmas have no ready solution, but they clearly pose the problem: Who sets (or should set) standards for a profession? In other words, from where does the mandate for professional functions and changes come? Answers to such questions may often involve important attitudes or sanctions of related professions.⁴

RESISTANCES WITHIN THE PROFESSION

Members of a profession who are spearheading moves to change its status must always take account of the possible resistances within their own profession. For many members the present set of operations may be exactly what they want or, if not quite that, exactly what they were trained for. They may feel comfortable having achieved success or recognition on the basis of their present skills. Drastic changes in the system may invite new competition or require new courses of education and training. For people who have settled into a

¹ Dan C. Lortie, "Doctors without Patients: The Anesthesiologist, a New Medical Specialty" (unpublished Master's thesis, Department of Sociology, University of Chicago, 1950).

² The radiologist, who is often a salaried hospital employee, is now urged by his professional associations to go onto a fee-for-service basis with direct charges to "his" patients.

³ Harvey L. Smith, "Two Lines of Authority Are One Too Many," *The Modern Hospital* (Modern Hospital Publishing Co., 1955), pp. 59-62.

⁴ Harvey L. Smith, "The Value Context of Psychology," *American Psychologist*, IX (September, 1954), 532-35.

career line, change is always difficult, often impossible. The overthrowing of a system of status and prestige arrangements may involve deep-seated threats to personality. Thus the brakes on change within a profession should never be overlooked. Any organized profession includes resistance to change within its very principles of organization: it possesses institutional inertia.

In every established occupation shared values and expectations are developed around the bonds of collegueship, the meaning of the occupation, and its place in the world. This occupational culture may be threatened or torn apart during periods of drastic professional change. Conflicting alternative conceptions may arise. Then, when a common front is most required of the professions, when it faces opposition or misunderstanding, it may be least able to muster unity. This may be quite disrupting to the personal security of the members of the group involved.

Modern professions are so complex that the many different levels or specialty groups within them may lose the sense of having a common culture and of sharing a common fate. Some groups within a profession may feel closer to members of some other profession than they do to certain members of their own. Nurses, for instance, in terms of career hazards and educational requirements, are sometimes closer to the world of the administrator than to many colleagues in nursing. Many psychiatrists are finding closer intellectual ties among social scientists than among their fellow physicians in other specialties.

Such differentiation within a profession poses difficult problems of intraprofessional understanding and communication. When enough members of a profession develop skills and sympathies outside the usual competence of their colleagues, disruptive centripetal forces are set in motion. These often lead to a considerable proliferation of sections and subsections within the professional society, each representing a special interest. In some professional groups the coming-together of such special interests has led to their splitting off from the parent

society and the establishment of explicitly separate societies. In this way, the American Psychiatric Association has fathered more than a dozen new societies from among its own profession's special-interest groups.⁵

The problem is posed: How to contain the professional divergences within one association and avoid the "hiving-off" process? Or, is it better to have competition among societies? This is an area for research; at present there are no clear answers. Yet every profession needs to be sensitive to the problem and its effects upon professional unity.

The structural complexity of the modern profession entails other important problems; communications and relations within the profession may become more difficult to maintain; and the occupation may find it harder to establish its place clearly in the rank order of occupations and professions.

Such problems take many forms. The varied approaches of psychiatry—to the outside observer incompatible—appear to be preventing the blanket acceptance of psychiatry within medicine. Acceptance is sporadic and piecemeal. The public's image of the profession, often a source of much support, may become confused and inadequate.⁶ Colleague relations may no longer be all-inclusive within the profession but, instead, increasingly selective. Old-fashioned neuropsychiatrists and new-fashioned psychoanalysts—both psychiatrists—may have almost no shared universe of discourse. Authority systems and formal structures may become more firmly established and grow farther apart from the informal working patterns of members of the profession. In fact, the crucial working relationships which provide the occupational "hard core" with much of its meaning (i.e., the physician-patient relationship) may become harder to define and maintain. Patients in

⁵ Harvey L. Smith, "New Roles for Psychiatry: A Sociological Study" (unpublished manuscript).

⁶ The public often cannot clearly distinguish among registered nurses, aides, and practical nurses in hospitals. Similar confusion has been shown to exist as to the differences among psychologists, psychiatrists, and psychoanalysts.

teaching hospitals, confronted with students, house staff, and senior staff, are unable frequently to discover the physician primarily responsible for their care. Key professional identifications may become vague and elusive. Thus psychologists are engaged in defining for the public the legitimate activities of members of their profession, and nursing is specifying distinctions among the various levels and functions of nurses. It is at such times that committees on functions, standards, and qualifications are established or looked to for guidance. Much current nursing activity concerns this problem area.

The impetus for change requires special consideration. We must ask: Who is aware of the need for change; how have they become aware; of what needs are they aware; and of which needs are they unaware or insensitive? The same questions arise concerning the particular forms of change advocated. Involved here, again, is the professional mandate. Is the sponsorship for change outside the profession (i.e., the client public) or another profession or is it from within the profession; if so, at what level: rank and file, professional associations, training institutions? Different sponsoring groups may respond to different contingencies.

PROFESSIONAL FICTIONS

Every profession operates in terms of a basic set of fictions about itself.⁷ These provide the profession with a comforting self-image, some stereotype to help meet and adapt to the varied and often drastic contingencies of everyday operation. The Air Force pilot gazing up into the blue—Pasteur, Osler, Florence Nightingale—these are symbols of professional fictions. In themselves they may be true images, but they are allowed to engulf or obscure contrary truth and images, and so become professional fictions. These fictions help to de-

fine immediate functions; they help the professional person to relate to others in terms of some mutuality of expectancy; they are often primary foci of recruitment. Therefore, they perform a useful and necessary function.

As with all fictions operating in human behavior, however, unless there is occasional testing of reality, the individual, or the profession, is in danger. If the profession has come sincerely to believe in a set of fictions too grossly at variance with reality, the final contemplation of that reality may indeed be a shock.⁸

What appears to happen in many professions is that some comfortable, perhaps quite "real," self-image is selected out from among the many available and then generalized to the profession at large. In this latter context it may be a gross distortion, or untrue. Recruitment posters do not mention that most Air Force personnel remain on the ground doing administrative, clerical, or maintenance work. It is the pilot who is the rallying symbol. Nursing and medical imagery highlight the drama of healing and ignore the many drudgeries of medical care. We must ask: Of whom in the profession are such fictions true, and to whose professional experience are they false? Since such fictions tend to concentrate the rewards of prestige in some areas of professional activity and to ignore others (who attributes high drama to the dermatologist?), they figure importantly in the profession's adaptation. Recruitment of staff, as well as the satisfactions of incumbent personnel, may be intimately involved.⁹

⁸ For studies dealing with the gap between training and work and its effects see Miriam Wagenschein, "Reality Shock" (unpublished Master's thesis, Department of Sociology, University of Chicago, 1950); also Alvin Katz, "The Development and Design of Operational Devices: A Case Study of Sociological Research in Process" (unpublished Master's thesis, University of North Carolina, 1955).

⁹ Clearly, certain medical specialties are defensive about their present place in medicine. Recruitment problems are often entailed (e.g., among pathologists).

⁷ For a detailed, pioneer analysis of organization fictions by Robert Dubin see "Organization Fictions" in Dubin (ed.), *Human Relations in Administration* (New York: Prentice-Hall, Inc., 1951), pp. 341-45.

THE ROLE OF THE PUBLIC

Gradations and differentiation within a profession tend to be ignored by the public. The public image of a profession, although it may be quite different from the profession's self-image, has the same effect of polarizing attention upon certain selected areas or levels of professional activity at the expense of others.

Indeed, the public is usually unaware of the range of variations. Professional organization itself tends to impose an image of massive unity upon the public, which may have an adverse effect upon both profession and public. Psychotherapeutic treatment, for example, is often hindered by the public's failure to distinguish among psychology, neurology, psychoanalysis, or psychiatry as the proper treating agent.

Public recognition is one of the rewards of professional activity. The arbitrary selectivity of the public's stereotype leaves large areas of professional activity without such reward. Furthermore, groups or individuals within professions who seek to utilize public attention as a means of improving their position and prestige, as psychiatry has been doing in recent years, may be regarded with suspicion by members of their larger profession.

THE ROLE OF SKILL

Some specific skill, or cluster of skills, becomes the "hard core" which provides each profession with a distinctive focus. Such skills, however, are not always uniformly distributed throughout the profession. Although medicine usually insists that the "hard core" of the profession is the physician-patient relationship, many physicians have no such relationship. This is often the case in radiology, anesthesiology, pathology, and even surgery. Also, as we have noted, the complexity of hospitals may seriously dilute or interfere with such relationships. Something of the effects of this uneven distribution of intrinsic professional skills is revealed in references to the specialist who is "the doctor's doctor" rather than the patient's.

It becomes important, therefore, in professional analysis, to know who possesses these nuclear skills and who does not, since the latter representatives may feel, or even be, limited in the full exercise of their professional competence. Physicians without patients (anesthesiologists, radiologists, pathologists) have thus developed working relationships with hospitals rather than with colleagues or patients. As a result, they suffer from a form of status unease; they are salaried personnel rather than professionals who receive a fee. All are currently striving to change their hospital working relationships to those of clinicians paid by patients.

In rapidly changing professions quite different skill clusters may be operative. These reflect changes in the professional education system as well as changed functions for the professional. Nurses, for example, are currently being educated with a conception of nursing which older generations of nurses may find unacceptable.

Such skill differentials contribute to the centripetal forces within professions. Professional analysis needs to ask: Which skills are available and to whom? Which are selected or rejected and by whom and why?

SUMMARY

This paper has developed the differential sensitivity by personnel at various levels of a profession to the contingencies of their profession and its milieu. It indicates some problems involving institutional integration and adaptation which require continued research. The emphasis on technical functions, so important in professional studies, must yield to the study of the meanings of such skills and their place in the social organization of the profession. Technical solutions achieved by professions with regard to functions, standards, and qualifications must satisfy, at the same time, the demands of professional integration and adaptation.

SOCIAL-PSYCHOLOGICAL FACTORS IN PUBLIC ORIENTATION TOWARD AN OUT-GROUP

WILLIAM J. MACKINNON AND RICHARD CENTERS

ABSTRACT

An opinion survey, conducted in a large urban community, is designed to reveal some of the sources of opinions concerning the Soviet Union and the nature of the public's orientation toward that country. Newspapers are found to be the predominant source of opinion. Opinion is less divided with regard to the status of free speech in Russia than on the issue of equality of income; on the latter question social class appears to be an important determinant of opinion. Stronger opposition to information about the Soviet society arises from people with authoritarian personalities.

This report concerns some social-psychological variables relevant to the American orientation toward Soviet Russia. Certain questions about this orientation, and especially about its determinants, issue from prior studies. The following questions are particularly prominent: How salient are the mass media as sources of belief about Russia? How does the public compare the American and Russian societies as to equality of personal income or control of public discussion? What groups in our society differ in response to these questions? What forces resist information to facilitate comparisons? Finally, what groups favor atomic force to resist, under certain circumstances, Russian propagandizing of Americans?

Some indication of the answers to these questions was obtained in a public opinion survey in Los Angeles County.¹

RELATIVE INFLUENCE OF THE MASS MEDIA

To what extent does the newspaper provide the background for American opinion

¹ The interviewing of 460 people took place about the beginning of the year 1954. Each interviewer had quotas in each of four census tracts of different and (approximately) known social rank. These ranged from the poorest to the wealthiest areas. The interviewers were instructed to select equal numbers of men and women, half of whom in each case were to be over forty years of age. For other aspects of this survey see William J. Mackinnon and Richard Centers, "Authoritarianism and Urban Stratification," *American Journal of Sociology*, LXI (1956), 610-20.

about Russia? Content analysis in the past afforded some conception of the probable influence of the American newspaper on attitudes toward the Soviet Union.² The respondents themselves provided an answer here when asked how they had learned most of what they knew about the Soviet system or way of life. Reports of source of information produced the data in Table 1. The differences to be considered initially in this table are those based upon the source of belief mentioned first.

That newspapers dominate among sources of information is clear; more than one-third of the people mention the newspaper before any other source. It outnumbers radio and television combined by four to one. The printed word in general—newspapers, books, magazines, and the somewhat vague "reading" response—characterizes about two-thirds of these replies. Some people mention ordinary conversation. Very few, however, have learned much about Russia in school, and even fewer, as we might expect, have observed conditions in Russia directly or through the eyes of others who have been in the U.S.S.R.

The first-mentioned sources as presented in Table 1 also afford some comparisons be-

² On the basis of a thematic analysis it has been suggested that two probable effects of Soviet news as reported in the *New York Times* were reinforcement of American values in an ethnocentric fashion and strengthening of adverse attitudes toward Russia (see Martin Kriesberg, "Soviet News in the *New York Times*," *Public Opinion Quarterly*, X [1946], 540-64).

tween people with some college education and those without. Newspapers, for instance, are less prominent sources of beliefs about Russia among the college-educated group; books and magazines and "reading," by contrast, are more common among them. ("Reading," therefore, probably refers to books and magazines more than to newspapers.)

Similar differences were obtained from breakdowns on other sociological and psychological variables. Forty-two per cent of manual laborers, as contrasted with 33 per cent of non-manual workers, mentioned

mentioned sources were considered. Furthermore, newspapers consistently dominated as sources of belief.

They also dominate in Table 1 when the first three mentioned sources of belief are studied. The usual differences also appear between educational levels. In these cases the number of responses (769) rather than the number of respondents (460) provides the basis of the percentages. Thus the printed word in general and the newspaper in particular seem to be central in molding the background of American opinion toward Russia.

TABLE 1
PERCENTAGE FIRST MENTIONED AND FIRST THREE MENTIONED SOURCES OF INFORMATION ABOUT RUSSIA AS REPORTED BY COLLEGE AND NON-COLLEGE SUBGROUPS AND BY THE TOTAL SAMPLE

SOURCE	FIRST MENTIONED SOURCE AT LEAST			FIRST THREE MENTIONED SOURCES AT LEAST		
	No College (N=259)	Some College (N=201)	Total (N=460)	No College (N=408)	Some College (N=361)	Total (N=769)
Newspapers.....	40.9	30.8	36.5	36.8	24.4	31.0
Radio.....	8.5	4.0	6.5	13.0	9.7	11.4
Television.....	2.7	2.0	2.4	5.9	3.6	4.8
Books.....	3.1	14.9	8.3	5.2	13.0	8.8
Magazines.....	5.8	9.4	7.4	6.9	10.5	8.6
Reading.....	8.1	17.4	12.1	7.4	13.5	10.3
Conversation.....	7.0	3.9	5.6	8.3	5.8	7.2
School.....	3.9	7.5	5.4	3.4	5.3	4.3
Presence in Russia.....	0.8	1.5	1.1	0.7	1.1	0.9
Contact with American Russians.....	1.9	1.5	1.7	1.5	1.9	1.7
Miscellaneous*.....	17.4	7.0	12.9	11.0	11.1	11.0

* Includes "Don't know," "Other," "Indeterminable," and "No answer," the last under "First Mentioned Source" only.

newspapers first. Lower- and working-class people mentioned newspapers more often than did middle- and upper-class members. A similar difference appeared between authoritarians and equalitarians; the first mention newspapers more frequently.³ The direction of the differences in all these cases remained the same when the first three

Newspapers, of course, are intended to provide news far more than are radio and television, which rely largely upon entertainment to attract their audience. Newspaper editors, however, may seize upon qualities of the Soviet system alien to Americans to dramatize their presentation of news, to entertain. Lacking the auditory and visual concreteness of radio and television, they may perforce select and over-emphasize the dramatic content that will sell newspapers.

AMBIGUITY AND SOCIAL DYNAMICS

At least one area of the internal Soviet system remains ambiguous.⁴ Many years

⁴ A stimulus field or a communicative content that permits various structurizations is defined as

³ Social class was determined by requesting subjects to choose a name for their social class as described in Richard Centers, *The Psychology of Social Classes* (Princeton, N.J.: Princeton University Press, 1949). Authoritarian personality was defined in terms of a score above the sample median on the scale contained in Fillmore H. Sanford and H. J. Older, *A Short Authoritarian-Equalitarian Scale* ("Progress Report," No. 6, Ser. A [Philadelphia: Institute for Research in Human Relations, 1950]).

ago it was shown that Americans had widely divergent beliefs about the extent to which economic equality existed inside Russia.⁵ Even experts cannot fully characterize the status of Soviet socioeconomic stratification.⁶

Table 2 shows no clear consensus of views about the comparative range of income in the United States and the U.S.S.R. Subjects were asked: "Compared with the differences in earnings or income between the richest and poorest people in the United States, are the differences in Russia smaller, greater, or about the same?" Roughly, half the people say that the range of inequality in income is greater in Russia than in the United States, and one-fourth say that the range of inequality is smaller in Russia. In other words, for every two people who think economic inequality of this

sort is greater in Russia, there is one who thinks it is smaller. Apparently, the area is quite ambiguous.

Part of the divergence of opinion may arise from different interpretations of the question. Some people may have misunderstood the wording and answered in a direction contradictory to their belief. Some may have been thinking less of extremes in wealth than of the proportion of people who had attained a moderate degree of affluence. Yet, as mentioned before, an earlier opinion survey reported divergences in beliefs on a similar question (n. 5).

Less ambiguity surrounds the issue of comparative censorship (Table 2). An item directed toward the comparative control of

TABLE 2

PERCENTAGE OF OPINIONS ON COMPARATIVE ECONOMIC EQUALITY AND COMPARATIVE POLITICAL LIBERTY. UNITED STATES AND U.S.S.R.

OPINION	ITEM	
	Range of Income	Degree of Censorship
Greater in Russia.....	47	74
About the same.....	12	9
Less or smaller in Russia....	23	7
Don't know.....	18	10

ambiguous by A. S. Luchins, "The Stimulus Field in Social Psychology," *Psychological Review*, LVII (1950), 27-30, and Franklin Fearing, "Toward a Psychological Theory of Human Communication," *Journal of Personality*, XXII (1953), 71-88.

⁵ About 20 per cent of a national sample once thought that people in Russia were all paid about the same amount of money, and somewhat more than twice this percentage thought there were wide differences in pay. (Forty-four per cent said they did not know.) The dichotomous item in this earlier study asked for an absolute judgment, whereas the present trichotomous question requires a comparative judgment with the United States as standard (see Warren B. Walsh, "What the American People Think of Russia," *Public Opinion Quarterly*, VIII [1944], 513-22).

⁶ There are probably some incomes as large as 100,000 rubles in Russia. Stratification increased in the Soviet Union during the 1940's, though in 1950 vertical mobility was still perhaps greater in the Soviet Union than in the United States. Certain documents needed for precise determination have been lacking, e.g., information on the occupation of parents of students in Russian institutions of higher learning. These estimates and opinions come from Alex Inkeles, "Social Stratification and Mobility in the Soviet Union: 1940-1950," *American Sociological Review*, XV (1950), 465-79. A letter to the editor reports that "there is recent evidence against the increasing rigidity of the Soviet class structure" (see Barrington Moore, Jr., "Terror and Progress, U.S.S.R.," *American Journal of Sociology*, LXII [1956], 329).

public speech in the United States and Russia revealed that a large majority of people attribute greater censorship to the Soviets.⁷

Frequencies in attributing greater censorship to Russia differ significantly among various sociological and psychological groups. Percentages are higher among individuals having a college education, a non-manual position, middle- or upper-class membership, and an egalitarian personality (in each comparison $P < .05$).

On the other hand, only social class and occupational stratum yielded differences of importance on the comparative income-range item (Table 3). The combined middle and upper classes and the non-manual

⁷ For a discussion of personal values in relation to beliefs about political and economic realities inside Russia see M. B. Smith, "Personal Values as Determinants of a Political Attitude," *Journal of Psychology*, XXVIII (1949), 477-86.

stratum more frequently attribute a greater range of income to Russia than do their counterparts ($P < .025$).⁸

Since education does not produce clear-cut differences in this belief ($P > .10$), it would appear that objective information is lacking. Social class interest thus may shape perception protectively. This disparity between the American ideal of equality and the American social stratification may fade in the light of an assumed greater inequality elsewhere.⁹

As the battle for minds continues in the ideological world war, knowledge of the comparative equality and liberty of different nations will assume great importance.

That is, people with college education, in the non-manual stratum, in the middle or upper class, and with equalitarian personalities pose less resistance to such knowledge (in each comparison $P < .01$). The last-mentioned variable seems the most potent in this regard, producing as it does the largest percentage differences found between any group and its counterpart (Table 4). Authoritarianism appears to be a major force opposing internationalism. In the present case the force takes the form of resisting knowledge of a national out-group.¹⁰

Differences between authoritarians and equalitarians are even more pronounced on

TABLE 3
PERCENTAGE OF OPINIONS ON THE COMPARATIVE RANGE OF PEOPLE'S
INCOME IN THE UNITED STATES AND U.S.S.R. BY OCCUPATIONAL
STRATUM AND SOCIAL CLASS

OPINION	OCCUPATIONAL STRATUM*		SOCIAL CLASS*	
	Manual (N = 146)	Non- manual (N = 221)	Working and Lower (N = 146)	Middle and Upper (N = 228)
Greater in Russia.....	51	63	51	63
Smaller or same in Russia.....	49	37	49	37

* Exact $P < .025$.

Much research is necessary. The forces furthering or restraining this objectification therefore warrant attention. On this point interviewers asked: "By explaining the way government and industry are run in Russia, do writers help Americans or hurt them?"

The same groups which more often attribute less censorship to the United States than to Russia more often feel that Americans are benefited by information about the political and economic system of Russia.

⁸ The calculation of this limit of the exact probability was facilitated by a table in Ronald A. Fisher and Frank Yates, *Statistical Tables for Biological, Agricultural, and Medical Research* (New York: Hafner, 1949), p. 47. The counterpart of the combined middle and upper classes is simply the combined working and lower classes.

⁹ For examples of protective psychodynamics of social class members see Richard Centers, "Social Class, Occupation, and Imputed Belief," *American Journal of Sociology*, LVIII (1953), 543-55.

TABLE 4

OPINIONS OF AUTHORITARIANS, EQUALITARIANS,
AND BOTH AS TO BENEFICIAL OR HARMFUL
EFFECTS ON AMERICANS OF WRITERS WHO
DESCRIBE RUSSIAN GOVERNMENT OR INDUS-
TRY*

Opinion	Equali- tarians (N = 228)	Authori- tarians (N = 230)	Both (N = 458)
Help Americans...	78	63	70
Hurt Americans...	7	18	13
Don't know.....	15	19	17

* The chi square based on the 2×2 table, with "Don't know" and "Both" omitted yielded $P < .001$.

an item concerning propaganda. The subjects were asked whether atomic bombs should be used if necessary to stop propaganda coming from Russian ships at sea

¹⁰ For related types of intolerance and their determinants see Samuel A. Stouffer, *Communism, Conformity, and Civil Liberties* (Garden City, N.Y.: Doubleday, 1955).

and designed to win Americans to the Soviet side.¹¹

Only 11 per cent of equalitarians approved atomic bombing under these conditions as contrasted with 30 per cent of authoritarians. These were the greatest percentage differences found between the groups compared. Congruent with our previous results, destructive impulses were less common among people with college education, in non-manual positions, and belonging to the middle or upper class (in each case, $P < .001$). These are the groups in which authoritarianism less often appears.¹²

TABLE 5

AUTHORITARIANISM AND APPROVAL OF
ATOMIC BOMBING

Authoritarianism Scores (Class Intervals)*	N	Per Cent Approving
5 and above.....	13	61.5
4.5-4.9.....	35	54.3
4.0-4.4.....	63	25.4
3.5-3.9.....	81	22.2
3.0-3.4.....	117	18.8
2.5-2.9.....	62	14.5
2.0-2.4.....	47	2.1
1.5-1.9.....	25	8.0
1.0-1.4.....	17	0.0

* The range of possible scores is from 1 to 6.

Table 5 brings out the sharp differences here. Approval of atomic bombing rises rather decidedly with increasing degrees of authoritarianism. In the lowest three categories of authoritarianism less than 10 per cent of individuals would approve bombing

under the stated circumstances; in the two highest, more than 50 per cent would approve. The range extends from 0 per cent in the lowest class interval to 62 per cent for the highest. That the United States might repay militant propagandizing in kind or that the Soviet Union might repay atomic bombing in kind appear to be sober second thoughts occurring less often to authoritarians than to equalitarians.

SUMMARY

An opinion survey in Los Angeles County cast light on the sources and nature of public orientation toward Soviet Russia. According to the people's own report, the newspapers predominate among the sources of their beliefs about Russia. The predominance is most distinct among people in less advantaged strata and among related social-psychological groups.

Comparative economic equality in the United States and the U.S.S.R. seems to be a far more ambiguous area than comparative political liberty. Only occupational stratum and social class significantly relate to opinions on the economic comparison. Education and equalitarianism are additional significant determinants on the political judgment. The non-manual stratum and middle or upper classes more often attribute a greater range of personal income to Russia. College education and equalitarian personality are two additional correlates of the view that speech censorship is greater in Russia.

These four sociological and social-psychological factors also relate significantly to the belief that written descriptions of the Russian system benefit Americans. They also appear to counter tendencies that would meet intensified Russian propaganda with atomic reprisal.

UNIVERSITY OF ARIZONA
AND
UNIVERSITY OF CALIFORNIA

¹¹ This item is reminiscent of Maurice L. Farber, "The Armageddon Complex: Dynamics of Opinion," *Public Opinion Quarterly*, XV (1951), 217-24. In this article a willingness to have an immediate showdown with Russia, even at the risk of war, was related positively to an unsatisfactory personal future outlook, the male sex, and Catholicism. For a summary of an early and thorough study of American opinion on the atomic bomb see Leonard S. Cottrell, Jr., and Sylvia Eberhart, *American Opinion on World Affairs in the Atomic Age* (Princeton, N.J.: Princeton University Press, 1948).

¹² MacKinnon and Centers, *op. cit.*, pp. 610-20.

NEWS AND NOTES

University of Arizona.—Frederick A. Conrad has retired as head of the Department of Sociology after thirty-five years of service. He will continue to teach on a half-time basis.

Ralph R. Ireland joined the staff in September as professor and head of the department.

Joseph F. Pobrislo, who was recently awarded the M.A. degree in sociology, has been appointed instructor in sociology at Long Beach State College.

A program in correctional administration has been newly established under the direction of Clyde B. Vedder, who joined the department in 1956.

Bates College.—Peter P. Jonitis has been appointed associate professor.

University of Chicago.—As the *Journal* went to press we learned with sorrow of the death, at thirty-six, of R. Richard Wohl, associate professor. His loss will be deeply felt by students and colleagues alike and his contributions as friend and intellectual comrade missed sorely by all.

Awards for study in statistics by persons whose primary field is not statistics but one of the physical, biological, or social sciences to which statistics can be applied are offered by the Department of Statistics of the University of Chicago. The awards range from \$3,600 to \$5,000 on the basis of an eleven-month residence. The closing date for application for the academic year 1958-59 is February 15, 1958. Further information may be obtained from the Department of Statistics, Eckhart Hall, University of Chicago, Chicago 37, Illinois.

Resources for the Future, Inc., has made a grant to the Population Research and Training Center for a study of relationships between urban centers and their tributary regions, with special reference to patterns of natural resource utilization. The study will be directed by Otis Dudley Duncan, with the assistance of W. Richard Scott.

The Autumn, 1957, issue of the *School Review* contains a symposium—"Social Science and Education"—with contributions from eight

Chicago social scientists. Single copies are available at \$2.00 each on order to the University of Chicago Press, Chicago 37, Illinois.

Everett C. Hughes will participate at the meeting of the International Sociological Association, which, in co-operation with UNESCO, is holding a research round table in Moscow, January 4-12, 1958.

De Paul University.—Helen Znaniecki Lapata, who recently joined the sociology faculty, has completed a methodological study of "The Recommendation Process" for Dr. Nelson Foote, of General Electric. She is now engaged in expanding the scope of her earlier study, "The Social Role of the Young Suburban Housewife." The expanded study is being done under a grant from the *Chicago Tribune*.

James E. McKeown has been appointed Smith-Mundt Visiting Professor of Methods in Sociology at the University of San Simon in Cochabamba, Bolivia, for the term starting March, 1958.

Florida State University.—The Department of Sociology and the School of Social Welfare have initiated a new joint doctoral program, with a concentration in either marriage and the family or criminology and corrections. A number of assistantships are available for well-qualified students.

Francis R. Allen and Charles Grigg have received a grant from the University Research Council for a study of the attitudes of college students toward the Florida Nuclear Energy Program.

T. Stanton Dietrich has been granted a leave of absence for the academic year 1957-58 to serve as consultant to the Industrial Services Division of the Florida Development Commission. He will supervise the economic and industrial survey being conducted in the rural areas of Florida by the First Research Corporation of Miami and A. B. Little, Inc., of Cambridge, Massachusetts.

Charles Grigg has been appointed consultant to the state-wide (Florida) Ninth-Grade Testing Program.

Russell Middleton, formerly of Kansas State

College, has joined the staff as assistant professor of sociology.

William F. Ogburn will be visiting professor of sociology for the spring session. He will also return in 1958-59.

Gladys Putney served as temporary lecturer in sociology for the winter semester.

Snell Putney, previously of Drake University, has joined the Department of Sociology as assistant professor.

Vernon B. Fox has been working with F.S.U.'s radio and television services in the preparation of "Prison Document," a series of seven half-hour radio programs on today's prisons.

Recently, the British Health Society made Earl Lomon Koos a Fellow in the Society.

Jack C. Lamb, Stanley B. Williams, Johnny R. Price, and Lacy Hall, candidates for the doctorate, have been appointed part-time instructors in social welfare.

During 1957-58 David L. Levine will be research consultant to the National Children's Cardiac Hospital on a follow-up study of rheumatic-fever patients.

James D. Turner has been appointed associate professor of social welfare and will work in the area of criminology and corrections. For the past eighteen months Mr. Turner has been associated with the American Bar Association in its studies in the administration of criminal justice.

Foundations' Fund for Research in Psychiatry.—The Fund wishes to announce that February 1, 1958, is the next deadline for submission of applications for research fellowships in psychiatry, psychology, sociology, and other sciences relevant to mental health.

Interested persons and departments are invited to write for details to Foundations' Fund for Research in Psychiatry, 251 Edwards Street, New Haven 11, Connecticut.

University of Hawaii.—The Sociology Club is soliciting subscriptions to Volume XXI of *Social Process in Hawaii*. The volume is concerned with race relations and acculturation and may be obtained for \$1.00 through The Sociology Club, University of Hawaii, Honolulu 14, Hawaii.

Institutum Divi Thomae.—In September, 1957, Clark Tibbitts, assistant director of the Special Staff on Aging, United States Department of Health, Education, and Welfare, was given the honorary degree of Doctor of Science.

International Association of Applied Psychology.—The Association has announced the provisional program of its thirteenth congress to be held in Rome, April 9-14, 1958. For information write to Segretaria del XIII Congresso Internazionale di Psicologia Applicata, Istituto Nazionale di Psicologia, C.N.R., Piazzale delle Scienze 7, Rome, Italy.

University of Kansas.—Four new members have been added to the teaching staff. Harold A. Gould comes as instructor from Washington University, St. Louis, where he has been completing his doctoral studies. John Giele, a candidate for the Ph.D. degree at Nebraska University, joined the staff as instructor. Part of his time will be devoted to conducting sociology courses in the Extension Division. Ray P. Cuzort, assistant professor, comes from the Population Research Center, University of Chicago. Louise E. Sweet is visiting assistant professor of anthropology. Her manuscript, "Tell Toqaan: A Syrian Village," is nearing completion and will be published by the University of Michigan Near Eastern Studies Department. This ethnographic study is based on field work begun in 1953.

Mrs. Henrietta Cox, instructor during the past year, has resigned to return to Washington University, St. Louis, to continue her doctoral studies.

Marston M. McCluggage is on a year's sabbatical to the University of Washington, Seattle, where he is visiting professor in human relations in the Business School.

Esther Twente has resigned as chairman of the Department of Social Work in order to resume full-time teaching of sociology and social work.

Charles K. Warriner participated in the Social Science Research Council's summer institute on organizational theory and research held at Carnegie Institute of Technology.

Carroll D. Clark, chairman, conducted a human relations workshop at the University of North Dakota in the summer of 1957. He also served as staff member of the University of Kansas Executive Development Program.

During the summer of 1957 John and Jeanne Gullahorn served as consultants for the Foreign Student Leadership Project, a program of the National Student Association which brings student leaders from Asia, Africa, Latin America, and the Middle East to American colleges. They will continue these services throughout the aca-

demic year as well as continue their research in cross-cultural education.

University of Kentucky.—Newly appointed staff members are Bruce J. Biddle, assistant professor of sociology, and John Flint, instructor, who has just received the Ph.D. degree at the University of Wisconsin.

E. Grant Youmans has replaced John R. Christiansen as the representative of the Farm Population and Rural Life Branch, AMS, USDA, stationed at the University. Youmans also holds an appointment as lecturer in sociology.

James W. Gladden has been promoted to professor of sociology, and A. Lee Coleman to professor of sociology and rural sociology.

University of Michigan.—David Goldberg has been appointed director of a newly established Population Research Program which will be conducted by the department in co-operation with the University of Michigan Institute of Public Administration. This continuing research program will provide graduate students in population and human ecology with additional research-training opportunities.

Morris Janowitz recently received a grant from the Social Science Research Council to further his study of the professional soldier. University of Michigan grants have been awarded to G. E. Swanson to study problems of religion and ethics in primitive societies; to Tad Blalock to analyze factors related to discrimination in southern counties; to Harry P. Sharp to study methods of increasing interview-completion rates in survey research; and to Ronald Lippitt to study communications within the family between parents and teen-aged children.

Ronald Freedman will lecture this year at the Municipal University of Amsterdam on a Fulbright grant and will study postwar birth-rate trends in western Europe with the aid of a grant from the Guggenheim Foundation.

Horace Miner has been awarded a Rockefeller Foundation grant and will study a project of planned social change among the Hausa in northern Nigeria next year.

Harold Wilensky and Ted Newcomb are returning to the department after spending a year at the Center for Advanced Study in the Behavioral Sciences at Stanford.

Guy Swanson and Ronald Lippitt will spend 1957-58 at the Center.

New additions to the staff this year include

Seymour Yellin, of Northwestern University; Frances Scott, of the University of California at Los Angeles; and Henry Meyer, of New York University, who will have a joint appointment with the School of Social Work. Visiting professors this year include Everett Wilson, of Antioch College, and Milton Yinger (spring semester), of Oberlin College.

The University's Doctoral Program in Social Science and Social Welfare has admitted its first students. Morris Janowitz is chairman of the program.

Robert C. Angell has returned from Johns Hopkins University, where he served as the Deiches lecturer in sociology. He has recently been appointed director of the University's Literary College Honors Council.

The Inter-University Training Institute in Social Gerontology of the University announces thirty-five faculty fellowships for a summer training seminar in social gerontology to be held at the University of Connecticut, August 4-29, 1958.

A distinguished group of world authorities and research scientists and teachers in anthropology, psychology, psychiatry, sociology, economics, and other social sciences will lecture and lead faculty discussion groups covering the basic scientific perspectives of the psychological and social aspects of aging. Demonstrations, field trips, and special lectures will deal with selected aspects of programs and institutions in the field of aging.

The core faculty of John E. Anderson, Ewald W. Busse, Bernice L. Neugarten, and Clark Tibbitts will be joined by other prominent scholars and authorities, including James E. Birren, Ernest W. Burgess, Philip Hauser, Alvin I. Goldfarb, Raymond G. Kuhlen, A. I. Lansing, John W. McConnell, Nathan W. Shock, Leo Simmons, and Seymour L. Wolfbein.

Fellowships are open to college faculty members trained in one of the social sciences and actively interested in developing programs in social gerontology at their institutions. Fellows will receive an award of \$500 in addition to travel costs and living accommodations on the University campus.

For applications and further information address: Dr. Wilma Donahue, Director, Institute for Social Gerontology, 1510 Rackham Building, Ann Arbor, Michigan.

Michigan State University.—John Useem has been appointed head of the Department of Sociology and Anthropology and will also serve

as director of the Social Research Service and the Area Research Center. He replaces Charles P. Loomis, who has resigned as head but continues as research professor of sociology and anthropology. Professor Useem has been granted a sabbatical leave January 1, 1958, to December 31, 1958, to do research in India under the sponsorship of the Hazen Foundation. During his absence Charles R. Hoffer will be acting head of the department.

Edward O. Moe was in Costa Rica from June to November, 1957, at the Inter-American Institute of Agricultural Sciences, Turrialba, Costa Rica. While there, Dr. Moe worked on the evaluation of program planning and analysis, in accordance with the agreement of the Institute and Michigan State University to develop sociology and anthropology in Latin America.

Julian Samora has been appointed assistant professor. He spent the past year in research dealing with the influence of culture on health practices among the Spanish-speaking people in Denver, Colorado. The study was financed by the Carnegie Corporation.

New appointments to the staff include: Donald Olmsted, who will do teaching and research in social psychology; William Faunce, who will teach in the department and devote part time to research in the Labor and Industrial Relations Center; Sheldon Lowry, who has a joint appointment in the College of Science and Arts and in the Agricultural Extension Service; and William D'Antonio, who has a joint appointment in sociology and anthropology and in the Social Science Department of the Basic College.

W. B. Brookover, professor of social science and sociology, has assumed a new position as director of the Bureau of Educational Research in the College of Education.

During the first six months of 1958, Charles Cumberland will be in Mexico and Richard Adams will be in Chile. They will participate in the study of overseas programs of American universities that is being carried on by the University's Institute of Research on Overseas Programs. The Institute was established at the University in January, 1957, on a grant from the Carnegie Corporation.

Richard Adams was in Puerto Rico in November where he participated in a conference on the subject of "Plantation Systems in the New World." The conference was sponsored by the Pan American Union and the Puerto Rican government.

Duane Gibson was in Pittsburgh, Pennsylvania, in August where he served as a co-director

of a two-week workshop on intercultural education. The workshop was sponsored by the University of Pittsburgh and the Pittsburgh Council on Intercultural Education.

Moreau Maxwell has been appointed associate professor of sociology and anthropology and curator of the Museum, effective October 16, 1957.

Carl C. Taylor will serve as distinguished visiting professor of sociology and anthropology. He will be in residence during the Winter Quarter and will offer a graduate seminar.

University of Missouri.—Dr. S. Chandrasekhar, director of the Indian Institute of Population Studies, Madras, has joined the staff and will teach courses in population and cultures of Asia.

C. T. Pihlblad spent the winter semester at the Sociological Institute, University of Oslo, continuing his researches on population movements in Norway.

Robert W. Habenstein has been promoted to associate professor. He spent last summer as secretary to the Commission on Mortuary Education, whose report, *The Future of Funeral Service Education*, was published in October.

Robert F. G. Spier taught last summer in the General Extension Division at Portland, Oregon. He is continuing his research on Chinese tool acculturation.

Edwin A. Christ, formerly research associate, has been appointed assistant professor at the University of South Dakota. He received the Doctor's degree in August.

Robert J. Dwyer, who received the Ph.D. degree in June, has been appointed assistant professor at Idaho State College, Pocatello.

Marvin Riley has returned to his regular position as associate professor at South Dakota State College after a year of graduate study at Missouri.

Maurice M. Mook, of Pennsylvania State University, was a member of the staff during the past summer session.

Carl H. Chapman has been promoted to the rank of associate professor. After spending two years directing archeological field research, he has returned to his regular position in residence.

Richard Brown, University of Nebraska, and Robert Sweet, University of Chicago, have been appointed instructors in sociology.

John T. Mitchell, instructor in sociology for the last three years, has been appointed to a post in the classification department of the Missouri State Penitentiary.

National Science Foundation.—The Foundation wishes to announce that proposals in the Social Science Research Program for work to begin in the summer or fall of 1958 will be accepted until February 1, 1958. Proposals received after February 1, 1958, will be reviewed following the fall closing date of October 1, 1958, and grants approved in time for work to begin early in 1959. Inquiries should be addressed to National Science Foundation, Washington 25, D.C., The Social Science Research Program supports basic research in anthropology, archeology, demography, human ecology, social geography, economics, social psychology, and the history and philosophy of science.

University of Oregon.—Harry Alpert has been appointed dean of the graduate school and professor of sociology effective July, 1958.

Robert Dubin has returned from a year's stay at the Center for Advanced Study in the Behavioral Sciences.

Benton Johnson is serving as a replacement for Herb Bisno, who is on leave of absence with the Council on Social Work Education as project director for the Undergraduate Curriculum Study.

James L. Price, a doctoral candidate at Columbia University, has been appointed instructor.

John M. Foskett has been named editor of the *Pacific Sociological Review*, an official publication of the Pacific Sociological Society to be published at the University of Oregon. Theodore B. Johannis, Jr., has been named managing editor. Mr. Johannis has been elected secretary of the National Council on Family Relations and will be in charge of local arrangements when the Council holds its annual meeting in Eugene in August, 1958.

Walter T. Martin has returned from a year's leave of absence spent at the University of California and has been appointed chairman of the department.

University of New Mexico.—An eight-week conference and work session sponsored by the Behavioral Sciences Division, Air Force Office of Scientific Research, was held at the University during the summer of 1957, from mid-June to mid-August. Participants from the field of sociology were: Edgar F. Borgatta, Russell Sage Foundation; Edward L. Rose, University of Colorado; and William A. Westley, McGill University.

The Conference was under the direction of Paul Walter, Jr., professor of sociology, assisted by Ralph D. Norman, professor of psychology at the University.

Preparation for publication of research reports resulting from the Conference is now under way.

University of Pittsburgh.—James D. Thompson, of Cornell University, has been appointed director of the Administrative Science Center at the University and associate professor of sociology. He will give courses in administration and complex organization.

Roger Nett, formerly associated with Washington State College and Oklahoma A. and M., has joined the department as assistant professor; he will handle statistics and research methods.

Herbert A. Aurbach, research specialist with the Commission on Human Relations in Pittsburgh, has been appointed lecturer in intergroup relations.

William P. Lebra, who has completed course work for the Ph.D. degree at Harvard University, has been appointed instructor in anthropology.

Peter B. Hammond, of the Administrative Science Center, will have the rank of assistant professor of anthropology.

Buford H. Junker, also with the Administrative Science Center, will be associated with the department.

The Department of Sociology has received a grant-in-aid from the Educational Television and Radio Center at Ann Arbor, Michigan, for the purpose of conducting studies of the educational television audience in the Pittsburgh metropolitan area. Howard Rowland will direct the project with the assistance of Roger Nett.

C. K. Yang has completed much of his extensive research on the Chinese society. A forthcoming volume, "The Chinese Family in the Communist Revolution," will be followed by additional publications on the "Chinese Village" and "Religion in Chinese Society."

Portland State College.—Now in its third year of operation, Portland State College wishes to announce that its staff in sociology and anthropology has been expanded to four full-time members.

John James, professor of sociology, will be on part-time leave while serving as research director for the Oregon Study of Rehabilitation

of Mental Patients, a three-year project supported by a grant from the Federal Office of Vocational Rehabilitation and the State of Oregon.

Charles S. Brant joined the faculty in September, 1957, as assistant professor of anthropology. Dr. Brant's most recent experience has been in medical sociology. During 1956-57 he was research associate at Albert Einstein College of Medicine, New York City, under a grant of the Russell Sage Foundation.

Following a year's leave, Charles Frantz is returning to the college in the Winter Quarter, 1958, as assistant professor of sociology and anthropology. During the past year he has conducted field research among the Sons of Freedom sect of the Dukhobors, in British Columbia, in connection with his work for the Ph.D. degree at the University of Chicago.

University of Rochester.—The name of the Department of Sociology at the University of Rochester has been changed to "Department of Sociology and Anthropology."

Walter H. Sangree has been appointed assistant professor of social anthropology.

San Jose State College.—Three new members have been added to the Department of Sociology and Social Work: Mervyn L. Cadwallader, assistant professor, a specialist in sociological theory and social change; Heinz J. Graalfs, assistant professor, who taught in 1956-57 at the University of Oregon and whose major fields are human ecology and population; and Harold M. Hodges, assistant professor, who taught in 1955-57 at Wisconsin State College and whose interests lie primarily in the fields of culture and personality and social stratification.

Edwin G. Flittie has accepted a position on the sociology staff of the University of Wyoming.

Society for the Scientific Study of Religion.—The spring meeting of the Society for the Scientific Study of Religion will be held on Saturday, April 12, 1958, at Columbia University, New York City. Scholars who would like to submit brief papers of an empirical nature for inclusion on the program should send three copies of a three-hundred-word abstract to the chairman of the Planning Committee for the meeting, Dr. Lauris Whitman, 297 Fourth Avenue, New York, New York.

Southern Illinois University.—Three new members have joined the staff: Peter A. Munch, of Oslo, who formerly was chairman of the Department of Sociology at the University of North Dakota; Ronald Vander Wiel, M.S.W., Washington University, who was chief social worker at Anna State Hospital, Anna, Illinois; and Louise Johnson, who received the M.A. degree from Columbia University in 1957.

Eloise Snyder has been promoted to the rank of assistant professor.

The University of Oslo has recently published *A Study of Social Change: Rural-Urban Conflicts in Norway*, by Professor Munch, who is now preparing for publication "A Collection of Songs and Ballads from Tristan da Cunha." Columbia University Press will publish *People of Coaltown* by Professor Herman Lantz, in February, 1958.

The department, which now grants the B.A. and M.A. degrees, is in the process of developing a Ph.D. program.

University of Virginia.—Peter R. Goethals, Ph.D. candidate (anthropology) at Yale University, has been appointed instructor in anthropology. Mr. Goethals returned last summer from two years' field study in the Dutch East Indies, chiefly on the island of Soembawa, and in the Netherlands.

Western College for Women.—As part of its program of intercultural studies, the College has announced plans for a seminar in Africa this summer, the culmination of a year's concentrated study in that area. In preparation for the seminar Wyn Rees, from the University of Natal in Durban, South Africa, is serving as visiting professor and area expert this year. The seminar, which is limited to thirty-two members, will leave New York on July 1 for a five-week tour of Africa. Preference for membership is given to students, but others are also eligible to join. Further information about the seminar may be obtained by writing: The office of the President, Western College for Women, Oxford, Ohio.

T. A. Bisson, last year's area specialist on the Far East, has remained at Western to work with the intercultural studies program and to serve as acting associate professor of intercultural studies.

Dr. M. C. Elmer is John Hay Whitney Visiting Professor of Sociology for the 1957-58 academic year.

Miss Margaret Barrier, assistant professor of sociology, is on a leave of absence this year to continue her graduate study in sociology at Columbia University.

Western Reserve University.—The Ohio Superstitions Project by Newbell N. Puckett, chairman, Department of Sociology, is progressing, with something over ten thousand separate beliefs so far collected. During the past year considerable field work was done among the Old Order Amish, and some interesting experiments were conducted in regard to the collection of material by way of television and radio. During the summer there was some collection of Negro spirituals in Mississippi and of lumber-camp ballads in Canada.

Richard A. Schermerhorn has returned from a sabbatical leave of study at Harvard. During the summer he taught at American University in Washington, D.C. He was elected in August, 1957, president-elect of the Society for the Study of Social Problems.

Marvin B. Sussman is completing a study of the Hough Area—a number of Cleveland communities in transition. Funds were supplied by the Cleveland Foundation. R. Clyde White, professor of social work, School of Applied Social Sciences, is the other principal investigator. Eleanor K. Caplan, research associate, and Virginia K. White, field director, are also assigned to this project. Other projects by Dr. Sussman include a series of studies on the sociopsychological correlates, family aspects, and epidemiology of chronic diseases.

Yale University.—A continuation grant has been awarded by the National Institutes of Health for completion of a research project on "Alcohol and Higher-Order Problem-solving."

This research, which has been in progress for over a year, is being conducted by Omar K. Moore and Charles R. Snyder (sociology), in collaboration with John A. Carpenter and Edith S. Lisansky (psychology), under the auspices of the Interaction Laboratory of the Department of Sociology and the Center of Alcohol Studies.

Jack V. Buerkle has received a grant from the National Institutes of Health to develop and evaluate a device to measure the nature of interaction between husbands and wives as they react to hypothetical marital-role conflict situations. It is hoped that such work will lead eventually to a device for predicting marital stability. Theodore R. Anderson is serving as statistical consultant on this project, and Robin F. Badgley as research assistant.

August B. Hollingshead, who is on sabbatical leave during the academic year 1957–58, has received a Fulbright award to Great Britain. He will conduct research with the Social Research Unit of the British Medical Research Council, which is affiliated with the Psychological Institute of Maudsley Hospital, University of London. The project will be concerned with attitudes toward physical and mental illnesses in sample populations in England, Wales, and Scotland. In the spring he will also lecture at University College of North Staffordshire. His book, co-authored with Frederick C. Redlich, M.D., *Social Class and Mental Illness*, will be published by John Wiley and Sons in March, 1958.

Richard J. Coughlin, who has been working primarily in the Southeast Asia Studies program, has taken a year's leave of absence to join the Asia Foundation in Hong Kong. He will help develop a program of educational and other assistance for the Chinese in Hong Kong and for the overseas Chinese in Southeast Asia.

BOOK REVIEWS

Economy and Society: A Study in the Integration of Economic and Social Theory. By TALCOTT PARSONS and NEIL J. SMELSER. Glencoe, Ill.: Free Press, 1956. Pp. xxi+322. \$6.00.

It is embarrassing to review a work by a distinguished author whose system of thought makes no appeal whatsoever to the reviewer. The senior author of this work is pre-eminently a Man with a System. The system is internally coherent, and it is unquestionably *one* way of organizing thought about social phenomena. Whether it is a useful way, however, and, especially, useful enough to warrant the considerable expenditure of time and energy required to master it, is something else again. The present volume is important because, in a sense, it promises to be a test case of Parsons' system. The economic system is clearly a subsystem of a larger social system, of which Parsons' theory purports to give an account. The test, therefore, is this: Can his system be applied to the sphere of economics in such a way as to yield insights into the operation of the economic system which economics by itself cannot provide? This is the task of the present volume.

It would be impossible, even in an extended review, to give any adequate summary of Parsons' system itself. Briefly, that system divides social life into four parts or aspects, named by the four letters A (for Adaptive), G (for Goal-Gratification), I (for Integration), and L (for Latent-Pattern Maintenance and Tension Management). The A-system is best represented by the economic system; the G-system by the political system (or, as Parsons calls it, the Polity). The I-system and the L-system are less easy to identify but may be roughly equated with religion (sport, morale, ethics, etc.) and with the household or the social individual as a "boundary-maintaining" system. We must beware, however, of identifying Parsons' dramatis personae with anything that looks like flesh and blood; they are aspects of all organizations or all concrete realities, and no concrete reality can be identified with any one of them. Social

action he conceives as something which happens at the boundaries of these systems, each of which feeds outputs of some sort into the others and receives inputs from them. In and around them Parsons has developed an elaborate set of conceptual dichotomies of objects, attributes, and hierarchies; the central idea, however, seems to be the generalized notion of exchange as the prime mover of social life: everybody does something *for* something, whether that something be monetary reward, prestige, or praise or the fear of some sanction, legal or social.

The object of the book, then, is to study the economy as a subsystem of the general social system, especially concerned with the adaptive or ends-means relationships of social life, and to examine what exchanges take place between the economy, conceived as mainly an A-system, and the other three social systems, G, I, and L. There seems to me to be a basic misconception here as to the nature of economics—indeed, as to the nature of theoretical systems. Thus the authors say: "The peculiarity of economic theory, therefore, is *not* the separate class of variables it employs but the *parameters* which distinguish the special case or class of cases we call economic in the use of the general variables of social theory from the other important types of special case" (p. 6). The truth seems to be quite the reverse. It is *precisely* the separate class of variables which economics employs that, for the economist at any rate, distinguishes it from the other sciences. Economics studies prices, quantities of commodities exchanged, produced, consumed; interest rates, taxes, tariffs—its basic abstraction is that of the commodity. It seeks to find reasonably stable relationships among these variables, but it is the variables, not the relationships, which delimit the subject matter of the science. Economics can, of course, be illuminated by contributions from other disciplines. These contributions, however, consist in admitting new and different kinds of *variables* (status, rank, class, power, etc.) and not particularly in employing new kinds of relationships.

This basic difficulty runs all through the book and leads to a cumulative misinterpretation of what economics has to say. Thus wealth is "not an inventory of commodities but an instrumentality for achieving goals" (p. 24). "Raising the interest rate is a signal that productivity must increase at a sufficiently higher rate in order to justify the current level of credit: lowering the interest rate encourages enterprise by symbolically communicating that the rate of productivity increase need not be so high" (p. 75). (This is a massive confusion between "returns" and "productivity.") There is a really astonishing misunderstanding of the Keynesian system: the authors seem to think that "Keynesian" unemployment is a voluntary withdrawal of labor from the market—a supply-of-labor phenomenon rather than a demand-for-labor phenomenon. One could adduce many more instances.

A sociologist may be forgiven a few misunderstandings. The real test of the system, however, is the contributions it makes to economics. The test of labor is the baby, and a very substantial baby is promised. What we are promised is "determinacy" of the functions which comprise the basic relationships of economic theory. These functions, like the consumption function and the investment function in the Keynesian system, are distressingly vague; their exact shape and parameters are not determined by pure economics, but, with a little assistance from Parsons, we can learn their true forms! Thus we learn from the examination of the various inputs from the social systems in the family that the consumption function will have a kink (p. 225). We do not seem to learn much about the investment function except that behavior in the stock market may be due to "deeper drives to deviance in individuals, such as unbridled acquisitiveness, sadism, phantasied wish-fulfilment, etc." The mountain has labored and brought forth a mouse, if that.

I have not been able to disguise my opinion that this is not a good book. It is not, however, a trivial or unimportant work. It struggles with real problems. Occasionally, important insights break out of the cumbrous trappings of the system, for instance, the point that the credit and banking system belong more to the "polity" than to the "economy." The authors are conscientiously trying to work with an apparatus that they believe is an important contribution to the tools of the social scientist. A tool is not

to be condemned because it breaks once. But when that tool produces as little as it does in this volume and, furthermore, seems to be based on a basic misconception of the nature of theoretical systems, one must call the tool itself into question. Even an unsuccessful attempt at an important task, however, is praiseworthy, and, as we come to understand *why* certain attempts at system-building fail where others succeed, we shall lay the foundations for a more successful advance toward the integration of the social sciences.

K. E. BOULDING

University of Michigan

American Families. By PAUL C. GLICK. New York: John Wiley & Sons, Inc., 1957. Pp. xiv+240.

This volume is one of the "Census Monograph Series" sponsored by the Social Science Research Council and the Bureau of the Census. It is no compliment to the author to say that he is the outstanding family statistician in the United States, because he holds a virtual monopoly of the field. Perhaps the present monograph will stimulate sociologists of the family to explore the potentialities of official data in this central area of social research.

Four major areas of inquiry into American families at mid-century are treated here: household and family composition; the life-cycle of the family; marriage and dissolution of marriage; and future household and family formation. The sources of data, in addition to the decennial censuses and particularly that of 1950, are primarily the frequent reports produced through the Current Population Survey since 1944. Despite the inevitably cross-sectional character of such data, the author has exercised considerable ingenuity in converting them into the form of individual and family histories. In the face of the formidable problems presented by the data, it may be uncharitable to suggest that the highly dynamic character of the development of familial institutions in our present era makes them crucial for research, yet relatively intractable by means other than patient, painstaking, longitudinal inquiry.

The work can be characterized as descriptive, in the best sense of the term. It is difficult to believe that valid generalizations about American families can be produced independently of

the kinds of parameters reported here. At the very least, the reader should become highly sensitized to the vast penumbra of meanings concealed within the term "family." Perhaps the sluggish and ungraceful style is a direct reflection of the care with which a person dedicated to operationality approaches any subject of such inherent complexity. Sociologists should be grateful that a person of Glick's competence is active in the field of family sociology.

N. B. RYDER

University of Wisconsin

The Changing Shape of Metropolitan America: Deconcentration since 1920. By AMOS H. HAWLEY. Glencoe, Ill.: Free Press, 1956. Pp. 177. \$4.00.

Utilizing United States metropolitan data (the best in the world), Hawley analyzes thoroughly the changing pattern of population distribution within our expanding urban agglomerations from 1900 to 1950. His units are the Census SMA's (pushed back to 1900, eliminating those not comparable over each decade). In addition, in the first three chapters, he employs an interesting unit of his own—the Extended Metropolitan Area, which includes all counties with centers within 35 miles of the core of the central city—and thus avoids some of the biases of the SMA. He measures distance in terms of 5-mile zones from the center of the central city; the 0–5-mile zone is what is left over when the central city is removed. All zones taken together (excluding the central city) comprise the satellite area.

The most general conclusion, embodied in the title itself, is that the tendency was toward concentration from 1900 to 1920 and toward deconcentration from 1920 to 1950. This result, however, is partly an artifact of the procedure employed. The author tends, especially in stating his conclusions, to treat the whole satellite area as a single unit vis-à-vis the central city. Thus his view on concentration and deconcentration rests on the fact that in 1900–1920 the central cities grew faster than the satellite areas (i.e., all territory surrounding the center out to 35 miles and over), whereas in 1920–50 the opposite was true. But to use a fixed-distance notion of satellite area from 1900 to 1950 is to use a Procrustean bed. To live 20–30 miles from the center of a city in 1900 is

hardly the same thing as to live that far in 1950. Actually, the data presented for successive 5-mile zones show that from 1900 to 1910 the central city (which undoubtedly at that time included much open land into which people could expand) barely grew faster than the 5–10-mile zone (31.5 per cent as against 29.4 per cent). In the next decade the 0–5-mile zone grew faster than the central city (29.3 per cent as against 25.9 per cent). If one agrees that from 1900 to 1910 population was probably dispersing *within* the central city, then it follows that deconcentration has been going on since 1900 and perhaps before.

This interpretation is borne out by the fact that the fastest population growth in the SMA's occurred farther and farther out as time went by: 1900–1910, central city; 1910–20, 0–5-mile zone; 1920–40, 5–10-mile zone; 1940–50, 30–35-mile zone. The areas where the population grew more slowly than in the United States as a whole were beyond 10 miles in 1900–1910, beyond 15 miles in 1910–20, and beyond 25 miles in 1920–50. Thus the middle zones of the metropolitan areas have tended to draw population, relatively speaking, both from the distant periphery and from the center.

As one would expect, the larger the central city, the greater the distance outward in which rapid growth is found. Also, as the author reveals in a significant chapter, the dispersal of population tends to be related to the dispersal of industry. As between regions, the facts show that in 1900 the proportion in the central cities was roughly similar (South, 56.4 per cent; West, 58.5 per cent; and North, 62.5 per cent). From 1900 to 1930 the West led in rate of internal dispersion, to be displaced by the North from 1930 to 1950. As a consequence, the western agglomerations wound up with the smallest, the northern with the next smallest, and the southern with the largest proportion in the central cities.

The study concludes with a summary of seven variables that govern deconcentration within metropolitan areas. By "deconcentration," again, is meant the ratio between central-city growth and that of the *entire* satellite area. A more sensitive index could perhaps have been constructed by taking the various distance zones into account. Also, the data apparently do not permit control of the seven variables, which are overlapping. "Northern location," for instance, is not independent of "size of central city" or "proportion employed in manufactur-

ing" or "central city within 50 miles of other central cities." The list seems to lack a logical structure; yet its discriminatory power, in terms of the author's index, is great.

Whether or not one agrees with all of Hawley's broad conclusions, the fact remains that he has given a valuable presentation and skilful analysis of data on the changing location of people within United States metropolitan areas—an analysis filling an important niche in the growing body of systematic literature on this subject. Facts hitherto unknown or only dimly suspected are brought out with precision. The narrowness of interpretation mentioned above, together with an overly terse style, may make the volume less comprehensible than it might otherwise have been to the large body of non-technical people concerned with urban affairs, but it will not diminish the value of the study for the experts.

KINGSLEY DAVIS

University of California

The Sutherland Papers. Edited by ALBERT COHEN, ALFRED LINDESMITH, and KARL SCHUESSLER. Bloomington: Indiana University Press, 1956. Pp. 330. \$5.00.

Few men have the opportunity and the ability to leave their mark on a field of study as did Edwin H. Sutherland. Criminology in America today shows his influence in all its aspects, and it is largely his writings which have elevated the study of crime from an academic version of the detective story to a serious focus for sociological theory.

This collection of papers ranges from a statement of the theory of differential association, extracted from Sutherland's *Principles of Criminology*, to more fugitive articles and speeches such as "The Control of Crime" and "Juvenile Delinquency and Community Organization." The editors have added helpful introductory notes to the major sections. But these papers achieve unity not by a systematic exposition of a theory or an orderly marshaling of facts but by a pervasive point of view. With careful logic and a deep regard for the empirical data, Sutherland hammers away at one dominant theme—the sociological interpretation of crime and the reactions to crime. This orientation sometimes leads to an unfortunate slighting of psychoanalytical theory and a too quick dismissal

of the possible influence of biological factors in deviant behavior, but it also produces a consistent picture of crime which has been freed from excessive attention to idiosyncratic elements.

Sutherland's view of criminal behavior as learned behavior has, of course, many limitations. It would appear, however, that he was well aware of these limitations; and in one disarming essay in this volume—"Critique of the Theory," prepared for circulation among his associates—he relentlessly attacks his own position. It is an essay which clearly illustrates the scientific attitude in operation. In fact, Sutherland's disinterested search for scientific truth in the field of criminology and his ability to implant an enthusiasm for that search in his students and in those who knew him less directly are made plain throughout this collection of papers. It is one of Sutherland's greater accomplishments that he should have so dominated the field without breeding rigid doctrinairism.

GRESHAM M. SYKES

Princeton University

The Methodology of Preferential Sociometry. ("Sociometry Monographs," No. 37.) By ÅKE BJERSTEDT. Lund, Sweden: Hakan Ohlssons Boktryckeri, 1956.

Sociometric devices are among the most commonly encountered empirical methods used by social scientists. Yet there has not been available an impartial treatment of the history, complexity, and subtlety of the many variations of this seemingly simple methodology. Bjerstedt's elegant work fulfils this need. In fact, the reviewer cannot think of a better source for the advanced student who wishes to gain a good perspective of this area.

Following the Introduction, which is broadly conceived to deal historically and conceptually with sociometric measurement in general, the book is devoted to what the author calls "preferential sociometry" (i.e., that subclass of sociometry that refers specifically to social relations described in terms of preference—attraction, neutrality, rejection in a choice situation). In chapter ii, general principles for collecting sociopreferential data are reviewed and discussed. Problems of analysis are treated in the last chapter (chap. iii). A most impressive and eclectic bibliography rounds out the work.

This is a scholarly, advanced book, written

with a broad perspective in sociology and psychology as well as in the general area of formal scientific methodology. It is undoubtedly the most systematic treatment of the material, with original and useful formulations and terminological innovations.

RENATO TAGIURI

Harvard University

The Democratic and the Authoritarian State.

By FRANZ NEUMANN. Glencoe, Ill.: Free Press, 1956. Pp. x+303. \$6.00.

Eleven essays or addresses, originally published between 1937 and 1954, with a selected chronological bibliography of the writings of the late Franz Neumann, constitute a fitting memorial to a political philosopher of penetrating insight and high ideals.

The author expresses his ideas clearly and forcibly, and such repetition as occurs serves to lend emphasis to his considered judgments. The characteristic blending of idealism and objectivity is illustrated by the sentence "the truth of a doctrine will depend upon the extent to which it embodies concrete liberty and human dignity, upon its ability to provide for the fullest development of all human potentialities." Another might have written "value" instead of "truth," and "desirable" instead of "all."

It is basic that, "as man is rational, all men are rational" and that "man exists quite independently of the political organization in which he lives." As principles "not solely derived from Natural Law" the author upholds "the generality of law, the equality of men, the prohibition of individual legislative decisions, the impossibility of retroactive legislation especially in penal law [would he forbid an Act of Indemnity?] and an independent judiciary."

A power group, we are told, has at its disposal persuasion, material benefits, and violence. These are always present in all forms of government, but "naïve confidence in social reform, education, and constitutionality" can endanger democracy itself. By contrast, "it seems to be impossible to overthrow the holder of political power who is in unlimited possession of terror, economic power, propaganda, and education."

The final address, delivered at the Free University of Berlin in 1954 and entitled "Anxiety and Politics," explains how totalitarianism

arises and how, if it cannot be overthrown from within, it may be avoided. "Persecutory anxiety can lead to ego-surrender in the mass through identification with a leader." It can be developed by a "conspiracy theory of history" or "if a group is threatened with loss of status, without understanding the process which leads to its degradation." It can be institutionalized "by terror, propaganda and, for the followers of the leader, crime committed in common." As for the prophylactic, "Only through our own responsible educational and political activity can the words of idealism become history." The key word is "responsible."

H. F. ANGUS

University of British Columbia

The Torment of Secrecy: The Background and Consequences of American Security Policies.

By EDWARD A. SHILS. Glencoe, Ill.: Free Press, 1956. Pp. 238. \$3.50.

The year 1954 was the high tide of national concern with the complex questions of "loyalty" and "security." Professor Shils made the correct decision when he revised the polemical aim with which he began this book of essays and proceeded in a calmer mood to assess the significance of our national obsession with loyalty and security. The result is a volume of deep penetration and enduring worth for every citizen and every scientist, behavioral or not.

No doubt the most arresting idea in the book is the interpretation put forward by Shils of the changing connection of American intellectuals with politics. His view is that for a hundred years intellectuals identified themselves with "the genteel tradition" which "stood in conflict with the rough-handed, florid-faced politics of the big city machines, with the predominantly immigrant working classes of the big cities on which they were based and the dour, commercially minded farmers of the Middle West" (p. 122). However, the cultural pattern of gentility was "waning," and the result was to alienate the intellectual class from the political life of society.

With the New Deal, men of advanced education moved into the federal government on an unprecedentedly grand scale. "The unpleasant shock to the legislator of the loss of his power to the bureaucracy was not rendered easier to bear by the fact that it was being surrendered

to the intellectuals" (p. 125). The hubbub over loyalty and security is to be interpreted in part as a counteroffensive of the "politician" against the "intellectual"; and one result is a partial check upon the full incorporation of scholars and scientists into American civilization.

The counteroffensive was favored by a swarm of environing and predispositional factors. The most interesting problem—and the one to which Shils devotes himself—is why "loyalty" and "security" became key symbols, slogans, and program guides of the assault. From the viewpoint of research the most rewarding way to use this book is to scan the chapters for pertinent definitions and hypotheses. Here are a few:

"Privacy is the voluntary withholding of information" (p. 22). "Secrecy is the compulsory withholding of knowledge, reinforced by the prospect of sanctions for disclosure" (p. 26). "The growth of literacy and the extension of the franchise with the accompanying politicization of the populace has deprived the upper classes of their monopoly of conspiratorial hallucinations" (p. 30). "The exfoliation and intertwinement of the various patterns of belief that the world is dominated by unseen circles of conspirators, operating behind our backs, is one of the characteristic features of modern society" (p. 31). "The demand for extreme solidarity is the product of fear of betrayal" (p. 34). "The passion for publicity is the passion for a homogeneous society, a passion which emerges from the conception of politics as the relation between friend and foe" (p. 35). "The combination of secrecy which protects and publicity which destroys gratifies the ambivalence which at bottom is characteristic of all extremist orientations" (p. 35). "No other large-scale society possesses the internal self-perceptiveness of American society" (p. 40). "Populistic radicalism almost always harbors, alongside of its fear of secrets, a belief in their salvational character" (p. 46). "The specifically British disequilibrium, which is restricted in range, is the preponderance of privacy and traditional governmental secrecy over publicity. The specifically American disequilibrium is the preponderance of publicity and its attendant stress on salvational secrecy over privacy" (p. 57). "Xenophobia is the price paid for rapid and deeply penetrating assimilation" (p. 82).

The chapter on "The Strain of Politics" has an excellent statement of the impact of our political institutions upon the personal insecur-

ity of legislators. In the closing chapters a valuable attempt is made to formulate the political theory of a pluralistic society, with special reference to the role of scientists and scientific knowledge. The text is embellished throughout with shrewd comparisons drawn from the author's intimate understanding of British and American society.

This is the type of writing too rare among scholars who pursue the newer branches of specialization. It brings structural, psychological, and historical knowledge to bear on a recurring question of fundamental policy.

HAROLD D. LASSWELL

Yale Law School
Yale University

Civil Liberties in the United States: A Guide to Current Problems and Experience. By ROBERT E. CUSHMAN. Ithaca, N.Y.: Cornell University Press, 1956. Pp. xiii+248.

The author, a distinguished political scientist and student of problems of civil liberty for thirty-five years, was asked by the directors and officers of the Fund for the Republic "to prepare for their use, and perhaps for somewhat wider circulation, a summary which would present a bird's eye view of the entire field of civil liberties since the close of World War II" (p. vi). Working within a predominantly, but not exclusively, legal framework, Professor Cushman has obviously set himself the task of exposing this audience to the basic information on the entire range of conventionally defined problems of civil liberty in contemporary United States. Inside this limited framework, he has produced a very competent work.

Explicitly eschewing any philosophical framework except, as he says, a general bias in favor of adequate protection of civil liberty, he presents a series of rather narrowly defined, specific, concrete problems under headings ordinarily utilized by defenders of civil liberty. All are stated in a clear, straightforward manner, a few of the most essential variables isolated precisely, and unresolved areas clearly identified. Technical material is skilfully translated or sufficiently explained so that oversimplification is at a minimum. Given the breadth of coverage, persons familiar with a particular facet of current problems will probably not discover any new material. There are surprises, however: for example, we are told, without benefit of comparative data, that in 1953, 185 of 221 Re-

publicans applied for membership in the House Committee on Un-American Activities (p. 199). At the same time one is impressed with the workman-like comprehensiveness with which most of the topics are handled, and interested readers will appreciate the very useful discussion and valuable suggestions for reading. Lucidly written, the exposition suffers somewhat from the use of an outline form and a marked tendency to terseness. Assuming that the author felt that a lengthy document would not be read, a fuller discussion still would have been advisable, since its omission makes the present work difficult reading for the general reader.

A major disappointment is the author's failure to clarify the definition of civil liberties. Certainly the traditional conception of civil liberties as protection of the individual from governmental tyranny and restraint no longer satisfactorily embraces the subject in a world society where it is common in rhetoric, at least, to look to governmental apparatus to perform positive duties in providing the conditions for freedom, security, and equality. The prominent place which racial discrimination now plays in our current conceptions provides a real opportunity to investigate the antinomy, which Simmel, among others, has suggested exists between freedom and equality. Cushman's sensitivity to non-legal material—his use of the limited number of relevant social science studies, his discussion of pressure groups, voluntary associations, commonly held prejudices and attitudes within the context of specific problems—indicates that he could have added much to our understanding of the change in the conception of civil liberties and what its meaning will be both for daily activity and for broader philosophical notions about man's freedom. It is hoped that this work is but preparation for a more general treatment. In the meantime, we can be thankful that the raw material has been placed before us in such a palatable manner.

SAUL H. MENDLOVITZ

Rutgers University

Race and Culture Contacts in the Modern World. By E. FRANKLIN FRAZIER. New York: Alfred A. Knopf, 1957. Pp. ix+338. Trade ed., \$6.00; text ed., \$4.50.

The Negro in the United States. By E. FRANKLIN FRAZIER. Rev. ed. New York: Macmillan Co., 1957. Pp. xxxiii+769. \$6.40.

Frazier's professional career and publications reflect in many respects a significant trend among American students of race relations. He has progressed from an analysis (in 1932) of one institution within one racial group in one city (the Negro family in Chicago) to a book which has a global and historical orientation.

In *Race and Culture Contacts in the Modern World* Frazier regards the "expansion of Europe" as the most important development underlying race relations. In this expansion he sees "the economic factor" as the most crucial. His basic framework will be familiar to most American sociologists: relations between Europeans and other peoples are analyzed on four major levels—ecological, economic, political, and social—which provide the four divisions of the book.

Another basic feature is a threefold typology of "frontiers of race and culture contacts": (1) European settlements with racial frontiers; (2) tropical dependencies; and (3) the older civilizations of Asia. The classification involves primarily three phenomena: (1) the stage of development of the "native peoples"; (2) the climate; and (3) the relative size of the European "settlement." We are led to expect that most topics treated (e.g., the color bar, colonial administration, caste systems) will include at least a brief summary of developments in the various areas of the world identified with these three types of frontiers. Thus, while the main frame of reference is sociological, there is coverage of a large number of geographic areas within the time perspective of the last several centuries.

The task which Frazier sets for himself turns out to be far too vast for a manuscript of approximately 100,000 words. It was impossible to deal with all the areas according to the outline; on many topics certain areas are ignored while others receive only brief mention. One soon notices gaps in the materials and begins to wonder about problems of proper balance. References to race relations in the United States, which are most frequent of all, deal primarily with Negro-white relations in the southern states. Tropical Africa comes in for second most frequent treatment, followed by South Africa and South America (primarily Brazil). Frazier seems particularly inclined to dwell on those situations in which Europeans displaced or subordinated non-literate peoples and utilized forced labor to develop plantations. One finds little information about actual patterns of

relations within the multiracial commercial centers of the Far East or about the role of Asians, especially Chinese and Indians, in Southeast Asia, Australia, Oceania, Africa, or the New World. Race relations in Europe itself are almost completely ignored. For ethnic relations in Russia the most extensive quotations are from a publication of Stalin's in 1913.

In certain technical respects this book gives evidence of being hastily prepared. For example, different sets of statistics are given for several areas, apparently depending upon the vintage of the sources followed in writing a particular section. The book contains no preface; nor is there a bibliography, although there is an index of names, mainly of authors cited in the numerous footnotes.

At present our conceptual framework for the sociological analysis of race relations in world perspective is still quite inadequately developed. Frazier's book certainly makes an important contribution along these lines. It is hoped that he will carry us farther along this route in another book.

The Negro in the United States has for several years been an excellent text for sociology courses on the Negro. In view of the many significant developments in white-Negro relations since 1949, it is regrettable that, apart from some rewriting and alteration of viewpoint in the final chapter, the present revised edition is little more than a reprinting of the first. A comparison reveals that in the first 684 pages (all except the concluding chapter) only about half-a-dozen sections have been rewritten, involving about a dozen pages. None of the maps, only two of the tables, and three of the diagrams have been revised to include data from the 1950 Census. In most respects the 1949 edition was much more up-to-date for the year in which it was published than is the present edition. In spite of these limitations, this book continues to stand out as fundamental reading for students interested in race relations in the United States and as a superior text for courses focused specifically on Negroes in this country.

CLARENCE E. GLICK

University of Hawaii

tory of Italian immigrants in this country. After a brief account of the often ignored period before 1890 and of the later, more voluminous immigration, consideration is given to varied facets of Italian-American life. Topics discussed include the role of the immigrants in agricultural as well as industrial America; their contributions to art, literature, and the sciences; the formation of ethnic communities; the part played by the Catholic church and voluntary associations; the problems of assimilation and loyalty as related to American stereotypes of the Italian; and the question of distinctive Italian-American personality traits. An excellent annotated bibliography, which refers to many little-known but valuable sources, is included.

However, the contents of the book do not match its bibliography or the promise inherent in the range of topics. A few areas, e.g., the crucial social role of the *padroni*, are well handled and informative. But too often the discussion lapses into a mere cataloguing of names. Thus the first half of the chapter on social mobility gives abbreviated sketches of successful Italian immigrants; the latter half does nothing but list almost every prominent person of Italian descent in New Haven (a locale peculiarly singled out for detailed attention elsewhere in the book also). Most sociologically oriented readers will surely recognize the painstaking labors of the author, but they will find such listings of little value.

The book is also marred by an apologetic and defensive tone unsuited to a work of objective scholarship. There is a glossing-over of what are considered unfavorable aspects of Italian-American life and a reiteration of what Italians have positively contributed to American society. Possibly this book's main sociological interest lies in the fact that it presents a good example of the kind of arguments developed by the intellectual spokesmen of minority groups, who attempt to cope with the stereotypes and reactions such groups must overcome in the process of assimilation.

E. L. QUARANTELLI

*Harpur College
State University of New York*

The Italian in America. By LAWRENCE FRANK PISANI. New York: Exposition Press, 1957. Pp. 293. \$3.50.

Descriptive in intent rather than analytic, this book is presented as a social study and his-

Guerrilla Communism in Malaya: Its Social and Political Meaning. By LUCIAN W. PYE. Princeton: Princeton University Press, 1956. Pp. xvii+369. \$6.00.

On August 31, 1957, the Federation of Malaya achieved its independence and became the tenth member of the British Commonwealth. Four days later, Sir Abdual Rahman, in his first address to the parliament, offered amnesty to Communist terrorists (mostly Malayan Chinese) holding out in the jungle. According to the amnesty terms, the terrorists who surrendered to the government forces and proved their intention to abandon communism would be guaranteed freedom from prosecution for all offenses committed before national independence was achieved. The Communist rebels, whose total number is presently estimated at 1,800, are the subject of Professor Pye's book.

The volume consists of two distinct parts. The first, making up one-third of the book, contains a general description of such outstanding characteristics of National Liberation communism as the emphasis on military action, the existence of "liberated areas," and the skilful utilization of "popular-front" strategies. It also offers a sketch of the salient phases in the development of the Malayan Communist party from a small conspiratorial body with little political power to a rigidly organized and tightly controlled force representing the main roadblock to the internal consolidation of the new state.

The second part is concerned with "the human dimension" of Malayan communism. The author tries to answer a whole array of questions related to the psychology of communism. He examines in detail the types of people that are easy Communist recruits, their class position, their family background, their educational level, and their world outlook. He describes the work of party agents in molding the recruits into a militant Communist force by imbuing them with the spirit of discipline and self-criticism which depends on continuous exhortation for more and harder work. Particularly interesting is his analysis of the deeply imbedded Chinese traditionalism as a force thwarting a speedy Communist theoretical education and ideological indoctrination.

The book's jacket informs us that "the author has used the methods of social science in interpreting his material," and Pye acknowledges his "intellectual debt" to Nathan Leites for his stimulating ideas in "social science methodology." However, he does not give a precise definition of his methodological orientation with the exception of the forthright admission that his tables were not worth publication because "the sample was too small, and

to include them might appear as a presumptuous claim to scientific accuracy" (p. 123). Most of his generalizations are impressionistic statements based on firsthand, planned observations made by himself and responsible British officials. Unfortunately, a part of his analysis shows an obvious racist slant. To him the Malayan Chinese are "the people for whom understanding is basically an intuitive and not an analytical process" and who have "little ability to think, in terms of abstractions and generalizations" (p. 125). Their thinking, he infers, suffers from "logical inconsistencies." As L. Lévy-Bruhl would say, their mind is prelogical. One wonders what "social science methodology" the author used to produce this pearl.

ALEXANDER VUCINICH

San Jose State College

The People of Puerto Rico: A Study in Social Anthropology. Edited by JULIAN H. STEWARD. Urbana: University of Illinois Press, 1956. Pp. ix+540. \$10.00.

In several significant respects this is a first-rate book. Its perspective is broad and imaginative. Much of the ethnography is excellent. The comparability of the materials from the various subculture studies is a refreshing reminder of how much is to be gained when serious attention is paid to conceptual organization and research design. The typography and physical qualities of the book do honor to the publishers and the sponsors. Finally, as a product of the College of Social Sciences and the Social Science Research Center of the University of Puerto Rico, it is a handsome testimonial to and reward for their serious interest in a continuing self-inventory and in their expressed belief in the value of basic research.

The professed purpose of the study is to understand and account for the differences and similarities among a number of subcultures in Puerto Rico. These include a "tobacco and mixed crops municipality" (Manners); a "traditional coffee municipality" (Wolf); a "government-owned sugar plantation" (Seda); and a "rural sugar plantation proletariat" (Mintz). The terms in which these are classified indicate the variables whose effects are considered primary in the initial differentiation of subcultures. These are the uses to which land is put, the form of landownership, and the organization of production. Once the subcultures are

thus distinguished, a host of other factors are examined. These include crop processing, credit, cash crop, mechanization, type of management, labor, consumers' goods, sociocultural segmentation, ritual kinship bonds, political organizations, and supernaturalism and religion.

The argument of the book is that the subcultures became even further differentiated in the modern period, when they became unequally influenced by such factors as the introduction of cash crops, extension of credit, access to consumers' goods, access to markets, and improved technology. It is also claimed that these factors have "profoundly affected other folk cultures throughout the world."

Certain key difficulties of the study may be cited. First, one is never sure which variables are being held responsible for the changes. There is very uneven treatment of these matters in the four communities studied. Second, even when the same *nominal* variables are considered, the operational referents of the terms vary from one author to the next. Third, the amount of evidence brought to bear on any one hypothesis by the various authors is most uneven, and this is even more marked as between hypotheses. Fourth, though many of the arguments are plausible, there is no attempt to examine alternative hypotheses, and there is no way of knowing, therefore, how many other equally plausible arguments might have been adduced by students approaching the matters from a different point of view. It is hard to discover any places in which the authors made it possible to reject their notions. Fifth, the fact that the several authors found it useful to apply the same scheme of analysis in each of the areas does not, as Seda claims, constitute independent verification of one another's findings. Sixth, there is virtually no use whatsoever made of the study of the upper class by Scheele. One is forced to wonder why those materials were ever included in the volume.

It may seem curious for a sociologist to tax a group of anthropologists with an excess of concern for generalization. But the fact is that, while much of the ethnography is excellent and much of the theoretical speculation is by itself interesting and challenging, the quality of the interplay between ethnographic fact and social theory is simply not up to the other facets of the book.

In sum, here is a book which provides the reader with a series of excellent portraits of various local communities in Puerto Rico and

one in which the development of Puerto Rico in the modern period is examined with a good feeling for the complexity of local cultural patterns and for the interaction of the local areas with the increasingly important impulses and influences of the larger social world. As such, it is a most commendable achievement and one which deserves close study by any student of social change.

MELVIN M. TUMIN

Princeton University

Satellite Mentality: Political Attitudes and Propaganda Susceptibilities of Non-Communists in Hungary, Poland, and Czechoslovakia. By SIEGFRIED KRACAUER and PAUL L. BERKMAN. New York: F. A. Praeger, 1956. Pp. 194. \$4.00.

This study is based on interviews with 318 refugees from Hungary, Poland, and Czechoslovakia, conducted in Austria and West Germany from July, 1951, to March, 1952. The sample is grossly skewed: most of the respondents are urban, single males under thirty-one. Peasants comprise only 2 per cent of the Polish sample, although they are roughly 60 per cent of the population in Poland; 9 per cent (!) of the Czechoslovak sample are Slovak Catholic priests and students of theology. Much of the data is not presented in the volume reviewed but can be found in the unpublished report, "Media of Communication and the Free World as Seen by Czechoslovak, Hungarian, and Polish Refugees" (New York, April 1, 1953), done by International Public Opinion Research, Inc., who conducted the interviews used by Kracauer and Berkman.

The study is intended to document the proposition that the non-Communists of the eastern European satellites developed a set of similar apprehensions, expectations, and attitudes—a "common mentality"—under the unifying impact of the recently installed political and economic systems. This assertion, only partly borne out by the data, appears to be misleading when contrasted with national differences between Hungary, Poland, and Czechoslovakia in traditional images of and attitudes toward political and extra-political authority, traditional patterns of revolt, national aspirations, composition of Communist parties, and class and rural-urban differences. These differences were clearly

brought out in the course of the recent political crises. The authors' conclusion follows partly from their focus on sources of non-Communists' alienation and their superficial treatment of adaptive and assimilative processes and power relations.

Nevertheless, this volume merits the attention of students of public opinion, political science, and social psychology. It is one of the first studies of a Communist society which attempts to relate the expectations of non-Communists to their image of their own society. This is particularly well illustrated by the chapters dealing with the impact of Communist and Western propaganda on the non-Communist population and by the depiction of changes in friendship patterns. The discussion of the relationship between non-Communists and various types of nominal and convinced Communists is also illuminating.

JAN HAJDA

University of Chicago

When Prophecy Fails. By LEON FESTINGER, HENRY W. RIECKEN, and STANLEY SCHACHTER. Minneapolis: University of Minnesota Press, 1956. Pp. viii + 256. \$4.00.

Although they apologize a bit for it, the authors have done something as laudable as it is unusual for social psychologists of this generation. They espied a fleeting social movement important to a line of research they were interested in and took after it. They recruited a team of observers, joined the movement, and watched it from within under great difficulties until its crisis came and went. Their report is of interest as much for the method as for the substance.

The movement itself was a little apocalyptic group with a science-fiction eschatology. Space-men revealed through the medium of an earth-woman that a great flood would change the face of the earth, or at least of the American Midwest, on December 21, 1955; a few chosen people would be carried off to higher spheres in flying saucers. The authors report their first news of the movement, their contacts, then the course of events in the months preceding the date of the predicted catastrophe-victory, the crucial last hours before the zero hour, its coming (although two clocks did not tell the same minute), its passing, and what has happened since. It is an excellent job of reporting, representing,

as it does, a thousand pages of notes from recordings. One is tempted to tell something of the story; I resist the temptation and advise one and all to read it in full.

The story is preceded by a chapter reviewing literature on unfulfilled prophecies and disappointed messiahs. The authors set forth their hypothesis about what may happen when people convinced of and committed to a specific belief requiring action on their part face unequivocal refutation of the belief by events. It was interest in the response of people to the failure of prophecy that spurred the authors to carry out their study. The hypothesis is this: when certain conditions are met, the believers will believe more firmly than ever and will go out to tell the world the good news. The authors work hard at mustering the data that they believe support the hypothesis. It is carefully done, and I have no special quarrel with it. I do quarrel somewhat with their conclusion that, had accidents not dispersed the faithful few, and had they been better apostles, the movement might have taken on new life and growth after the "disconfirmation" of the prophecy. For people as punctilious about their evidence and proofs as the authors are, I think this is getting out too easily.

One might quarrel also with their handling of the prophecy and the prophets. It is made clear that most people in the very small inner circle had a history of chasing flying saucers, dianetics, diet cults, and of getting and seeking messages from other parts of the universe; they were *Seekers*. It is also reported that the very terms and substance of the prophecy took form out of this seeking and to some extent out of a battle of wills of three different mediums, each receiving her messages from a different character in outer space. Yet one senses that the authors think a prophecy is a prophecy, one is more or less like another. From the reports one might have concluded that these are people who have need of a prophecy, this one or some other. I am not sure but that there is some dimension of belief involved other than those of conviction and commitment, which the authors consider crucial. It may be the *need* for conviction and commitment. One thinks, in this connection, of the little groups of Japanese in Hawaii who refused to believe that Japan had been defeated and who read the papers as though they were written in some code and meant the opposite of what they seemed to say (Yukiko Kimura, "A Comparative Study of Collective Adjustment

of the Issei, the First-Generation Japanese, in Hawaii and in the Mainland United States since Pearl Harbor"[unpublished Ph.D. dissertation, University of Chicago, 1952]). One thinks also of Tumin's report of the prophesied appearance of the Virgin; it was "disconfirmed" for some, but many "saw" her. The desire and need to believe are perhaps not the same thing as conviction; they may represent commitment, although not quite in the sense in which the authors use it.

I also think the authors deal too simply with what they call the "rationalization" of the failure of the prophecy. A good many "prophecies" are merely a momentary form of deeper, enduring hopes and fears; the specific prophecy in this case drew together people who obviously had deep-seated problems, yearnings, and dispositions to believe. One might well call the prophecy itself a rationalization, growing out of the deeper feelings and sentiments of these people. The interpretation and reinterpretation of such prophecies is a major process in religious discourse. In such discourse there is constant preoccupation with time and space; past, present, and future are often confused. These people often begin to talk about whether the prophesized event had not already taken place a thousand years ago or whether a mistake might not have been made, thereby confusing this pinpointed date with something to come 10,000 years from now. But, in dealing with this matter, the authors are up against the old dilemma: Shall one stick to a single hypothesis at the risk of oversimplifying the matter in hand or sacrifice the hypothesis to consideration of the full richness of the case and to comparison with many other cases somewhat like it?

The authors have given us a brief appendix on their method of work. They tell us that they had either to study the group secretly or not at all; for it was one of those little groups of fanatics who do not tolerate neutral observers in their midst. This is almost certainly true. There are groups which will not allow such observation (Professor A. P. Elkin in his *Social Anthropology in Melanesia: A Review of Research* [New York: Oxford University Press, 1953] says the day is past when the anthropologist of European race and culture can go where he chooses to do field observation. The more bitter cargo cults and nativistic groups want no European learning their secrets.) This raises many crucial issues concerning research in social science. Some of these issues the authors

discuss quite frankly: the stratagems and deceptions the observers were driven into; the unavoidable effects of the four, non-believing, paid observers feigning belief upon a tiny group of usually less than twenty people; the utter fatigue and strain of constant observation.

But there are some issues they have not discussed. One of them is the ethics of participant observation. Have we a right to study people secretly and, if so, to report the findings to the public in such a way that they can be identified? I do not here propose an answer, but I contend that the terms of the answer are crucial to the future of social science. It brings up, among other things, the question of adopting roles which might, either of themselves or in case of slight mishap (suppose the police had raided the meetings of this group and the observers were faced with revealing their perfidy or going to jail, perhaps even being thought nuts to boot!), do harm to the people studied or of roles which can bring disrepute and suspicion upon their colleagues and upon their profession.

They also have not discussed the effects of secrecy upon the field workers themselves and upon their relations to one another in the course of a grueling and emotionally hazardous experience. I do not say that the authors should have discussed these problems in this book. But I do think the fundamental nature of the researcher's bargain with the people studied needs consideration, perhaps most of all in those cases where the people studied are not the people who furnish the money for the study.

EVERETT C. HUGHES

University of Chicago

The Image. By KENNETH E. BOULDING. Ann Arbor: University of Michigan Press, 1956. Pp. 175.

"This book," the author tells us, "is the result of a unique experience . . . eleven months of vigorous interaction, both playful and serious, with a group of thirty-six able social and biological scientists." It was "written, or rather dictated, in uninterrupted composition," and the reader is asked to "discount, and even to forgive, a certain atmosphere of intellectual exaltation." It is in the light of these statements by the author that our "image" of this book should be formed. It is a poem. It is a charming poem, and, like all good poetry, it induces new

insight. But this reviewer finds an undue readiness to push analogies; an inclination to present, as new, ideas which have had considerable currency (under other names) in one or another of the social sciences; a passion for unification of the sciences which is certainly premature in view of the weakness of the individual sciences, and possibly unattainable (co-operation among unlike sciences rather than unification of science may well be the better aim).

The final plea for "Eiconics, a New Science" must surely appear even to the author, as the mood of exaltation wanes, as embodying exaggerated hopes. Of course, when Professor Boulding explains that "the new structures, as new intellectual structures always have done, will have to live in an underworld . . . of deviant professors, gifted amateurs and moderate crackpots," he leaves the doubting reviewer worrying lest the orthodox, conservative professors in the various separate disciplines, with human prejudices and vested interests, should be blind to the new vision of this new science. It is well to be worried; but it would be unfortunate if we became too worried. There still is work for the old-fashioned.

V. W. BLADEN

University of Toronto

Demographic Analysis: Selected Readings. Edited by JOSEPH J. SPENGLER and OTIS DUDLEY DUNCAN. Glencoe, Ill.: Free Press, 1956. Pp. xiii+819. \$9.50.

Demographic Analysis is a companion volume of readings to the editors' *Population Theory and Policy*, which it is intended to complement by exemplifying "the work of the research demographer dealing with delimited concrete problems." Since articles on demographic topics are widely scattered, appearing in both social-science and natural-science journals, this collection of no less than sixty-three separate selections has a potential value not usually possessed by books of readings in other fields. It is likely to be the only demographic anthology of such size and scope for some time to come. Unfortunately, the book has a number of serious deficiencies that greatly limit its usefulness.

To begin with, it is hard to see why the editors have chosen to include only journal articles, a restriction they did not follow in the companion volume. Much of the best work in demography appears in book form, and books

and monographs on population are not necessarily more accessible than journals, particularly official publications that are difficult to locate. The exclusion of selections from books makes the volume much less usable "as a relatively self-contained basis for a course in demographic analysis." The editors' decision largely to exclude methodological articles on the ground that texts and manuals on method are widely available is a sound one, but the omission of book selections presumably accounts for the absence of any readings on British or Indian population, about which much is known, and the inclusion of two readings on Chinese population, about which little is known.

Second, the range of selections betrays a certain national parochialism. There are four articles from an international journal published in London, *Population Studies* (two of them, however, are by Americans), and one article from a Canadian journal. The remainder are from American journals, although some of the authors are foreign scholars. Yet demography, more than other fields of sociology, is a truly international science in both theory and methods. Is it expecting too much of the editors to suggest that they might at least have translated a few French studies? French demographers have a journal of their own, are richly productive, and combine technical skill with a historical sophistication unmatched by most American scholars. Even English-speaking non-Americans are poorly represented: there are no selections by D. V. Glass or Colin Clark and, while John Hajnal is represented, his justly famous article (1947) on the postwar international recovery of births is not included.

Finally, there are too few selections of special interest to sociologists. In this country demography has largely become a field of sociology. And in recent years there has been an increase in the number of students of population who not only have received formal training as sociologists but bring to the analysis of demographic data a distinctive sociological perspective. They are represented in this volume, to be sure, by the excellent articles of Kingsley Davis, Kurt Mayer, Wilbert Moore, and Hilda Hertz Golden. There are also several selections which suggest the broad sociological implications of general areas of research like the labor force. That they are inconsistent with the book's declared emphasis on concrete problems would scarcely matter, were it not for the fact that they are so painfully and unrelievedly pedes-

trian as to stand in embarrassing contrast to the high level of technical demographic and economic skill displayed by the more typical selections.

Class differences in fertility are discussed only in one part of one selection, although they are a subject of considerable interest to sociologists as indicators of variant life-styles and in their bearing on social mobility; yet many excellent studies were available to the editors. Recent research on group differences in contraceptive values and practices and on preferences in size of family is also underrepresented, perhaps because such studies use survey methods rather than demographic techniques. But, if so, why is a brief résumé of the Indianapolis Study included?

In fairness to the editors, it should be noted that their companion volume contains a larger number of readings of sociological relevance; however, some of my strictures on the collection under review apply to it as well.

DENNIS H. WRONG

Brown University

Le Mouvement Poujade. By STANLEY HOFFMANN, with the collaboration of MICHEL DES ACCORDS, SERGE HURIG, JEAN DU ROSTU, and JEAN-MICHEL ROYER. Preface by JEAN MEYNAUD. Paris: Librairie Armand Colin, 1956. Pp. xxvii+417.

The movement led by Pierre Poujade started in the summer of 1953 as a direct-action, anti-parliamentarian, taxpayers' protest movement among small merchants and artisans in the economically stagnant or declining parts of provincial France. It grew rapidly and, after some successes in municipal elections, as well as elections for the social security administration and in chamber of commerce elections, it obtained nearly 2.5 million votes or 9 per cent of the registered voters in the elections for the lower house of the national parliament on January 2, 1956. The movement which had denounced all parliamentarians as "rotten" gained 52 seats in the chamber of deputies. Hoffmann's study covers the development of the movement up to this point, except for a brief postscript on the difficulties met by the Poujadists after entering parliament.

Jean Meynaud in the Preface appraises Hoffmann's findings and points out some further problems to be investigated. He also em-

phasizes that Hoffmann had to work "almost single-handed," which seems to be one reason why we do not find as thorough an ecological analysis of the 1956 elections as one would like.

In the first half of the book Hoffmann gives a detailed, well-documented account of the origin, growth, and internal development of the movement, beginning with a discussion of the economic and fiscal background and a characterization of Poujade's personality. This is an excellent piece of sociologically oriented historiography. Hoffmann has made careful use of primary sources (pamphlets and newspapers, campaign literature, informal interviews, etc.)

The remainder of the book is devoted to a systematic analysis of the ideology (*mythologie*) of the movement, of its organization and internal operation, of the relations between the movement and occupational organizations (trade associations, chambers of commerce, farmers' organizations, trade unions, etc.), and, finally, of the relations with political parties.

Throughout the text we find examples of posters, important documents, and chronological enumerations of major events, which are very helpful.

The author uses comparisons and interpretations sparingly, except in the concluding chapter. He thinks Poujadism is essentially a movement of protest against the modernization of the French economy and the accompanying changes in the style of life—urbanization, depopulation of certain rural sections, centralization of business, and the replacement of the independent small businessman by the salaried employee and manager. It is a movement of the small people who never had been politically active. Poujade himself, a veteran of the Free French Forces, may be described as a *déclassé*, and so are most of his lieutenants.

The Poujadists' attitude toward parliament has been vacillating; for a long time they avoided making political promises and demanded instead the convocation of the *états généraux*, this being their central political "myth," which then would work out a program and a policy. In their negative attitude toward the state, Hoffmann sees an essential difference from fascism. And yet the very structure of the movement; the authoritarian leadership; the phraseology of Poujade's speeches; his opportunistic appeals to farmers, salaried employees, and other groups; the infiltration of special-interest organizations; and, last but not least, the original direct resistance against law enforcement

—the prevention of “controls,” of seizures and foreclosures, the shopkeepers’ strikes, and the use of boycotts, its anti-Semitism, its exploitation of the Algerian question—these and numerous other traits of the movement recall certain aspects of the early Nazi movement and especially some of the farmers’ movements that paved the way.

While Hoffmann does not seem aware of all these resemblances, he clearly sees the conditions which make a movement like this possible, in particular the fact that large parts of the French nation have not yet really accepted the modern form of representative government (p. 382). The Poujade movement is a new and in many respects unique expression of this anti-parliamentarian undercurrent.

For the student of social movements, Hoffmann’s study offers an extremely interesting case.

RUDOLF HEBERLE

Louisiana State University

Diagnosing Human Relations in Organization.

By CHRIS ARGYRIS. New Haven, Conn.: Labor and Management Center, Yale University, 1956.

This case study of relationships between management and nursing in a hospital is the second of the studies in organizational behavior undertaken by Professor Argyris at the Yale Labor and Management Center and is a direct outgrowth of his earlier study of a bank. Members of this center are involved in the development of their own concepts and techniques and their own theoretical framework. Their language and approach must be learned before the study can be understood. Outlining the approach is perhaps the best means of reviewing this book.

The “fusion process” focuses the chief theoretical concepts. This is the resultant of the attempts of workers and organization “simultaneously to actualize themselves by making instruments toward that end of each other.” Techniques are developed for describing the mutual expectancies of personnel and formal organization and to estimate the degree of “self-actualization” in the relationship. Scores for this may be constructed. A “satisfactory fusion score,” involving the simultaneous self-actualization of both individual and organization awaits further work by this group.

General notions of organization processes are also employed, such as workflow, authority, perpetuation, identification, and communication. The emphasis is upon organizational “steady state” rather than upon relationships of change; upon tendencies toward stability rather than disorganization. Equilibrium, feedback, and circular processes are stressed.

In the actual research process, members of each of the groups studied report their views of organizational problems which concern them. The “dominant assumptions” of their views are analyzed, and their suggestions for action are detailed. Data are secured by use of semistructured interviews. From these the “dominant predispositions” of individuals are abstracted, and their roles in the organizational steady state are analyzed. The book details these analyses for nurses and management. Corrective measures stem from knowledge gained by the research. They depend upon the resultant increased awareness of self and organization for the individuals concerned. Hopefully this can lead to self-modification of behavior. At this level the book is explicitly highly tentative.

The crucial definitions involved stem from the earlier work. Predispositions “represent observed or inferred behavioral tendencies in the individual. They are supposed to depict the behavioral directions toward which the individual will tend to strive if free to behave as he desires.” This seems to be a rediscovery of the concept of “attitude.” Personality factors are specially defined as “any desire expressed by the individual or any desire on the part of the individual inferred by the researcher.” This is far from a rigorous formulation.

The emphases of the book are threefold: development and presentation of a method, analysis of a situation, and contributions to a theory. The method certainly works and had predictive value with some nurses. The situation would seem to have been well analyzed. But the method is cumbersome and excessively private. Certainly the conceptual apparatus of social research needs clarifying and sharpening. But there are perils if each school invents its own separate system. Professor Argyris fails adequately to build upon the work of others: in this book almost no references are cited to organizational studies outside those done by him and his colleagues, despite the existence of some closely related work. An entailed problem is that data secured through such private modes of analysis cannot readily be added to existing

general knowledge and fail to make their contribution to general scientific growth. Similarly, the theoretical contributions may not focus upon general concerns but upon private problems: here "feedback," "workflow," "steady state," etc.

The work of the research group, while undoubtedly successful and skilful, suffers from insulation. There are many other more readily available modes of analysis which would do the task at least as well. It is not clearly demonstrated that learning this special language will lead to new analytic skills. The theory clearly stems from Argyris' special interests, and its general relevance is accordingly limited.

Perhaps our field is still so feebly developed that each new endeavor in research must create its own language, techniques, and theory. Some of our more conventional approaches may still be used with as great profit, perhaps greater. They provide for intellectual interchange among colleagues dealing with shared problems in areas broader than single departments. Finally—and what is most important—the undoubted intrinsic contributions of Argyris in this important area of individual-organizational relationships would emerge more clearly if related to the general field of organizational research.

HARVEY L. SMITH

University of North Carolina

The Psychology of Sex Offenders. By ALBERT ELLIS and RALPH BRANCALE. Springfield, Ill.: Charles C Thomas, 1956. Pp. vii+132.

This little volume is one of a series published in the "Bannerstone Division of American Lectures in Public Protection." The title of this monograph is somewhat misleading, since it implies a general analysis of sex offenders, while in reality it is restricted to a limited group of three hundred male adult offenders who were examined at the New Jersey Diagnostic Center between April, 1949, and June, 1950. Unfortunately, it is never made quite clear whether the sample includes all convicted male offenders in the state or those from a specific region. A second shortcoming is the failure to present background information as to age, occupation, education, and socioeconomic status of this group of offenders.

Within the limited number of pages which constitute this book, an attempt is made to

cover a wide range of material. The authors establish a descriptive system for the classification of sex offenders and then make comparisons between offenders in this sample and offenders discussed in other published research. Comparisons become difficult because the authors can never be quite certain that their definitions of the various sex offenses are similar to the definitions used in other studies.

Hypotheses suggested by other research are then accepted or rejected on the basis of material gathered on the sample used for this study. Here the authors depart from all quantitative considerations and, in the tradition of Kinsey, become dramatic in describing what constitutes a sex offender. A fine line is drawn between sexual deviation and psychiatric deviation. Placement within either of these categories seems to depend upon whether or not one is caught in a sexual activity considered contrary to the norms of our society.

The authors are quite correct in stating that there is a need for the "fostering of research, more research, and still more research in the problems of sex offences and their treatment."

DAVID GOTTLIEB

Chicago, Illinois

Non-parametric Statistics for the Behavioral Sciences. By SIDNEY SIEGEL. New York: McGraw-Hill Book Co., Inc., 1956. Pp. xvii+312. \$6.50.

Finally, a book about non-parametric methods! After the years of sliding along with statistical methods designed to deal with data of a sort which we hardly ever have in sociology, merely the announcement of this book gave rise to eager anticipation. A first glance at the book promised even more. Its contents are summarized by a magnificent chart just inside the cover: Are you concerned with a test based on a single sample, on two samples, or on more than two samples? If more than one sample, are they independent or related? Are the data considered as lying in a nominal scale, an order scale, or an interval scale? A cross-tabulation of these questions shows both the relevant tests presented and where to find them in the book. When one turns to any particular test, he again finds an excellent organization of the discussion. First the purpose of the test is discussed, then the method and its rationale; then an example is worked through in a systematic fashion. Not

only is everything well arranged, the discussions are, by and large, clear, and the compendium of tabulations at the end of the book is by itself worth the price of the book. The 97-item Bibliography gives not only background references for the tests presented but also references to additional non-parametric techniques.

It is perhaps hardly necessary to discuss the meaning of "non-parametric." Professor Siegel treats tests of hypotheses which primarily make use of ranks or unordered categories or of other scales weaker than the ordinary (real) number system. The essential characteristic of non-parametric tests is that relatively few assumptions are made about the underlying distribution which gives rise to the data; this is the origin of an alternative usage, by which non-parametric methods are called "distribution-free" methods.

A closer reading of this book dampens somewhat the initial enthusiasm. Chapters ii and iii, which appear to have a didactic purpose, do not come up to expectations. The reader who has any experience with statistics is likely to skip them, and he will miss nothing. But to the novice this part of the book, in part, is outright misleading. Admittedly, it is not easy to give a clear exposition of the notion of testing hypotheses, but that is hardly an excuse to omit all mention of the fact that statistical tests apply only if based on random or probability samples. Indeed, this is one of the major problems of the use of statistical inference in the behavioral sciences, and in a book addressed to behavioral scientists this problem should not be completely ignored. One way to rationalize the use of tests when the randomness of samples is questionable is to use the criteria of a statistical test as a yardstick, e.g.: If the sample had been random, a value as large as that observed would have been found significantly different from zero at the 5% level. Such a statement as this would, of course, be greatly strengthened if added to it were: The non-random factors in the selection of the sample would tend to diminish the value of the statistic. Though statements of this sort seem to be an easy way out of the untenable situation in which behavioral scientists commonly find themselves when testing hypotheses, the reviewer has never seen this argument employed.

The ignoring of the sampling problem undermines some of the examples in this book. I am sure that better examples will appear in future editions. When a problem clearly points toward

more specific alternative hypotheses, it is both wrong and confusing to the student to let the alternative hypothesis be simply the denial of the null hypothesis. Either the example used to illustrate the McNemar test does not do it justice, or this is a very curious test. As presented, it is a chi-square test of the hypothesis that a population proportion is 0.5. Since the direction of the deviations is of interest, the chi-square test is peculiarly inappropriate, and it does not help to speak about one-tailed and two-tailed chi-square tests, since the null hypothesis will be rejected only for large values of chi-square; it is possible to make an independent test of the direction of deviations, and the combination of these two tests results in what Professor Siegel calls a "one-tailed test."

The final chapter presents the contingency coefficient, C , one of the classical coefficients devoid of any rationale, and measures of rank correlation which have been relatively accessible in Kendall's *Rank Correlation Methods*. The reviewer missed here the sophistication of the recent discussion of measures of association by Goodman and Kruskal (*Journal of the American Statistical Association*, XLIX, 732-64).

These criticisms weigh little when compared to the great usefulness of this book: they are details. The appearance of Professor Siegel's book is an important event for sociologists, and not only its subject matter but also its general excellence make it important.

ARNOLD SIMMEL

Columbia University

The Theory of Social Structure (with a Memoir by Meyer Fortes). By S. F. NADEL. Glencoe, Ill.: Free Press, 1957. Pp. xvi+159. \$5.00.

This is one of those rare books that are as stimulating on the third reading as on the first. It will undoubtedly become one of the classic theoretical works in social science. Its merit derives from the systematic exploration of the implications of considering "social structure" as a specialized conceptual tool.

For Nadel "structure" is "an ordered arrangement of parts . . . being relatively invariant, while the parts themselves are variable" (p. 8). A picture or model of the structure of a society is built up "through abstracting from the concrete population and its behaviour the patterns or network (or 'system') of relation-

ships obtaining between actors in their capacity of playing roles relative to one another" (p. 12). The book is a consideration of the serious conceptual and methodological difficulties involved in describing a society in terms of its structure.

The author's aim is to develop propositions that will map out the area of social structure and thus provide the basis for properly conducted empirical investigation. Theory is seen by Nadel as consisting of propositions; it functions as a logical framework for procedure and analysis. From this point of view an acceptable theory of social structure presupposes an adequate theory of roles, which, according to Nadel, has been lacking. Hence the greater part of the book is devoted to the development of an adequate framework for the analysis of role systems and their articulation. Here he makes his most important contribution, a well-reasoned presentation of basic problems in role analysis: role as a bridge concept intermediary between society and individual and as a class (logical) concept; properties underlying role classification; role cues; internal structure of roles; their basic or pivotal attributes; the allocation, accommodation, conformity, deviance of roles; their interconnection and the coherence of role systems; independent and dependent roles; problems of abstracting roles; quantification and comparison of role systems; change in them through time, etc.

One of the many productive ideas explored by Nadel is the approach to role in terms of the class concept in logic. A role brings together numbers of individuals in virtue of certain common properties. This perspective allows the classification of roles according to underlying properties which he calls *contingent* and *achievement*. Contingent properties are "physiological characteristics (sex, age, somatic features), qualities of descent and extraction, personality or character traits, or qualities resulting from some extraneous turn of events (as when we speak of a 'veteran' or a 'widow')" (p. 23). Achievement properties "represent characteristic ways of acting, resting, on particular proficiencies, interests or attitudes" (p. 23). Nadel maintains that one distinctive or pivotal property serves to signal and entail all the other properties of the role, and he calls it the *governing property*. Roles defined by a contingent governing property (which is an inevitable or fortuitous state of the individual) are called *recruitment* roles, since the governing

property indicates the method of recruitment. Roles defined by an achievement-governing property (which is a behavioral attribute, active or passive, which individuals are free to choose as a goal or objective) are called *achievement* roles, as the recruitment is implicit in the precondition of achievement. Achievement roles correspond roughly to the familiar "achieved status," though recruitment roles imply the elaboration into roles of the governing properties rather than the investment of persons with offices or roles on inherited grounds as implied in the familiar concept of "ascribed status." In other words, recruitment roles represent an accommodation to inevitable differences between individuals; the roles are adjustments to the differences.

In the memoir Meyer Fortes points out that Nadel's work has an ethnographic and a theoretical side. This separation of data from theory is an indication of the present immaturity of social science rather than any reflection on Nadel's brilliance. Further progress in the field will eventually require an integration of data and theory.

A. KIMBALL ROMNEY

University of Chicago

Common Frontiers of the Social Sciences.
 Edited by MIRRA KOMAROVSKY. Glencoe, Ill.: Free Press and Falcon's Wing Press, 1957. Pp. viii+440. \$6.00.

This volume contains ten contributions under as many separate headings, plus a thirty-page Introduction by the editor, grouped in two parts: Part I, "History and Social Research," and Part II, "Economics and Sociology." The contributions, written by eighteen different authors, include essays, "notes," monographs, and a "debate" on "History and Public Opinion Research." The general purpose of the symposium, as stated by the editor, was to contribute to the somewhat neglected study of the interrelations of the social studies. The general method was to subject to scrutiny—in some cases a reciprocal scrutiny—specific research topics or groups of relatively specific studies in particular overlapping fields of research. As the part-titles indicate, the present volume reports on the following: (1) inquiries into the reciprocal relations and possibilities of cross-fertilization of historical research; (2) research into topics of historical interest by methods

other than those commonly used by professional historians; and (3) somewhat similar inquiries into the reciprocal relations of economics and sociology. A footnote on page 3 intimates that this volume was conceived as part of a series; however, the series is nowhere more specifically identified.

The general theme to which this book is devoted, especially the possibilities of using in one established discipline the methods developed in another, is certainly important. Those who are actively interested in the problem and who have done some work on it will probably find the intensive study of one or more of the contributions gathered here quite rewarding. The material requires hard work from the reader. At an ordinary careful reading, the present reviewer found the material stimulating at various points, difficult and technical at others, notably in Part II, where considerable reference is made to Keynesian economic theory, examined in the context of sociological concepts developed by Talcott Parsons and associates. Komarovsky's Introduction is excellent in every way.

FLOYD N. HOUSE

University of Virginia

The Student Physician: Introductory Studies in the Sociology of Medical Education. Edited by ROBERT K. MERTON, GEORGE READER, and PATRICIA L. KENDALL. Cambridge: Harvard University Press (for the Commonwealth Fund). Pp. xii+360. \$5.00.

This volume, consisting of ten short pieces by eleven contributors, serves three purposes: it presents the background and theoretical orientations of the large-scale study of medical students conducted by Columbia University's Bureau of Applied Social Research; it describes in some detail the Cornell Medical School's experimental program in "family care" as a teaching device; and it presents some initial findings of the BASR research.

The sociologist's major interest in the volume lies in the BASR study and its initial findings. Six essays are devoted to these materials: Natalie Rogoff analyzes correlates of early and late decisions to enter medical school; Wagner Thielens, Jr., compares law and medical students' patterns of vocational decision; Patricia Kendall and Hanan C. Selvin present panel data on preferences for specialty versus general

practice; Mary Jean Huntington examines factors related to the students' self-perceptions as "doctors" rather than "students"; William Martin reports on the development of the students' abilities to inhibit preferences for particular types of patients; and Renée C. Fox interprets many phenomena of medical education as "training for uncertainty."

Robert K. Merton, in an introductory essay, describes the BASR project as a function of the convergence of no less than ten historical trends in medicine and sociology, defines the separate tasks of sociology and psychology, and presents a very suggestive list of values and value conflicts in medical practice.

Naturally, this volume will be of high interest to specialists in medical sociology and to social psychologists interested in the applications in another content area of such BASR conceptualizations as "cross-pressure" and "decision-making," but the major significance of this book may be in the area of research methodology.

In the last decade a series of major developments in research methods has come from Columbia University. The end-product may well be considered a special style of survey research. Thus, with the exception of Fox's essay, which is avowedly "qualitative," it is impossible to tell whether the research reports were done by the same person, by different people, or by a group. Yet anyone familiar with contemporary research reports will know, on reading a page or two, that each is a product of "The Bureau."

At first glance, we note the trademark of this school—the absence of significance tests—and, as in other recent Bureau publications, an appendix purporting to justify their exclusion. While some of the argument is trenchant (the cases are, as they say, a universe, not a sample) and some of it plain foolish (e.g., If you use significance tests, you don't report non-significant findings, and sometimes the failure of a hypothesis is interesting), it is our opinion that it is not the absence of significance tests but the absence of *any formal criterion* for arriving at a conclusion which typifies this approach.

The essence of the Columbian exposition of research data seems to consist of intuitive interpretations of heavily partialized tables. While almost anyone is in favor of the partials, the intuitive part raises some problems.

Consider, for a moment, "percentage differences." Without significance tests, how is one going to tell when a finding is worth interpretation? On page 201, a 13 per cent difference

between the extremes of three groups is considered a fairly firm finding. On page 135, however, another 13 per cent difference among three groups is shrugged off as relatively minor. On page 128 a 19 per cent difference among five groups (which, in addition, shows an internally inconsistent trend) is presented as a flat finding, with no qualification at all. The reader is left with no idea of how the researchers determine which tables do or do not show relationships.

Again, on page 191 one of the authors wants to sum scores on five items to get an index. He says that the normality of the distribution of the sums (which, of course, is not tested) "suggests that the questions differentiate students according to their preferential attitudes" and proceeds to use the sum as an index. Six pages later he notes that another such distribution "is markedly skewed" (it is also bi-modal) and then proceeds to use the sum as an index. We don't know whether normal or non-normal distributions make better indexes, but we are entitled to doubt that the author is using any criterion at all in deciding whether it is justifiable to use a sum as an index.

And again, on page 143, it is claimed that medical students perceive more undergraduate scholastic competition on the basis of 49 per cent reporting "a great deal of competition" as compared with 18 per cent of the law students. However, seven pages later the same author, in another context, says: "It is natural that students, perceiving the competitive situations in which they are engaged, might well react with feelings of concern," and then he examines data on "concern with school standing." The table here shows 13 per cent of the medical students and 14 per cent of the law students reporting such concern. Now in the methodological appendix to the book it is claimed that "internal consistency" can replace significance tests as a criterion. We suggest that either it is *not* natural that feelings of concern arise from competition or the criterion of internal consistency has been applied in a somewhat relaxed form.

Finally, on page 181 the finding that there is a relationship between students' perception of themselves as "doctors" and their being treated as such by others is considered as "lending support" to the proposition that "there is a consonance between the status that students believe others in their role set assign them and the status to which students assign themselves." The author says that this proposition has long been recognized but little investigated. How-

ever, on page 203 another author reports that there is no relationship between the students' self-confidence and their grades, and "it is therefore not the case that self-evaluations correspond to faculty judgment." Is this a "dis-confirmation" of the proposition? No, it is suggested that the two measures tap different dimensions! The reader wonders exactly what sort of evidence it would take to "dis-confirm" hypotheses in the new methodology.

The point is not that there are internal inconsistencies in the materials. Rather we would suggest that, without formal criteria, tests of significance, or other numerical criteria, it is almost impossible for the research worker to be consistent, regardless of the elegance of his arguments in a methodological appendix. Furthermore, without some communicable decision process, it is even less possible for different investigators to arrive at the same conclusions from data. The net result is "art," not "science."

In a very literal sense, the new survey methodology comes close to an art form in which a verbal argument is presented, accompanied by complex and fascinating counterpoint on the IBM machine. Since, however, none but aesthetic grounds can be given for evaluating the harmony between the two, it is the artistic skill of the performer which occupies the attention of the reader. That the performers in this volume are talented, sophisticated, and probably correct in most of their interpretations only makes their method, as method, more dangerous.

JAMES A. DAVIS

University of Chicago

Varieties of Human Value. By CHARLES MORRIS. Chicago: University of Chicago Press, 1956. Pp. xv+209. \$5.00.

The author, a professor of philosophy at the University of Chicago and known to sociologists as the editor of G. H. Mead's lectures, set out to test empirically certain ideas about values. His earlier work, *Paths of Life* (1942), presented the view that values could be understood in terms of three basic dimensions expressed in seven value patterns; and the study here reviewed is intended to test these patterns empirically. The author, one of the collaborators on the *International Encyclopedia of Unified Science*, felt that this approach might bring the study of values within the scope of

unified science. This research undertaking deserves both interest and praise from all sociologists who have seen their own theorists sailing along the paths of speculation unimpeded by the harsh realities of statistical investigation. If philosophers are now using the tools of science, then scientists can hardly disregard those tools.

Morris' study was based on the administration of a document entitled "Ways To Live" to a number of students in the United States, India, China, Japan, Canada, and Norway. In preliminary samples, seven ways of life were described and submitted to the students who had a chance to add additional liked ways of life. In this manner, the number of ways was raised to thirteen. Each way to live was described in about a dozen lines. The subjects were asked to indicate the kind of life they would like to live by marking for each way whether they liked it or disliked it very much, quite a lot, slightly, or were indifferent to it.

The descriptions of these ways of life are open to criticism on the basis of content as well as formulation. The author states that "the thirteen alternatives are on the whole positive in tone, normal rather than abnormal, constructive rather than destructive, beneficent rather than malevolent." In other words, as far as he himself is concerned, the values expressed in the ways of life can be subsumed under a general concept of the "good" which he holds. The omission of values of which the author disapproves introduces an element of one-sidedness which diminishes the value of the study. Terms such as "abnormal," "destructive," "malevolent," could conceivably be applied by some students to some of the thirteen ways listed and not applied by others to certain ways of life which were omitted.

While several ways to live contain statements of a passive or even masochistic nature (e.g., "a person should let himself be used"), there is a noticeable absence of aggressive-sadistic statements. References to the power strivings, so important in our culture, are missing except in the description of one way to live in which action is eulogized, but primarily in terms of the expenditure of body energy in overcoming physical obstacles. None of the ways proposes reckless sensual enjoyment.

Each description contains references to numerous elements of behavior, but not each way to live is described in terms of the same categories of behavior. No attempt was made to try out all possible combinations of the behavior

items composing what was held to be the good life. Several of the ways, however, seem to be overdescribed and thus unnecessarily narrow.

Formulation of the ways is inconsistent. Since subjects were asked to express their "liking," it would have been reasonable to describe the ways in terms of liking, wanting, desiring or in terms of neutral descriptions of behavior. But descriptive terms (e.g., "in this 'design for living,' the individual actively participates . . .") and terms of wanting ("one wants the good things of life . . .") are outnumbered by statements of obligation ("the individual should be active . . ."). In addition, purely factual statements appeared ("Driving ambition and the fanaticism of aesthetic ideas are the signs of discontented people who have lost the capacity to float in the stream of simple, care-free, wholesome enjoyment"). Consequently, any expression from a tested subject about liking a way contains an element of liking the *formulation* of the way. This reviewer, for example, found himself rejecting certain ways which he might have considered acceptable, had they been formulated in terms of volition rather than of obligation. In other cases he felt unable to take a stand concerning a way which carried a description he knew to be false empirically but which he would like to be true: "The good things of life come of their own accord, and come unsought." Does a favorable judgment concerning this way of life mean that it would be nice if that were true, does it mean it would be nice to live as if that were true, or does it mean that the statement is believed to be true?

Since the wording of the descriptions of the ways is so general and vague, it is almost impossible to compare anybody's actual behavior to a way. One begins to wonder what the people who rated the ways actually did like or dislike.

Most of the 2,015 male and 831 female college students who rated the document in the United States were enlisted by members of philosophy departments, but some persons in psychology, the social sciences, and physical education also co-operated. The author concedes the possibility that the sample "is somewhat biased in the direction of students in the humanities, psychology, and social sciences as over and against those in the natural sciences and the professions." The foreign samples also come primarily from universities. They are smaller in number and probably still less representative of the total populations of these countries.

In the study of the results obtained, scaling

techniques and factor analysis as well as simple correlation techniques were used. Multiple and partial correlations were not applied, though they might have contributed to better understanding.

Comparisons show that all ways to live are accepted in all countries, though by different proportions of those tested. Differences between classes or occupations within each country are less pronounced than differences between countries. These findings could be attacked because of the quality of the samples on which they are based.

The factor analysis (carried out by Professor Lyle V. Jones) led to the isolation of five factors contained in the thirteen ways of life: social restraint and self-control, enjoyment and progress in action, withdrawal and self-sufficiency, receptivity and sympathetic concern, and self-indulgence. Again it is found that these factors, though in different proportions, occur in all cultures sampled. The author believes that they constitute the major value dimensions of human behavior, but, in view of his moral censorship in selecting the raw material for these factors, they may constitute the dimensions of the author's value system rather than of that of humanity.

Value attitudes are related to differences in sex, somatotype, temperament, character, intra-cultural traditions, economic status, and size of community. The author finds that "psychological and constitutional differences seem to play the greatest part in the rating of the ways, with population variation and economic status next in importance, and with sex and body size differences playing the smallest part." It is unfortunate that these findings are interpreted in terms of determination, even if it is suggested that this determination may be reciprocal, whereas the data merely show correlation. If one follows the author in his emphasis on the determination of value by body type and if one sees, with him, value as a determinant of human behavior, an acceptance of some form of racial determinism becomes logically inescapable.

The author distinguishes among operative values (that is, values expressed in behavior), conceived values (that is, values embraced in terms of speech or thought), and object values, which are defined as the requirements for the survival of the individual or his group. The individual, as well as his group, is seen by the author in terms of "system," but he treats these

"systems" not as conceptual tools but as real entities. The object values—the survival values—thus become empirically verifiable facts for him. He believes that he has shown that conceived values as well as operative values are related to these object values and thus that he has opened the door not only for a descriptive study of human valuation but for normative findings through science.

This reviewer is unwilling to follow the author in his conclusions, and he has serious doubts as to the validity of some of his specific findings, but there can be no doubt that Morris has shown how a comparative intercultural study of values can be undertaken. If the basic instruments for the gathering of opinions and the samples of individuals tested could be brought up to standards, the findings should prove of extreme interest to sociologists as well as philosophers.

FRANZ ADLER

University of Arkansas

Essays in Sociology and Social Philosophy, Vol. I: *On the Diversity of Morals*. By MORRIS GINSBERG. New York: Macmillan Co., 1957. Pp. xiv+329. \$4.00.

Morris Ginsberg, England's senior sociologist, presents here the first of two projected volumes of his collected papers, of which there are eighteen here, ranging in date from 1932 to 1955 and grouped into two sections, one devoted to ethics, the other to sociology. Ginsberg knows very well that if sociology is to be a positive science, it can have no traffic with normative judgments, and, indeed, he insists upon the propriety and importance of the distinction between ethical and sociological concerns. But, having made the distinction, he believes that sociology can contribute a great deal to ethics. The first eight papers in the book indicate some relevant possibilities.

The ethical essays have in common a central theme, namely, that, despite the diversity of morals to be discerned in human societies, there remain certain constants—for example, moral obligation itself—and that therefore the doctrine of ethical relativity is erroneous. In the author's own words, "Despite the diversity of moral codes, general principles are discoverable which are implicit in all of them and which come increasingly to be recognized as universally binding in the course of moral development."

Ginsberg develops his thesis with both clarity and rigor. Among the essays that touch upon this problem in one way or another, we should like to mention, in particular, the title essay—the Huxley Memorial Lecture of 1953—in which he proposes to prove that “there is no necessary connexion between the diversity of morals and the relativity of ethics.” Ever since Locke’s *Essay Concerning Human Understanding*, sophisticated scholars have been aware of the variability of moral judgments in various societies. But Locke was not arguing that universal moral principles do not exist but only that they are not innate ideas. In spite of variation, we find that conduct everywhere is guided by a principle and referred to a standard. It is this that is universal, and not the codes themselves, and it is this, among other considerations, that removes the curse of relativism from ethics. There are convergences, furthermore, in moral systems—similarities in spite of differences—and all accounts of the good for man ultimately revolve around a very small and restricted number of ideas, such as happiness, wisdom, virtue, and fulfilment. Finally, in our comparison of societies we are driven to a judgment of a higher and a lower ethic in spite of our protestations of neutrality.

In the essays devoted to sociological subjects Ginsberg touches on many issues, including the individual and society, history and sociology, the concept of evolution in sociology, the problem of stages in social evolution, national character, the place of sociology, the study of social institutions, and the claim of eugenics. There are, in addition, two first-rate papers devoted, respectively, to Comte and to Durkheim.

One soon discovers that if this is sociology, it is also literary and philosophic discourse of a high order. At the same time, there is a certain benign, almost old-fashioned, flavor about this book. These are the papers of a man who has mastered the essay form and one who apparently believes, as few Americans do, that sociology can still be written in discursive prose. It is old-fashioned, too, in that the author treats his predecessors back at least as far as Montesquieu as if they were his contemporaries and worthy of respect—not only as classical figures in the history of social thought but as men whose theories still merit the most serious reflection. And, finally, even if it strikes a provincial note, we cannot help remarking that, though Ginsberg exhibits an intimate knowledge of the French and German literature, his essays are almost

wholly devoid of references to American sociology. We do not by any means regard this as a defect in an otherwise admirable collection of sociological papers. But it serves to indicate at least the regrettable distance that still separates our senior English colleagues from ourselves.

ROBERT BIERSTEDT

City College of New York

Essays in Human Relations. By DAVID GREENWOOD. Washington, D.C.: Public Affairs Press, 1956. Pp. viii+76. \$2.00.

This book, by a visiting British scholar, is a collection of six evaluative essays, two of which he devotes to the Kinsey studies, three to family research and theory, and one to Parsons’ concept of personality. In the first paper he rightly chastises Kinsey for his conceptions of sex as primarily biological, culture as unnatural, the equivalence of all orgasms, and the quantitative criterion of normality. If Greenwood had, however, built upon the observations of previous commentators, such as Kuhn, Gorer, and Clausen, his analysis of Kinsey’s implicit assumptions might have been more profound. In the second paper the author takes up the significance for family adjustment of Kinsey’s claim that, while men of different social classes differ vastly in their sexual behavior, women do not. (I am inclined to challenge Kinsey’s latter finding on the ground that his sample contained too few lower-class women for reliable generalization.)

The third paper gives us a history of research on “family group patterns” by such practitioners as LePlay, Zimmerman, Chapin, and Stouffer. Rambling over a vast methodological terrain, the commentary is too condensed and technical for the undergraduate and yet not particularly enlightening to the research specialist. Next, the author offers *précises* of the theories of family evolution of LePlay, Sorokin, Spengler, Zimmerman, Engels, and Kingsley Davis. With Zimmerman, he shares a disdain for contemporary individualism. “The tragedy of the contemporary situation,” he tells us, “is that all too few members of western civilization realize that the existing conditions are distinctly analogous to the period of family decay which preceded the ultimate decline of Greece and Rome.” He makes this sweeping statement without bothering to confront the argument of

many American family sociologists, that the contemporary urban middle-class family is a viable institution functionally adapted to a changing industrial society. The fifth essay is a thoughtful and statistically sophisticated look at the difficulties in assessing age at marriage. We learn that, while the data are inadequate, the age at first marriage probably increased from Colonial times to about 1890 and has probably decreased since then. In fact, however, "we know very little about the age at marriage in the United States" and are unlikely to learn more until the methods of data collection and compilation by the Census Bureau are improved.

I found his last paper, an exposition of Parsons' concept of personality, to be the most interesting. While he finds much of value in Parsons' synthesis, he criticizes it as being too much concerned with intra-organismic events and too little with the social-environmental field. Moreover, he suggests that the theory is deficient in operational definitions and predictive power. While this is probably true, it should be pointed out that Parsons is trying to relate personality structure to social structure within a consistent conceptual framework and is not attempting to make clinical predictions about individual personalities.

These papers are uneven in literary style and sociological substance. They are often ponderous and pedestrian. (The text, by the way, contains several glaring typographical errors.) At his best, however, Greenwood writes clearly and gives us a useful perspective based upon a fusion of European and American academic traditions. It is to be hoped that he will follow this collection of disparate essays with a more sustained exploration of a single area.

JEROME HIMELHOCH

Brandeis University

most unique of all living creatures, by virtue of his possession—in so highly developed a degree—of the capacity for learning. Indeed, the species character which should be part of the definition of *Homo sapiens* is educability." Between Preface and Conclusion, the author includes negative arguments against looking at man as a purely biological animal. He begins by reviewing briefly from the historical point of view some of the early philosophies about the nature of man but soon becomes deeply engrossed in the task of proving Darwin and Freud wrong. This, in turn, leads to a consideration of etiological factors in the production of criminality, which consideration serves further to prove the author's thesis, namely, a repudiation of reductionistic philosophies that man is "nothing but" biological evolution and hereditary constitution.

The author is trying to disprove the static view of man, and does so expertly and completely. To him, human nature expresses the interaction of three systems: genetic endowment, uterine environment, and cultural environment. He apparently sees each of them as equally important, and he damns all who exalt one above the others or who neglect any of the three.

Many today would question the influence of the uterine environment in determining human nature, and many would emphasize the dynamic aspects of man's cultural environment. However, each person is entitled to his own views, and it can be said to this author's credit that at least he has thought deeply about these things.

The book makes enjoyable and interesting reading. It is somewhat philosophical in nature, but, for the most part, describes the author's personal views.

ROBERT R. MEZER

Boston, Massachusetts

The Biosocial Nature of Man. By ASHLEY MONTAGU. New York: Grove Press, 1956. Pp. 123. \$1.00.

In the Preface the author states: "In this little book I have attempted to deal with some of the questions which must be asked if one is to gain some understanding of the nature of human nature." In the Conclusion, the author states: "What we need to understand as students of man is that the biosocial nature of man is such that he may be truly described as the

The Child's Conception of Space. By JEAN PIAGET and BARBEL INHELDER. Translated by F. J. LANGDON and J. L. LUNZER. New York: Humanities Press, Inc., 1956. Pp. xii+490. \$8.00.

Most American sociologists are familiar with Piaget's early studies of the development of language and thought, judgment and reasoning, representation of the world, conceptions of physical causality, and moral judgment in the

child. Since the above work, however, Piaget and his collaborators have turned out some fourteen additional books, exploring the conceptions of reality or being, of number, time, movement, speed, and, in the present volume, conceptual or representative (as opposed to perceptual) space.

Here, they seek to demonstrate that the earliest mode of conceiving space is topological rather than Euclidean: the child first distinguishes and represents objects by means of such topological relations as proximity, separation, order, and inclosure rather than by the manner in which linearity, angularity, and the like contribute to conceptualized form or shape. Only later, as he becomes able to take the viewpoints of others, can he deal accurately with the representation (and construction) of simple projective lines and perspectives, for such "projective relationships" presume the "inter-co-ordination of objects, separated in space, as opposed to the internal analysis of isolated objects by means of topological relationships." Both Euclidean and projective space stem developmentally, as well as logically, from topological space, Euclidean apparently being ontogenetically the most advanced. (The more general perceptual reality is treated in *The Construction of Reality in the Child*, while Euclidean space is reserved for fuller treatment in *La Géométrie spontanée de l'enfant*.)

It is curious that Piaget pays little attention to the role of the cultural content of social life in the development of intelligence, for his pet antagonist is the sort of psychologist who claims that the world "is" Euclidean and that all we do is perceive its true Euclidean character. On the contrary, Piaget insists, perception, sensation, and, indeed, the very world must be built up or constructed by the ongoing co-ordination of actions centered on portions of external reality. But if it is selective action that constructs the representation of space, then the development of that representation may vary from culture to culture insofar as culture implies variable, selective action. Intensive and sophisticated replication is badly needed to explain the role of culture in the development and form of intelligence.

But this criticism is intended to qualify the admirable achievement of Piaget. He has given psychology a theory of cognition (including the development of cognitive processes) that is unmatched in detail and sophistication by anyone in the United States. For the sociologist, his

work may be a break-through to a more empirical sociology of knowledge than exists at present. As to theory, he presents a "genetic" epistemology that is a necessary complement to a "sociocultural" epistemology. On the level of research, his writings are larded with a variety of ingenious experimental situations that might well be used to test and refine empirically some of the important analyses of "world-view" or "mentality" advanced by such writers as Durkheim, Whorf, Weber, Mauss, Mannheim, and Redfield. Indeed, solely this methodological accomplishment of rendering into concrete actions or operations such notions as that of the straight line and of temporal simultaneity makes Piaget enlightening reading for any social scientist.

ELLIOT FREIDSON

City College of New York

Cultural Pluralism and the American Idea: An Essay in Social Philosophy. By HORACE M. KALLEN. Philadelphia: University of Pennsylvania Press, 1956. Pp. 208. \$5.00.

This volume is the first in a series which the Albert M. Greenfield Center for Human Relations at the University of Pennsylvania proposes to issue as its "Studies in Human Relations." It contains three essays of Horace M. Kallen on "Cultural Pluralism and the American Idea," pertinent comments by eight discussants, and a reprise by Kallen, in which he comments on the commentators. Among the latter are jurists, philosophers, educators, organizers, and one sociologist (Stanley H. Chapman). The volume is written in distinguished English and beautifully printed, but there is no index.

The work ought to be challenging fare for sociologists. The author, a professor of philosophy at the New School of Social Research in New York, is known as the vigorous protagonist of cultural pluralism, which he presents here as *the American Idea*, formulated by the Founding Fathers, symbolized in the great seal of the United States, and epitomized in the motto *e pluribus unum*; the latter he explains as pointed toward an orchestrated union of diversities and thus fundamentally opposed to the mechanized unity, or uniformity, of totalitarian regimes. He would not deny that nativism, know-nothingism, daughterism, klanism, whittism, and all the various philosophies of Ameri-

canization and the melting pot are American ideas of some sort, but he would assert that they are contradictory to *the* American Idea, which is the idea of freedom and opportunity or, as one might say, of the non-establishment in America of any autocratic idea which does not recognize the validity of other ideas.

This is the essence of what Kallen says in many cogently reasoned variations in the second and third of his essays. In the first, entitled "Of Meanings of Culture," which could be used as required reading in introductory sociology classes, he deals with the evolution of the term "culture" itself. He describes the Hellenic, Hebraic, and Christian ideas of what he calls (nighttime) leisure and the contemplative life, in contradistinction to the ideas of (daytime) labor and the active life, and shows that the former were equated with culture until recently, even among "the Joneses of the Soviet élite." But, much to the satisfaction of the social scientist, he ends up on a note of approval of the more inclusive, and hence more democratic, idea of culture to which sociologists and anthropologists have accustomed themselves and their readers and listeners. He praises the anthropological studies of a wide variety of the ways and works of men in their life together, as well as Florian Znaniecki's concept of "national culture societies" (cf. my review of Znaniecki's *Modern Nationalities: A Sociological Study*, in this *Journal*, LIX, No. 2 [September, 1953], 169-71) as the basis of a modern science of culture. Unfortunately, he stops abruptly at this point and does not elaborate further.

The critical remarks by Henle, Chapman, and Pfeffer are to be taken most seriously, all the more so since it is doubtful whether they are adequately met in Kallen's reprise. Henle, as a Catholic spokesman, fears that the American Idea may entail the seeds of a secularistic, even materialistic, semitotalitarianism; granted, as Kallen asserts, that this is not at all the American Idea, it must at least be admitted that it is a real and ever present danger. Indeed, Chapman and Pfeffer, one as a sociologist and the other as a constitutional lawyer, do just that. They do not doubt that the motto *e pluribus unum* contains the core of Americanism, but they question whether the Declaration of Independence (or Linton's concept of participation in culture) was meant to extend the guaranty of liberty from the individual to the group; in other words, they question whether the American Idea embraces cultural pluralism. Really, has American reality not corroded, or nearly corroded, all subcultures brought in contact with it? Has it not tended to atomize the social structure? Have mass production and mass communication not enhanced the threat of uniformity? In reply, Kallen points to the multiplicity of associational life in America, but thereby the very ground on which he stands is shifted and the original meaning of cultural pluralism is lost. One must hope that the discussion will be carried on.

WERNER J. CAHNMAN

Hunter College

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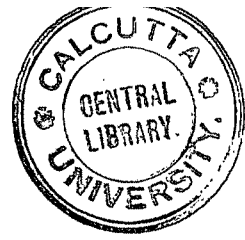
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THE AMERICAN JOURNAL OF SOCIOLOGY

Volume LXIII

MARCH 1958

Number 5

FROM ORGANIZATION TO SOCIETY: VIRGINIA IN THE SEVENTEENTH CENTURY

SIGMUND DIAMOND

ABSTRACT

Virginia in the early seventeenth century may be taken as an example of a social system established in accordance with the model of a commercial organization, in which the behavior of the members was expected to be entirely determined by their positions within the organization. The concessions that were offered to induce them to accept positions within the organization so multiplied the number of statuses they held as to alter decisively their behavior and to transform Virginia from a formal organization into a society. The analysis of the early history of Virginia is an illustration of the way in which historical knowledge may be used to suggest problems of interest to sociological theory.

Fad and fashion play their roles in the world of scholarship as elsewhere, and often products of the intellect may assume the quaint air of artifacts for no better reason than that, with the passage of time, they are made obsolete by the appearance of new, if not necessarily better, models. But in scholarship, if not in manufacturing, novelty is a virtue that has limits; and even old ideas and interests may be resurrected if they demonstrate the existence of problems or give promise of solving problems for which more recent ideas have proved inadequate. So it is that historical sociology, though conceded to be one of the roots from which the discipline itself emerged, has, in this country at least, suffered from the competition of more stylish fashions. And so it is, too, that there is increasing evidence today that historical sociology, so long an outmoded form of inquiry, is once again commending itself as an important subject of research. What follows is, frankly, an attempt to aid in the rehabilitation of historical sociology, not

by exhortation, but, it is hoped, by a persuasive demonstration that questions of considerable importance for sociological theory may be raised when problems are examined in historical perspective. Our interest in this essay is in the utilization of certain aspects of the history of Virginia in the early seventeenth century to suggest significant questions concerning the creation of new statuses and the circumstances under which the character of an organization may be so altered as to be transmuted into something which is not, properly speaking, an organization at all but a society.

I

It must be conceded at the outset that the group we have selected for study was pathetically small. In 1607, when the Virginia Company established a settlement at Jamestown, its population numbered 105; and in 1624, when the crown revoked the charter of the Company, the population of Virginia amounted to just over 1,200, de-

spite the fact that the Company had sent more than 5,000 emigrants during that seventeen-year period.¹ But, just as a limited duration of time is no necessary detriment to a study of this kind, because there are periods of history when the rate of change is accelerated, so, too, the limited size of the group affords no accurate measure of the importance of the enterprise. Judged in terms of its outcome, its importance is self-evident. But, judged even in terms of the criteria of importance imposed by contemporaries, the verdict must be the same. The articles on the Virginia settlement in the *Kölnische Zeitung* and the *Mercure françoise*; the running series of reports from the Venetian ambassadors in London to the Doge and Senate; the letters from Jesuit priests in England to the Propaganda Fide in Rome and the newsletters from Venice and Antwerp in the Vatican archives; the continuing stream of dispatches from the Spanish ambassadors to King Philip III, pressing him to attack Jamestown, advising him of the latest decisions of the Virginia Company, and relating their efforts to recruit English spies; and the existence in the royal archives at Simancas of a description of the layout of Jamestown and the earliest known map of the town, the work of an Irish spy in the service of Spain²—all this is eloquent testi-

mony of the position of Virginia in the international relations of the seventeenth century and of the concern felt in the capitals of Europe in the Virginia Company's undertaking. Nor was the expression of this concern merely verbal. In August, 1613, when the population of Virginia barely exceeded 200, the settlement at Jamestown had a decidedly cosmopolitan cast, for it contained eighteen prisoners—fifteen Frenchmen, including two Jesuits and several members of the nobility; a Spanish spy, Don Diego de Molina; a renegade Englishman in the pay of Spain; and an Indian princess, Pocahontas.³

At the May Day, 1699, exercises at the College of William and Mary, one of the student orators—who must have been a sophomore—exclaimed:

Methinks we see already that happy time when we shall surpass the Asiaticians in civility, the Jews in religion, the Greeks in philosophy, the Egyptians in geometry, the Phoenicians in arithmetic, and the Chaldeans in astrology. O happy Virginia.⁴

We may be intrigued by the ingenuousness of the student, but we are interested in the statement as evidence of the fact that in 1699—and for some time earlier—Virginia was a society and Virginians were nothing if not ebullient about its prospects. For it had not always been so.

At its inception—and for a number of years thereafter—it had been a formal organization, and, if the joyous outburst of the student reflects its character at a later date, its earlier character is better revealed by the instructions given by the Virginia Company to Sir Thomas Gates on the eve of his departure for Jamestown in May, 1609:

You must devide yo^r people into tennes twenties & so upwards, to every necessary worke a competent number, over every one of w^{ch} you must appointe some man of Care & still in that worke to oversee them and to take dayly ac-

¹ Philip Alexander Bruce, *Social Life of Virginia in the Seventeenth Century* (Richmond, 1907), pp. 15, 17-18; "The Virginia Census, 1624-25," *Virginia Magazine of History and Biography*, VII (1899-1900), 364-67; Edward Channing, *A History of the United States* (New York and London, 1905-25), I, 204-5.

² See, e.g., Alexander Brown, *The Genesis of the United States* (Boston and New York, 1897), I, 142, 180, n. 1, 244-45, 393-99; II, 595-96, 738, 741; *Calendar of State Papers and Manuscripts Relating to English Affairs . . . in the Archives and Collections of Venice . . .*, Vol. XI, Nos. 52, 466, 794, 821; Carl Russell Fish (ed.), *Guide to the Materials for American History in Roman and Other Italian Archives* (Washington, 1911), pp. 150 ff.; Henry Chandlee Forman, *Jamestown and St. Mary's* (Baltimore, 1938), pp. 37, 38; Alexander Brown, *The First Republic in America* (New York and Boston, 1898), pp. 48, 50, 51-52, 62, 79-80, 121, 123, 125, 152, 160, 184-85, 218-19.

³ Brown, *Genesis*, II, 700-706.

⁴ Quoted in Louis B. Wright, *The First Gentlemen of Virginia* (San Marino, 1940), p. 109.

counte of their laboures, and you must ordayne y^t every overseer of such a number of worke-men Deliver once a weeke an accounte of the wholle comitted to his Charge . . . you shall doe best to lett them eate together at reasonable howers in some publike place beinge messed by six or five to a fnesse, in w^{ch} you must see there bee equality and sufficient that so they may come and retourne to their worke without any delay and have no cause to complain of measure or to excuse their idleness uppon y^e dressinge or want of diet. You may well allowe them three howers in a somers day and two in the winter, and shall call them together by Ringinge of a Bell and by the same warne them againe to worke.⁵

And, if in later years "O happy Virginia" could be a spontaneous outcry of its citizens, it could not have been earlier. Testifying in 1625 about conditions under the administration of Sir Thomas Dale in 1614–16, Mrs. Perry, one of the fortunates who survived more than a few years in the first quarter-century of Virginia's history, revealed that

in the time of Sr: Thomas Dales Government An leyden and June Wright and other women were appoynted to make shirts for the Colony servants and had six nelds full of silke threed allowed for making of a shirte, w^{ch} yf they did not p'forme, They had noe allowance of Dyott, and because theire threed naught and would not sewe, they tooke owt a ravell of y^e lower p^{te} of y^e shirte to make an end of y^e worke, and others y^t had threed of thiere owne made it up wth that, Soe the shirts of those w^{ch} had raveled owt proved shorter then the next, for w^{ch} fact the said An leyden and June Wright were whipt, And An leyden beinge then wth childe (the same night thereof miscarried).⁶

Our first inquiry, then, must be into the characteristics of the original settlement at Jamestown—characteristics which changed so markedly during the course of the next quarter-century.

Virginia was not established as a colony to take its place among the territories gov-

erned by the British crown; it was not a state, and, properly speaking, it was not a political unit at all. It was property, the property of the Virginia Company of London, and it was established to return a profit to the stockholders of that company. Under the political and economic conditions of seventeenth-century England, speculators in overseas expansion could count on no support from the government except verbal encouragement and some legal protection—and sometimes precious little of these. Under the circumstances, therefore, colonization had to be undertaken as a private business venture, and the first charge imposed on the property was the return on the shareholder's investment. Traditionally, this episode has been dealt with primarily in terms of the motivation of participants—did they come to establish religious freedom, to seek a haven for the politically persecuted, or to found a "First Republic"?—and it is true that those who joined the Virginia enterprise did so for many reasons. Some, like Richard Norwood, were foot-loose and fancy-free after having completed their apprenticeships. Robert Evelin wrote his mother that he was "going to the sea, a long and dangerous voyage with other men, to make me to be able to pay my debts, and to restore my decayed estate again . . . and I beseech you, if I do die, that you would be good unto my poor wife and children, which God knows, I shall leave very poor and very mean, if my friends be not good unto them." In its promotional literature the Virginia Company took advantage of this broad spectrum of motives and cast its net wide to snare the purses and bodies of all sorts and conditions of persons in support of a venture in which

. . . profite doth with pleasure joyne,
and bids each chearefull heart,
To this high prayسد enterprise,
performe a Christian part.⁷

⁵ Susan Myra Kingsbury (ed.), *Records of the Virginia Company* (Washington, 1906–35), III, 21.

⁶ "Minutes of the Council and General Court," *Virginia Magazine of History and Biography*, XXIII (1915), 138.

⁷ Wesley Frank Craven and Walter B. Hayward, *The Journal of Richard Norwood, Surveyor of Bermuda* (New York, 1945); Brown, *Genesis*, I, 442; "London's Lotterie," *William and Mary Quarterly*, V (3d ser., 1948), 259–64.

But, from the point of view of the managers of the enterprise, recruitment was perceived less as a problem of motivation than of achieving an organizational form through which the resources and energies of the participants could be mobilized. The basic objectives of the promoters in establishing a plantation in Virginia are quite clear: to exploit the mineral resources which they were certain were there; to search for that elusive will-o'-the-wisp—a water route to the Pacific through North America—and to monopolize whatever local trade existed and whatever oriental trade would be developed with the opening-up of the northwest passage.

The organizational form adopted for the venture was not created by the promoters; the roots of the joint-stock company, though it was still subject to considerable experimentation, lay deeply imbedded in English history. Nor were the proprietors themselves totally without experience in the establishment of plantations or unaware of the experience of others. Sir Thomas Smythe, a leader of the Virginia enterprise, was one of the merchant princes of London, a governor of the East India Company, the Muscovy Company, and many others. And they had before them the experience—which was, as we shall see, not entirely an unmixed blessing—of the colonizing efforts of Sir Walter Raleigh and Sir Humphrey Gilbert, of the trading posts established by the great commercial companies, of Spain and Portugal, and of the founding of plantations in Ireland.⁸

What they established was a business organization; and, though the form of that organization was changed at various times during the Company's history, those changes were at all times dictated by the need to make the business pay, which, in the words of Sir Edwin Sandys, one of the

two great leaders of the Company, was "that whereon all men's eyes were fixed."⁹ Its problems were those of any business organization. It sold shares, begged contributions, and organized lotteries to raise the necessary funds; it was concerned to recruit a proper labor force; it had to cope with the problem of adequate supervision and administration so as to maintain its authority; and it engaged in a full-scale advertising campaign to sell to potential adventurers and planters the glories of a land where the "horses are also more beautiful, and fuller of courage. And such is the extraordinarie fertility of that Soyle, that the Does of their Deere yeelde Two Fawnes at a birth, and sometimes three." And it was confronted with the petty harassments of cajoling those whose good will was needed for the success of the organization. "Talking with the King," wrote the Earl of Southampton to Sir Robert Cecil, "by chance I told him of the Virginia Squirrills which they say will fly, whereof there are now divers brought into England, and hee presently and very earnestly asked me if none of them was provided for him. . . . I would not have troubled you with this but that you know so well how he is affected by these toys."¹⁰

But though the Company's plans were eminently rational, its grand design suffered from a fatal flaw: reality was far different from what the Company expected. Its model had been the East India Company, and its dream had been to reproduce the Spanish looting of a continent; but conditions in Virginia were not those of India or Mexico and Peru. "It was the Spaniards

⁸ Wesley Frank Craven, *Dissolution of the Virginia Company* (New York, 1932), p. 24. For an account of the structure of the Company see William Robert Scott, *The Constitution and Finance of English, Scottish and Irish Joint-Stock Companies to 1720* (Cambridge, 1910), II, 247-59, 266-88.

¹⁰ *A Declaration of the State of the Colonie and Affairs in Virginia* (London, 1620), in Peter Force (ed.), *Tracts and Other Papers, Relating . . . to the . . . Colonies in North America* (Washington, 1836-46), III, 5; Brown, *Genesis*, I, 357.

⁸ Herbert Levi Osgood, *The American Colonies in the Seventeenth Century* (New York and London, 1904, 1907), I, 32-34; II, 30-32; Philip Alexander Bruce, *Economic History of Virginia in the Seventeenth Century* (New York and London, 1896), I, 3-4.

good hap," wrote Captain John Smith later in the history of the Virginia Company,

to happen in those parts where were infinite numbers of people, whoe had manured the ground with that providence that it afforded victuall at all times; and time had brought them to that perfection they had the use of gold and silver, and the most of such commodities as their countries afforded; so that what the Spaniard got was only the spoile and pillage of those countries people, and not the labours of their owne hands. But had those fruitfull Countries been as Salvage, as barbarous, as ill-peopled, as little planted laboured and manured, as Virginia; their proper labours, it is likely would have produced as small profit as ours. . . .

But we chanced in a land, even as God made it. . . . Which ere wee could bring to recompence our paines, defray our charges, and satisfie our adventurers; wee were to discover the country, subdue the people, bring them to be tractable civil and industrious, and teach them trades that the fruits of their labours might make us recompence, or plant such colonies of our owne that must first make provision how to live of themselves ere they can bring to perfection the commodities of the countrie.¹¹

But though the error in conception made by the leaders of the Virginia Company was, from their viewpoint, a grievous one, it is also thoroughly understandable. It is true that the late sixteenth and early seventeenth century was a period of rapid expansion in the organization of trading companies; no less than thirty-four were chartered during that time. But the significant point is that the Virginia Company was the eighteenth to be founded, and, of the previous seventeen, whose experience could be taken as models, all dealt with countries within the European seas, with settled communities along the African coast, or with the advanced societies of Asia. For them, the problem was to exploit the already existing labor force of a settled society.¹² For the Virginia Company, the problem—and it is in this that the crucial

difference lies—was to recruit a labor force.

It must be understood, therefore, that, in conformity with its objectives and organizational form, the establishment planted by the Virginia Company at Jamestown was a private estate, which, in the absence of an amenable local labor force, was worked on the basis of imported labor. Basic policies were laid down in London by the General Court of the Company, the body of those who had purchased the £12 10s. shares or who had been admitted for favors in the Company's behalf; the management and direction of affairs were intrusted to agents of the shareholders; and the supervision of those whose labor in Virginia was necessary for the attainment of the Company's objectives was placed in the hands of officials appointed in London.

Under the circumstances there were many potent inducements to English investors to purchase the Company's £12 10s. shares, a price, incidentally, which was the Company's estimate of the cost of transporting a settler to Virginia. Under the charter of 1606 they were guaranteed that after a five-year period, during which the settlers in Virginia would be supported by a stream of supplies sent at Company expense, the profits gained through trade and the discovery of minerals would be divided among the investors in proportion to the number of shares they held, and grants of land would be made to them on the same basis. But what were to be the inducements to become the labor force of a company trading post?

It should be noted at once that the English imitated the Spaniards in attempting to mobilize native labor. For the Company the key to the integration of the Indians into the labor force was in the ease with which, it was anticipated, they could be

¹¹ John Smith, *Description of Virginia and Proceedings of the Colonie* (Oxford, 1612), in Lyon Gardiner Tyler (ed.), *Narratives of Early Virginia* (New York, 1907), p. 178.

¹² Susan Myra Kingsbury, "A Comparison of the Virginia Company with the Other English Trading Companies of the Sixteenth and Seventeenth Centuries," *Annual Report of the American Historical Association for the Year 1906* (Washington, 1907), pp. 162-63.

converted to Christianity and thereby won over as well to the secular values of Europeans. To them would accrue spiritual benefits; the Company, already blessed with those, would receive something more substantial. As a certain "Maister Captaine Chester" put it:

The land full rich, the people easilie wonne,
Whose gaines shalbe the knowledge of our faith
And ours such ritches as the country hath.¹³

But though the Company succeeded for a time in exacting some tribute from the local tribal chiefs in the form of goods and weekly labor services, the Indians proved unwilling to accept the Company's spiritual and secular offerings. Long before the Indian uprising of 1622 gave an excuse to the settlers to engage in a campaign of extermination, it was clear that the Virginia Company would be forced to import its own labor force.¹⁴

Between 1607 and 1609, when its charter was changed, the Virginia Company sent over 300 persons to Jamestown. They were a disparate crew of adventurers and rough-necks, imbued with the hope that after a short period in Virginia they would return home with their fortunes in their purses. The social composition of the original labor force, the tasks they were expected to perform, and the nature of the settlement they were expected to establish can all be inferred from the passenger lists of the first expedition and the three subsequent supplies that were sent out by the Company before its charter was modified in 1609. The original expedition numbered 105 persons, of whom we have the names of 67. Of these 67, 29 were listed as gentlemen and 6 were named to the local council; the rest were listed by occupation—1 preacher, 4 carpenters, 12 laborers, 1 surgeon, 1 black-

smith, 1 sailor, 1 barber, 2 bricklayers, 1 mason, 1 tailor, 1 drummer, and 4 boys—and 2 were unidentified. In the three succeeding supplies, the rather high proportion of gentlemen was not substantially reduced, nor did the range of occupations alter significantly. Seventy-three of the 120 persons in the first supply of 1608 can be identified. In this group, gentlemen exceeded laborers 28 to 21. The remainder was made up of an odd assortment of craftsmen, including jewelers, refiners, and goldsmiths—bespeaking the expectations of the Company—apothecaries, tailors, blacksmiths, and—mute testimony to the fact that gentlemen must be gentlemen whether in the wilds of Virginia or a London drawing room—a perfumer. In brief, the two most striking characteristics of this original labor force are the presence of so high a proportion of gentlemen and the absence of any occupations indicative of an intention to establish a settled agricultural community.¹⁵

From the point of view of the promoters of the Virginia enterprise, these men were not citizens of a colony; they were the occupants of a status in—to use an anachronistic term—the Company's table of organization, and the status was that of workman. Such other qualities or attributes that they possessed might have been of importance when they were in London, Norwich, or Bristol, but what counted in Virginia was that they should accept the directions of their superiors and that they should be willing to work.

Even under the best of circumstances, the problem of maintaining discipline and authority would have been crucial to the success of the Company. But these were hardly the best of circumstances, for the very social composition of the original labor force intensified what in any case would

¹³ Quoted in Keith Glenn, "Captain John Smith and the Indians," *Virginia Magazine of History and Biography*, LII (1944), 231, n. 12.

¹⁴ Wesley Frank Craven, "Indian Policy in Early Virginia," *William and Mary Quarterly*, I (3d ser., 1944), 65-82.

¹⁵ John Smith, *Description of Virginia*, in Tyler (ed.), *op. cit.*, pp. 125-26, 140-41, 159-60; Thomas Jefferson Wertenbaker, *Patrician and Plebeian in Virginia* (Charlottesville, 1910), pp. 5-9; Bruce, *Social Life*, pp. 39-43.

have been a grievously difficult problem. In the long intervals between the arrival of supplies under the direction of the Company's admiral, Christopher Newport, conditions in Jamestown bordered on anarchy; men were beaten by their officers, plots were hatched to escape the country, and insubordination was rampant. The Company's administrative methods, characterized by the utmost laxness, could not cope with the situation. "I likewise as occasion moved me," wrote President Wingfield, discussing the supplies in Virginia, "spent them in trade or by gift amongst the Indians. So likewise did Captain Newport take of them . . . what he thought good, without any noate of his hand mentioning the certainty; and disposed of them as was fitt for him. Of these likewise I could make no accompt." Nor did the high percentage of aristocrats help matters. Unused to the heavy work of axing timber, they cursed so much at their blisters that the president of the council ordered that at the end of the day's work a can of cold water be poured down the sleeve of each offender for every curse he had uttered. To Captain John Smith, the problem was the presence of too many gentlemen: "For some small number of adventrous Gentlemen . . . nothing were more requisite; but to have more to wait and play than worke, or more commanders and officers than industrious labourers was not so necessarie. For in Virginia, a plaine Souldier that can use a Pickaxe and spade, is better than five Knights."¹⁶

Clearly, even if the mortality figures had been less gruesome than they were—in July, 1609, between 80 and 100 were alive of the 320 who had been sent since 1607¹⁷

¹⁶ The quotations are in Osgood, *op. cit.*, I, 46-47; Smith, *Generall Historie*, in Tyler (ed.), *op. cit.*, pp. 331-32; John Smith, *The Proceedings of the English Colonie in Virginia* (Oxford, 1612), in the A. G. Bradley edition of Edward Arber (ed.), *Travels and Works of Captain John Smith* (Edinburgh, 1919), I, 149. See also Osgood, *op. cit.*, I, 50, 54-55; Bruce, *Economic History*, I, 197.

¹⁷ Channing, *op. cit.*, I, 204.

—qualitative considerations alone would have dictated a change in the composition of the labor force. For the Company the situation was brought to a head with the realization that there were to be no quick returns from metals and trade and that profits would have to be made through the exploitation of agricultural resources.

Never did the Company rely fundamentally on the recruitment of involuntary labor, but so desperate were its labor requirements and so necessary was it to keep the good will of those authorities who favored the transportation of undesirables that it felt compelled to resort to forced labor.

As early as 1609, a letter from Lisbon revealed that the Portuguese were transporting fifteen hundred children over the age of ten to the East Indies and suggested that the same be done in the case of Virginia. Shortly thereafter the Privy Council notified the mayor of London that the plagues of the city were due mainly to the presence of so many poor persons and recommended that a fund be raised, with the help of the commercial companies, to send as many of these as possible to Virginia. The Virginia Company promptly gave an estimate of the expenses involved and of the terms that would be offered to the emigrants; but, though a large sum of money was raised, no persons were actually transported at that time. In 1617, however, the City of London raised £500 to pay the cost of shipping one hundred children to Virginia, where they were to be apprenticed until the age of twenty-one, thereafter to be the fee-simple owners of fifty acres of land each. So delighted were the Company and the Virginia planters that they continued the practice, but it is evident that not all the children were equally pleased by the future arranged for them. In January, 1620, Sandys wrote to Sir Robert Naunton, the king's principal secretary, that "it falleth out that among those children, sundry being ill-disposed, and fitter for any remote place than for this Citie, declare their unwillingness to goe to Virginia: of whom

the Citie is especially desirous to be disburdened; and in Virginia under severe Masters they may be brought to goodness." Since the City could not deliver and the Company could not transport "theis persons against their wills," Sandys appealed to the Privy Council for the necessary authority. It was quickly given. Exact figures cannot be determined, but, before the demise of the Company in 1624, additional shipments of children had been delivered to Virginia, and it is evident that several hundred must have been involved.¹⁸

Concerning the shipment of convicts and rogues and vagabonds the information is scanty. Some convicts were certainly in Virginia before 1624, though we do not know how many; but the Virginia Company was antagonistic to the importation of such persons, and, in any case, convict-dumping on a large scale did not become a characteristic of the colonial scene until the second half of the seventeenth century.¹⁹ So, too, was the Company antagonistic to the importation of rogues, possibly because, unlike the case of the London children, it was forced to assume the cost of transportation. It engaged in the practice under pressure from King James I. For one group of fifty boys sent out in 1619, the Company expected to receive £500 in tobacco from the planters to whom they were indentured; but as late as October, 1622, it had received only £275.15.6, and Governor Yeardley was told that the planters "should be caused to make satisfaccon for the 224¹¹4:6:w^{ch} is remayninge due unto the Companie this yeare in good leafe Tobac-

co." That still others were sent is certain; the Court Book of Bridewell Hospital records that in 1620 Ellen Boulter was "brought in by the Marshall for a Vagrant, that will not be ruled by her father or her friends," to be kept at her father's charges to go to Virginia.²⁰

But throughout its history the Company was dependent upon the recruitment of voluntary labor, and especially was this true when it realized that profits would have to be made from agricultural staples and not minerals. The change in objective not only emphasized the necessity of recruiting a larger labor supply but required that it be qualitatively different from the earlier one, for now that the glitter of gold was vanishing the Company needed not soldiers of fortune but sober workmen who would be able to extract from the land the food supplies necessary for their own support and the staples whose export would produce profit for the shareholders.²¹ But what could the Company offer as sufficient inducement to motivate large numbers of persons to come to Virginia, especially when—as the evidence indicates—enthusiasm for emigration from England was confined to the wealthy, who themselves were hardly likely to exchange the comforts of life in England for the dangers of life in Virginia?²² The difficulties the Company faced in this respect were exacerbated by the whispering campaign started by settlers who had already returned from Virginia. "Some few of those unruly youths sent thither," said a Virginia Company broadside in 1609,

(being of most leaued and bad condition) and such as no ground can hold for want of good direction there, were suffered by stealth to get

¹⁸ *Calendar of State Papers, East Indies, 1571-1616*, No. 432; Brown, *Genesis*, I, 252-54; E. Ribton-Turner, *A History of Vagrants and Vagrancy* (London, 1887), 141; Kingsbury (ed.), *Records*, I, 304-6, 270, 359; III, 259; *Acts of the Privy Council of England, Colonial Series*, Vol. I, No. 42; Abbot Emerson Smith, *Colonists in Bondage* (Chapel Hill, 1947), pp. 147-49; Richard B. Morris, *Government and Labor in Early America* (New York, 1946), p. 385.

¹⁹ A. E. Smith, *op. cit.*, pp. 94-95; Morris, *op. cit.*, p. 323.

²⁰ Kingsbury (ed.), *Records*, I, 520, II, 108; A. E. Smith, *op. cit.*, pp. 139-40.

²¹ Craven, *Virginia Company*, pp. 29-33; Scott, *op. cit.*, II, 250-52; Philip Alexander Bruce, *Institutional History of Virginia in the Seventeenth Century* (New York and London, 1910), II, 237-41.

²² A. E. Smith, *op. cit.*, pp. 44-46.

aboard the ships returning thence, and are come for England againe, giving out in all places where they come (to colour their owne misbehaviours, and the cause of their returne with some pretence) most vile and scandalous reports, both of the Country itselfe, and of the Cariage of the business there.²³

The Company now determined to be discriminating in the selection of settlers:

And for that former experience hath too clearly taught, how much and manie waies it hurtheth to suffer Parents to disburden themselves of lascivious sonnes, masters of bad servants and wives of ill husbands, and so to dogge the business with such an idle crue, as did thrust themselves in the last voiage, that will rather starve for hunger, than lay their hands to labor.²⁴

It was conceded that some "base and disordered men" might inveigle themselves into the body of settlers, but they could not do too much harm, for, as the Reverend William Crashaw said on the departure of Governor de la Warr to Virginia, "the basest and worst men trained up in a severe discipline, sharp lawes, a hard life, and much labour, do prove good members of a Commonwealth. . . . The very excrements, of a full and swelling state . . . wanting pleasures, and subject to some pinching miseries," will become "good and worthe instruments."²⁵

Clearly, if prospective settlers in Virginia faced "severe discipline, sharp lawes, a hard life, and much labour," substantial concessions would have to be offered to induce them to emigrate. The status the Company was asking them to accept was that of servant, employee of the Company, but it was one thing to create a position and quite another to get men to fill it. Since

²³ Brown, *Genesis*, I, 355.

²⁴ *Ibid.*, I, 356.

²⁵ A *Sermon Preached in London before the Right Honourable Lord la warre, Lord governor and Captaine Generall of Virginia* (London, 1610), quoted in Perry Miller, "Religion and Society in the Early Literature: The Religious Impulse in the Founding of Virginia," *William and Mary Quarterly*, VI (3d ed., 1949), 31; Brown, *Genesis*, I, 364.

perpetual servitude was obviously no inducement, the Company was required to limit the period of service and to make other concessions. Every settler over the age of ten, whether he paid his own way or was shipped at Company expense, was promised one share of stock in the Company, with potential dividends from the profits of trade and a land grant to be made at the time of the first division after seven years. Every "extraordinaire" man—such as "Divines, Governors, Ministers of State and Justice, Knights, Gentlemen, Physitions" or such as were "of worth for special services"—was given additional shares according to the value of his person. The Company expected, in return for assuming all the costs of maintaining the plantation and providing supplies to the emigrants, that each settler would work at tasks assigned him under the direction of Company-appointed officers. For a period of seven years, all supplies were to be distributed through the Company store, all exports were to be shipped through the Company magazine, and all land was to be held by the Company.²⁶ In effect, the Company created the status of landowner in order to induce persons to accept the status of non-landowner; it was asking emigrants to accept the present burdens of membership in a lower status in anticipation of the future benefits they would receive upon promotion to a higher status. From the point of view of the structure of an organization, this was simply automatic progression—promotion to a higher position in the table of organization after a limited tenure in a lower position. From the point of view of a society, however, this was a guaranty of social mobility, and, as we shall see, it seriously compromised the Company's abil-

²⁶ James Curtis Ballagh, *White Servitude in the Colony of Virginia* ("Johns Hopkins University Studies in Historical and Political Science, 13th Series," Vols. VII-VIII [Baltimore, 1895]), pp. 15-17; Craven, *Virginia Company*, pp. 29-33; Craven, *Southern Colonies*, pp. 85-90; A. E. Smith, *op. cit.*, pp. 8-10; Kingsbury, "Comparison," *op. cit.*, pp. 163-69.

ity to secure its organizational objectives.

That the Company expected the combination of limited servitude and potential landownership to solve its labor problem is quite clear; sufficient numbers of workmen would be induced to emigrate to Virginia and, having arrived, would be motivated to do the work that was essential to the Company's success. Virginia planter and London adventurer were to be united in a single relationship. Do not discourage the planters, the London stockholders were admonished, "in growing religious, nor in gathering riches, two especial bonds (whether severed or cojoined) to keepe them in obedience, the one for conscience sake, the other for fear of losing what they have gotten." How the planter's concern for his own interests was to benefit the Company was quite clear. "The Planters," wrote Alderman Johnson, "will be in such hope to have their owne shares and habitations in those lands, which they have so husbanded, that it will cause contending and emulation among them, which shall bring foorth the most profitable and beneficiall fruites for their ioynt stock."²⁷

But land for the settlers and profits for the stockholders were affairs of the future, and both were dependent upon the skill and speed with which the planters could be molded into an efficient labor force. It was of the utmost importance, therefore, that the Company establish its authority in Virginia and maintain discipline, and for the achievement of these purposes the Company was not content to rely simply on the self-discipline it hoped would be the by-product of the effort to obtain profits. The first step was taken with the issuance of the new charter of 1609. During its first three years in Virginia, the Company felt, "experience of error in the equality of Governors, and some out-rages, and follies committed by them, had a little shaken so tender a body." To avoid the evils of divided

authority, "we did resolve and obtain, to renew our Letters Pattents, and to procure to ourselves, such ample and large privileges and powers by which we were at liberty to reforme and correct those already discovered, and to prevent such as in the future might threaten us . . . under the conduct of one able and absolute Governor."²⁸ But changes in the formal structure of authority were not sufficient.

Religion, too, was counted upon to do its part in maintaining order. Doctrinal conflict was minimized from the start by the ban on Catholics, but what really distinguishes the role of religion under the Virginia Company was its conscious utilization for disciplinary purposes. No less an authority on colonization than Richard Hakluyt had pointed to the advisability of taking along "one or two preachers that God may be honoured, the people instructed, mutinies better avoided, and obedience the better used."²⁹ The Company was quick to take the hint. Religion was used to screen prospective planters before their arrival in Virginia, and it was used to discipline them after their arrival. "We have thought it convenient to pronounce," stated the Company in a broadside of 1609, "that . . . we will receive no man that cannot bring or render some good testimony of his religion to God."³⁰ And during the time that Sir Thomas Dale's code of laws was sovereign in Virginia—from May, 1610, to April, 1619—the settlers were marched to church twice each day to pray for relief from dissension and for the showering of blessings upon the shareholders:

O Lord . . . defend us from the delusion of the devil, the malice of the heathen, the invasions of our enemies, & mutinies & dissensions of our own people. . . . Thou has moved . . . the hearts of so many of our nation to assist . . . with meanes and provision, and with their holy praiers . . . and for that portion of their sub-

²⁷ *The New Life of Virginea . . . Being the Second Part of Nova Britannia* (London, 1612), in Force (ed.), *op. cit.*, I, 17-18; *Nova Britannia*, in Force (ed.), *op. cit.*, I, 26.

²⁸ *A True and Sincere Declaration* (London, 1609), in Brown, *Genesis*, I, 352.

²⁹ Quoted in Craven, *Southern Colonies*, p. 64.

³⁰ Appendix to *A True and Sincere Declaration*, in Brown, *Genesis*, I, 352.

stance which they willingly offer for thy honour & service in this action, recompence it to them and theirs, and reward it seven fold into their bosomes, with better blessings.³¹

In a society of ranks and orders, deference is owed to certain persons by virtue of their social position, and the Company attempted to maximize the potentiality for discipline in such an arrangement by appointing to leading posts in Virginia those persons to whom obedience was due because of their high status. Insofar as it was possible, the Company selected only persons of high birth to be governor; when it was not possible, as in the case of Governor Yeardley, it quickly, and it seems surreptitiously, secured for him a knighthood.³² And at all times the governors were urged to surround themselves with the pomp and circumstance of high office, the better to impress the governed. "You shall for the more regard and respect of yo^r place," read the Company's instructions to Sir Thomas Gates,

to beget reverence to yo^r authority, and to refresh their mindes that obey the gravity of those lawes under we^{ch} they were borne at yo^r discrecon use such formes and Ensignes of government as by our letters Pattents wee are enabled to grant unto you, as also the attendance of a garde uppon your pson.³³

Ultimately, however, the Company relied upon a military regimen and upon the imposition of force to obtain labor discipline. Governor de la Warr had been instructed that his men were to be divided into groups and placed under the charge of officers "to be exercised and trayned up in Martiall manner and warlike Discipline."³⁴ Settlers were forbidden to return to England without permission, and their letters were sealed and sent first to the Company in London before being forwarded.³⁵ But

³¹ *For the Colony in Virginea Britannia, Lawes Divine, Morall and Martiall, &c* (London, 1612), in Force (ed.), *op. cit.*, III, 68.

³² Kingsbury (ed.), *Records*, III, 216-19.

³³ *Ibid.*, p. 15.

³⁴ *Ibid.*, p. 27.

³⁵ *Ibid.*, p. 22.

the full code of military discipline was not worked out until the arrival in Jamestown of Captain Thomas Dale, marshal of the colony, who had been granted a leave of absence from his post in the Netherlands army at the behest of the Company. Dale supplemented the usual list of religious offenses and crimes against the state and the person with a series of enactments designed to protect the Company's interests. Slander against the Company, its officers, or any of its publications; unauthorized trading with the Indians; escaping to the Indians; theft; the killing of any domestic animal without consent; false accounting by any keeper of supplies—all were punishable by service in the galleys or death. Failure to keep regular hours of work subjected the offender to the pain of being forced to lie neck and heels together all night for the first offense, whipping for the second, and one year's service in the galleys for the third.³⁶

Moreover, Dale created a military rank for every person in Virginia and specified the duties of each in such a way as to provide us with important clues into the nature of labor discipline and what was expected to provide the motivation to work.

Because we are not onely to exercise the duty of a Souldier, but that of the husbandman, and that in time of the vacancie of our watch and ward wee are not to live idly, therefore the Capitaine . . . shall . . . demand . . . what service, worke, and businesse he hath in charge, from the Governor . . . in which worke the Capitaine himselfe shall do exceeding worthily to take paines and labour, that his Souldiers seeing his industry and carefulnesse, may with more cheerfulness love him, and bee encouraged to the performance of the like.

Of the corporal:

His duty is to provide that none of his Squadron, be absent, when the drumme shall call to any labour, or worke, or at what time soever they shall be commanded thereunto for the

³⁶ For the full text of the code see *For the Colony in Virginea Britannia. Lawes Divine, Morall and Martiall &c* (London, 1612), in Force (ed.), *op. cit.*, Vol. III.

service of the Collonie, in the performance of which said workes he is to be an example of the rest of his Squadron by his owne labouring therein . . . that thereby giving incorageing to his superior officers he may be held by them worthy of a higher place.

Of the private soldier:

He shall continue at his worke until the drumme beat, and . . . be conducted into the church to heare divine service, after which he may repayre to his house or lodging to prepare for his dinner, and to repose him until the drumme beate shall call him forth againe in the afternoone . . . the Generall having understanding of his promptitude and dilligence may conferre upon him, and call him into place of preferment and commaund.³⁷

What is so striking about Dale's Code is the way in which it stripped from people all attributes save the one that really counted in the relationship which the Company sought to impose on them—their status in the organization. Behavior was expected to conform to a set of prescriptions the major characteristic of which was that the rights and obligations of persons depended on their position within the organization. In this respect, the contrast between Dale's Code and the first set of laws the settlers were able to enact for themselves at the General Assembly of 1619 is startling. For then, considerations other than status within an organization were fundamental:

All persons whatsoever upon the Sabaoth days shall frequente divine service and sermons both forenoon and afternoone. . . . And every one that shall transgresse this lawe shall forfeite three shillings a time to the use of the churche. . . . But if a servant in this case shall wilfully neglecte his Mr's commande he shall suffer bodily punishment.

Or consider the following petition drafted by the Assembly:

. . . that the antient Planters . . . suche as before Sir T. Dales' depart were come hither . . . maye have their second, third and more divisions suc-

cessively in as lardge and free manner as any other Planter. Also that they wilbe pleased to allowe to the male children, of them and of all others begotten in Virginia, being the onely hope of a posterity, a single share a piece.³⁸

For the planters in Virginia, considerations of length of residence and of varying degrees of freedom now affected the rights and obligations of persons. No longer could relations be determined exclusively by the positions persons held within a single system—the organization of the Company. By 1619 Virginia was becoming a society, in which behavior was in some way determined by the totality of positions each person held in a network of sometimes complementary, sometimes contradictory, relationships. The key to this transformation from organization to society lies in the concessions the Company was forced to offer to induce persons to accept positions in the organizational relationship; for those concessions so multiplied the number of statuses and so altered the status of persons that a system of relationships was created where only one had existed before.

The fact is that the reforms the Company instituted in 1609 were not sufficient either to swell the supply of labor migrating to Virginia or to motivate the planters who were there to work with the will the Company expected. The Company had hoped that by its reforms it would be able to obtain not "idle and wicked persons; such as shame, or fear compels into this action [but] fit and industrious [persons], honest sufficient Artificers."³⁹ Yet so unproductive were they that as late as 1616 John Rolfe could indicate to Sir Robert Rich that what had been was still the Company's most serious problem. Our greatest want, he wrote, is "good and sufficient men as well of birth and quality to command, soldiers to marche, discover and defend the country from invasion, artificers, labourers,

³⁸ Kingsbury (ed.), *Records*, III, 173, 160.

³⁷ *For the Colony in Virginia Britannia*, in Force (ed.), *op. cit.*, III, 44, 55, 61–62.

³⁹ Appendix to *A True and Sincere Declaration* (1609), in Brown, *Genesis*, I, 352; Virginia Company broadside of 1610, in Brown, *Genesis*, I, 439.

and husbandmen."⁴⁰ And so dissatisfied had the settlers become with their situation that, in a letter smuggled to the Spanish ambassador in London with the connivance of English sailors, Don Diego de Molina, the prisoner in Jamestown, reported that "a good many have gone to the Indians . . . and others have gone out to sea . . . and those who remain do so by force and are anxious to see a fleet come from Spain to release them from this misery."⁴¹ The hope that Don Diego attributed to the colonists was, no doubt, the wish of a patriotic Spaniard; but it is nevertheless true that some settlers did flee to the Indians, that the Company did succeed in obtaining authority to deport to Virginia those settlers who had escaped back to England, and that Coles and Kitchins, who had been Don Diego's guards, were executed in 1614 for organizing a plot to escape to Florida.⁴²

Nor did the concessions granted to superior colonists in 1614, including a kind of modified right to private property and some relief from the obligation to work on the Company lands, suffice to solve the labor problem.⁴³ For the simple fact was, as Captain John Smith wrote, that "no man will go from hence to have less liberty there than here."⁴⁴ The Company, determined in 1619 to make a final effort to create of Virginia the profitable investment it had always hoped it would be, took his advice to heart. Though it was faced with declining financial resources, with internal bickering, and with increasing evidence that the king was losing patience with its meager achieve-

ment, the Company decided to pin its hopes on a quick return. The key to profits, it felt, lay in raising the value of the Company lands through increasing population and in diversifying products through the importation of labor skilled in many trades. The success of the effort, obviously, rested upon the strength of the additional inducements that could be offered to both investors and potential emigrants.⁴⁵

As always, one of the principal devices used by the Company to attract labor and to increase productivity was that of easing the terms on which land could be acquired. The effect of the reform was to create within the Company a new group of statuses differentiated from one another in terms of the amount of property attached to each or the length of time required to obtain land on the part of those who were not yet entitled to it:

1. "Ancient planters" who had come to Virginia at their own cost before 1616 received 100 acres per share in perpetuity rent-free.

2. "Ancient planters" who had come to Virginia at Company expense received 100 acres at an annual rent of 2s. after the completion of their seven-year period of servitude on the Company's land.

3. All persons who came to Virginia after 1616 at their own expense received 50 acres at an annual rent of 1s.

4. All persons who came to Virginia after 1616 at Company expense were to receive 50 acres after having worked on the Company's land for seven years, during which time half their produce belonged to the Company and half to themselves.

5. All tradesmen received a house and 4 acres of land so long as they plied their trades.

6. All persons who paid for the transportation of emigrants received 50 acres per person.

7. Company officers not only were entitled to their regular land grants but were supported by the labor of tenants-at-halves on large tracts

⁴⁰ Quoted in Charles M. Andrews, *The Colonial Period of American History* (New Haven, 1934-38), I, 113-14.

⁴¹ Brown, *Genesis*, II, 648-49.

⁴² Morris, *op. cit.*, pp. 169-71.

⁴³ Ballagh, *op. cit.*, pp. 22-23; Osgood, *op. cit.*, I, 75-77; Bruce, *Economic History*, I, 212-15; Craven, *Southern Colonies*, pp. 116-17; A. E. Smith, *op. cit.*, pp. 10-11.

⁴⁴ Quoted in Miller, "Religion and Society," *op. cit.*, p. 37.

⁴⁵ Craven, *Virginia Company*, *passim*, but esp. pp. 168-71; Craven, *Southern Colonies*, pp. 145-47; Scott, *op. cit.*, II, 266-88; Susan Myra Kingsbury, *An Introduction to the Records of the Virginia Company of London* (Washington, 1905), pp. 34-35, 40-41, 94-95.

of land reserved by the Company for that purpose.⁴⁶

8. Indentured servants, whose transportation was paid by the Company or by private associations of investors and who were then sold to planters on their arrival in Virginia, were entitled to "freedom dues"—including a land grant—on the expiration of their servitude.⁴⁷

Nor was this all. Determined to improve the morale of the colonists and, eventually, to relieve the Company of the burdensome cost of transporting labor from England, Sandys also began in 1620 to ship women to Virginia to become wives of the planters. There had been marriages in Virginia before, of course, but the supply of single women, restricted to the few female servants of married couples, was far smaller than the demand. Now, however, the Company organized the shipment of women on a business basis, forming a separate joint-stock company for the purpose. Though the women were, in any case, to be paid for by the planters at the rate of 120 pounds of the best leaf tobacco per person and though the Company conceded that it was dubious as to its authority to control marriages—"for the libertie of Mariadge we dare not infrindg"—it nevertheless discriminated between classes of planters in the bestowal of the women. "And though we are desireous that mariadge be free according to the law of nature," the Company wrote to the Governor and Council of Virginia, "yett would we not have these maids deceived and married to servants, but only to such freemen or tenants as have meanes to maintaine them."⁴⁸

Finally, in a radical departure from previous policy, the Company limited the

scope of martial law and ordered Governor Yeardley to convene an assembly of elected representatives from each district in Virginia. The Company did not intend to diminish its own authority, for the Governor was given the right to veto all enactments of the Assembly, and the General Court of the Company in London retained the right to disallow its decisions. Rather was it the Company's hope that the degree of acceptance of its program would be increased if it had the added sanction of approval by representatives of the planters themselves.⁴⁹

In a sense, the Company's reforms succeeded too well. Lured by the new prospects in Virginia, about 4,800 emigrants departed from England between November, 1619, and February, 1625, nearly twice as many as had gone during the entire period from 1607 to 1619.⁵⁰ But, while the Company's propaganda could refer blandly to "each man having the shares of Land due to him" and to "the laudable forme of Justice and government,"⁵¹ actual conditions in Virginia were quite different. Goodman Jackson "much marvelled that you would send me a servant to the Companie," young Richard Freethorne wrote to his parents:

He saith I had beene better knocked on the head, and Indeede so I fynde it now to my great greefe and miserie, and saith, that if you love me you will redeeme me suddenlie, for wch I doe Intreate and begg. . . . I thought no head had beene able to hold so much water as hath and doth daylie flow from mine eyes. . . . But this is Certaine I never felt the want of ffather and mother till now, but now deare freinds full well I knowe and rue it although it were too late before I knew it.

⁴⁶ "Instructions to Governor Yeardley, 1618," *Virginia Magazine of History and Biography*, II (1894-95), 161-62; Bruce, *Economic History*, I, 226-33, 511-14; Ballagh, *op. cit.*, pp. 25-28, 31; Craven, *Virginia Company*, pp. 50-57; Craven, *Southern Colonies*, pp. 127-29.

⁴⁷ A. E. Smith, *op. cit.*, pp. 11-17; Ballagh, *op. cit.*, pp. 28-30; Bruce *Economic History*, II, 41-48; Morris, *op. cit.*, p. 395.

⁴⁸ Kingsbury (ed.), *Records*, III, 115, 493-94, 505.

⁴⁹ Thomas Jefferson Wertenbaker, *Virginia under the Stuarts* (Princeton, 1914), pp. 38-39; Craven, *Virginia Company*, pp. 70-80; Craven, *Southern Colonies*, pp. 127-29; "Proceedings of the First Assembly in Virginia, Held July 30, 1619," in *Colonial Records of Virginia* (Richmond, 1874).

⁵⁰ Samuel H. Yonge, "The Site of Old 'James Towne,' 1607-1698," *Virginia Magazine of History and Biography*, XI (1903-4), 399-400.

⁵¹ *A Declaration of the State of the Colony* (1620), in Force (ed.), *op. cit.*, III, 5-6.

"To write of all crosses and miseries w^{ch} have befallen us at this tyme we are not able," said Samuel Sharp. "So the truth is," Edward Hill wrote to his brother, "we lyve in the fearefullest age that ever Christians lived in."⁵²

Though Company policy was not responsible for all the suffering endured by the settlers, it was responsible for intensifying their sense of deprivation by having promised too much. "My Master Atkins hath sould me," Henry Brigg wrote to his brother, Thomas:

If you remember he tould me that for my Diett the worst day in the weeke should be better then the Sunday, & also he swore unto you that I should never serve any man but himselfe: And he also tould us that there they paled out their groundes from Deare & Hoggs. But in stead of them we pale out o^r Enemyes.

"If the Company would allow to each man a pound of butter and a pounce of Chese weekly," wrote a planter to Sir John Worsenholme,

they would find more comfort therein then by all the Deere, Fish & Fowle is so talked of in England of w^{ch} I can assure yo^r yo^r poore servants have nott had since their cominge into the Contry so much as the sent.

"I am pswaded," George Thorp wrote to John Smyth of Nibley,

that more doe die of the disease of their minde then of their body by having this country vicuall over-praised unto them in England & by not knowing, they shall drinke water here.⁵³

No doubt the chasm between expectation and reality contributed to the planters' alienation from the organizational relationship into which they had been lured by the Company's promises. But that relationship was affected even more by the development of a network of relations that followed inevitably from the inducements to get men into the Company.

⁵² Kingsbury (ed.), *Records*, IV, 59, 61-62, 239, 234; see also *ibid.*, pp. 41-42, 232, 235-36.

⁵³ *Ibid.*, pp. 235-36, 312-32; III, 417; see also *ibid.*, III, 456.

At one time in Virginia, the single relationship that existed between persons rested upon the positions they occupied in the Company's table of organization. As a result of the efforts made by the Company to get persons to accept that relationship, however, each person in Virginia had become the occupant of several statuses, for now there were rich and poor in Virginia, landowners and renters, masters and servants, old residents and newcomers, married and single, men and women; and the simultaneous possession of these statuses involved the holder in a network of relationships, some congruent and some incompatible, with his organizational relationship.

Once the men in Virginia had been bachelors who lived in Company-provided barracks. Now they lived in private houses with their families, and, though the Company attempted to make use of the new relationship by penalizing each "Master of a family" for certain crimes committed by those under his authority⁵⁴—hoping thereby that the master would use his authority to suppress crime—it can hardly be doubted that its action involved the head of the family in a conflict of loyalties.

Once all persons had been equal before Company law, and penalties had been inflicted solely in accordance with the nature of the offense. Now, the General Assembly found that "persones of qualitie" were "not fitt to undergoe corporall punishment."⁵⁵

Once length of residence was irrelevant in determining the obligations of persons to the Company. Now, however, it was enacted that all "y^e olde planters, y^t were heere before, or cam in at y^e laste cominge of Sr. Tho: Gates they and their posteritie shalbe exempted from their psonall service to y^e warres, and any publike charge (Churches dewties excepted)."⁵⁶

Once Virginians had been governed administratively through a chain of command

⁵⁴ Proclamation of Governor Wyatt, June, 1622, in Kingsbury (ed.), *Records*, III, 659.

⁵⁵ Act of March, 1623/24 (*ibid.*, IV, 584).

⁵⁶ Act of March, 1623/24 (*ibid.*, IV, 582).

originating in the Company's General Court. Now an authentic political system existed, and the members of the Assembly demanded the same right to disallow orders of the General Court that the Court had with respect to the Assembly.

Once all land had been owned by the Company. Now much of it was owned by private persons, and even more had been promised to them, and the opportunities for the creation of private fortunes involved the planters in a new relationship with the Company. No longer was the planter willing to have his tobacco exported through the Company at a fixed price, when, as a free landowner, he might strike his own bargain with the purchaser. No longer was the planter willing, at a time when labor meant profit, for the Company to commandeer his servants. Even officers of the Company, expected to administer its program in Virginia, saw the chance to subvert it to their own purposes; "The servants you allow them, or such as they hire," Captain John Smith told the Company, "they plant on their private Lands, not upon that belongeth to their office, which crop alwaies exceeds yours." Indeed, it became increasingly difficult to get planters to accept Company positions:

S^r George is taken up with his private. . . . Capt. Hamor is miserable poore and necessities will inforce him to shift. . . . Capt: Mathews intends wholie his Cropp, and will rather hazard the payment of forfeitures, then performe our Injunctions. . . . M^r Blanie is now married in Virginia, and when he hath discharged your trust in the Magazine wilbee a Planter amongst us. . . . And I would you could persuade some of qualities and worth to come out.⁵⁷

The increase in private wealth tended to subordinate status in the Company to status in a different relationship among the planters. The muster roll of early 1625 shows 48 families bearing various titles of distinction, most of which had been earned in Virginia. They alone held 266 of the ap-

proximately 487 white servants in Virginia, 20 of the 23 Negro servants, and 1 of the 2 Indian servants.⁵⁸ These were the families at the apex of Virginia society, determined to uphold their rights as over against other persons and sometimes going beyond their rights. Acting through the General Assembly, they insisted upon scrupulous enforcement of contracts of servitude, forbade servants to trade with the Indians, and, so as not to lose their labor, regulated the right of their servants to marry. Nor, as the chronic complaints bear witness, were they loath to keep their servants beyond the required time.⁵⁹ That aspect of the relationship between master and servant was eloquently revealed in a petition to the Governor by Jane Dickenson in 1624:

[She] most humblie sheweth that whereas her late husband Ralph Dickenson Came ov^r into this Country fower Yeares since, obliged to Nicholas Hide deceased for y^e tearme of seaven yeares, hee only to have for himselfe & yo^r petitioner y^e one halfe of his labors, her said husband being slaine in the bloody Masacre, & her selfe Caried away wth the Cruell salvages, amongst them Enduring much misery for teen monthes. At the Expiration it pleased God so to dispose the hartes of the Indians, y^t for a small ransome yo^r petitioner wth divers others should be released, In Consideration that Doctor Potts laid out two pounds of beades for her releasement, hee alleageth yo^r petitioner is linked to his servitude wth a towefold Chaine the one for her late husbandes obligation & thother for her ransome, of both w^{ch} shee hopeth that in Conscience shee ought to be discharged, of y^e first by her widdowhood, of the second by the law of nations, Considering shee hath already served teen monthes, two much for two pound of beades.

The pmisses notwthstanding D^r Pott refuseth to sett yo^r petitioner at liberty, threatening to make her serve him the uttermost day, unless she pcure him 150^{li} waight of Tobacco, she

⁵⁷ *Ibid.*, IV, 564, 581; Smith, *Generall Historie*, in Tyler (ed.), *op. cit.*, p. 356; George Sandys to John Ferrar, April 11, 1623, in Kingsbury (ed.), *Records*, IV, 110-11.

⁵⁸ The figures are derived from the muster rolls in John Camden Hotten, *The Original Lists of Persons of Quality; Emigrants, Religious Exiles . . . Who Went from Great Britain to the American Plantations, 1600-1700* (London, 1874).

⁵⁹ A. E. Smith, *op. cit.*, pp. 226-29; Kingsbury (ed.), *Records*, IV, 128-30.

therefore most humbly desireth, that you^a wilbe pleased to take w^t Course shalbe thought iust for her releasement fro' his servitude, Considering that it much differeth not from her slavery wth the Indians.⁶⁰

But that was only one aspect of the relationship. Conditions in Virginia were now more fluid than they had been, and persons of low estate might also rise. Secretary of State John Pory wrote Sir Dudley Carleton that "our cowekeeper here of James citty on Sundays goes accowtered all in freshe flaminge silke; and a wife of one that in England had professed the black arte, not of a scholler, but of a collier of Croydon, wears her rought bever hatt with a faire perle hat band." The Company was opposed to such unseemly displays of wealth on the part of persons of low estate,⁶¹ but it could not prevent them.

The ultimate stage in the transition of Virginia from organization to society was reached when the settlers came to feel that the new relationships in which they were now involved were of greater importance than the Company relationship, when their statuses outside the organization came largely to dictate their behavior. For at that point they were no longer willing to accept the legitimacy of their organizational superiors. William Weldon warned Sir Edwin Sandys that the planters who now had land were grumbling at Company policy:

I acquainted them wth my restraint of plantinge Tobacco w^{ch} is a thinge so distastefull to them that they will wth no patience indure to heare of it bitterly Complayninge that they have noe other meanes to furnish themselves with aparell for the insuinge yere but are likely as they say (and for aught I Can see) to be starved if they be debarred of it.⁶²

From general discontent it was but a short step to ridicule of Company officials and outright refusal to accept a Company assignment. Wrote planter William Capps to John Ferrar:

⁶⁰ Kingsbury (ed.), *Records*, IV, 473.

⁶¹ Pory to Carleton, September 30, 1619, in Tyler (ed.), *op. cit.*, p. 285; Kingsbury (ed.), *Records*, III, 469.

⁶² Kingsbury (ed.), *Records*, III, 263.

The old smoker our (I know not how to terme him but) Governor, so good so careful mild, Religious, iust, honest that I protest I thinke God hath sent him in mercie for good to us, he undergoeth all your cares & ours and I feare not but god will bless him in all his pceedinges but who must be th'Instrument to make all this whole againe? Why Capps: all voyces can sett him forth about the business: But who must pay him his hyre? The Contrey is poore and the Companie is poore and Capps is poore already, & poorer he will be if he follow this course.

Like other men, planter Capps believed that "Charity first beginnes at home," and he divorced his own interest from that of the Company:

I will forswear ever bending my mind for publike good, and betake me to my own profit with some halfe a score men of my owne and lie rootinge in the earthe like a hog, and reckon Tobacco ad unguem by hundrethes, and quarters.⁶³

That the Company could no longer expect to command obedience was clear, for even its officers in Virginia perceived themselves as having a set of interests distinct from those of their London superiors and turned their backs to their authority. "Such is the disposicon of those who glorie in their wisdomes," wrote George Sandys, the treasurer in Virginia, to his brother, Sir Miles,

that they will rather Justifye and proceed in their Errors than to suffer a supposed disgrace by reforming them. . . . Who clere themselves by the wrongings of others; objecting unto us their Instructions, whereof manie are infeasible and the most inconvenient, for to say the truth they know nothing of Virginia.

"Such an Antipathy is there between theyr vast Comands and o' grumbling Obedience," Sir Francis Wyatt wrote to his father:

Mingling matters of honor and profit often overthrow both. They expect great retournes to pay the Companies debt. . . . For me I have not a third part of my men to inable me to either. . . . I often wish little M^r Farrar here, that to

⁶³ *Ibid.*, IV, 38-39.

his zeale he would add knowledge of this Contrey.⁶⁴

In 1607 there had been no "Contrey," only the Virginia Company. It was the Company's fate to have created a country and to have destroyed itself in the process. More than a century later, James Otis wrote bitterly: "Those who judge of the reciprocal rights that subsist between a supreme and subordinate state of dominion, by no higher rules than are applied to a corporation of button-makers, will never have a very comprehensive view of them."⁶⁵ His comment was intended as an observation on contemporary political affairs, but we can detect in it a verdict on the past as well.

The Company had been faced with the problems of motivating its members to work for the ends which it was created to achieve and, at the same time, of maintaining the discipline that was essential for its organizational integrity. The solution it adopted for the first problem made it impossible to solve the second; and the burden of achieving order and discipline now became the responsibility not of an organization but of a society.

Among the papers in the Sackville collection is a document entitled "A Form of Policy for Virginia," written when it was already apparent that the Company had failed. The proposal was never adopted, but it is significant nonetheless, for, as Professor Fernand Braudel reminds us,

victorious events come about as a result of many possibilities, often contradictory, among which life finally has made its choice. For one possibility which actually is realized innumerable others have been drowned. These are the ones which have left little trace for the historians. And yet it is necessary to give them their place because the losing movements are forces which have at every moment affected the final outcome.⁶⁶

⁶⁴ *Ibid.*, pp. 71, 237; see also *ibid.*, pp. 455-57.

⁶⁵ *The Rights of the British Colonies asserted and proved* (Boston, 1764), in Samuel Eliot Morison (ed.), *Sources and Documents Illustrating the American Revolution . . .* (2d ed.; Oxford, 1929), p. 8.

The significance of the document, drafted as a royal proclamation, lies in its awareness of the problems of motivation and order, in its realization that they could no longer be solved by instructions handed down through a chain of command, and in its conscious application of particular social inventions to solve them:

Wee . . . knowinge that the perfection and happinesse of a commonwealth, lyeth . . . first and principally in the government, consisting in the mutuall duties of commandeing and obeyeing, next in the possessing thinges plentifully, necessarie for the life of man, doe professe that . . . we intend wholly the good of our subjects . . . endeavouringe to cause both England and Virginea, to endowe each other with their benefittes and profits that thereby layeing aside force and our coactive power, we may by our justice and bountie marrye and combinde those our provinces to us and our soveraigntye in naturall love and obedience.

The problem of order was solved by the meticulous enumeration of every social status that was to exist in Virginia, with a specification of the rights and obligations that inhered in each. The problem of motivation was solved by the granting of both economic rewards and social privileges to each status and by the opportunity given to move from one to another:

The meanest servant that goeth (God soe blessing him and his endeavours, that hee can purchase and [an] estate in England or compasse to carrie over or drawe over with him of his friends and adherences the number of 300 men) he may become a lord patriot which is the greatest place the commonwealth canne beare.

The problem of consensus was solved through devices to enhance the mutual affection of persons in these statuses:

To the end that love may be mayntayned, and that theise degrees may not estrange the upper orders from the lower, we wish that the heires and eldest sonnes of the upper orders may marrie with the daughters of the lower orders. . . . And that the daughters of the upper

⁶⁶ Quoted in Paul F. Lazarsfeld, "Public Opinion and the Classical Tradition," *Public Opinion Quarterly*, XXI, No. 1 (Spring, 1957), 53.

orders being heires may marrye with the sonnes of the lower orders, making choice of the most vertuous . . . that all degrees may bee thereby bound together in the bonds of love that none may be scorned but the scorner. To this end alsoe, although we would not have you imitate the Irish in their wilde and barbarous maners, yet we will commend one custome of theires unto you, which is that the poorer sort sueing to gett the nursing of the children of the lordes and gentry, and breedeinge upp in their minorities as their owne, this breedinge . . . doth begett anoother nature in them to love their foster children and brethren, as if they were naturally bread of the same parentes.

Written in the margin of the document, by whom we do not know, is a lengthy commentary. Concerning the importance of status and order, the following is written: "This maintenance of theire degrees will immoveably fixe the frame of the collonie." Concerning the importance of mobility and motivation, the following is written: "Soe framage the government that it shall give all men both liberty and meanes of riseinge to the greatest places and honours therein, whereby they will receave such content that they will all strive to maintaine it in the same forme we shall now settle it."⁶⁷

Shakespeare had written:

Take but degree away, untune that string
And hark, what discord follows.

The author of the document agreed. He rested his hopes for stability on the attachment of each person to a position in which recognized rights and responsibilities inhered. What he did not realize is what may be learned from the history of the Virginia Company—that each man is attached to many positions, that each position involves him in a separate relationship that imposes its own necessities, and that his behavior is the product of all the positions he holds and, because he has a memory, of all the positions he once held.

II

The generalizations that emerge from our study are of two kinds: those directly

⁶⁷ Kingsbury (ed.), *Records*, IV, 411, 417, 424-25, 416, 419.

tied to the events of the time and place that we have analyzed and those of a more abstract kind that derive from the analysis of these historical particulars but can be stated in such a way as to be of more general applicability.

There seems little room for doubt about some of the conclusions we have drawn: that the character of seventeenth-century North American society was shaped decisively by the fact that, in contrast to the situation in Latin America, the creation of the society was accomplished through the recruitment of a voluntary labor force; that higher statuses in that society were created as a result of the need to induce persons to accept positions in lower statuses; and that the behavior of persons in that society was determined not only by opportunities for advancement, as Whiggish interpreters of our history would have us believe, but, as well, by the fact that these opportunities were less than people had been led to expect.

With respect to more general hypotheses, it may be suggested that the mechanism by which the change from organization to society was accomplished lay in the very effort to apply the blueprint that was intended to govern the relations between persons, for this so multiplied the number of statuses persons held, and therefore the relationships in which they were involved, as to alter their behavior in a decisive fashion.

The testing of these hypotheses, of course, would involve the examination of still other consciously selected historical situations for the purpose of comparison—the experience of the British in establishing other colonies in North America and in coping with a totally different problem in India, of the French in Canada and the Spanish in South America, of the reasons for the difference between the blueprint in accordance with which utopian communities were planned and the outcome of their establishment, and the like. Herein lies the design for a research in historical sociology.

CULTURE PROFILES AND EMPHASES¹

EDWARD ROSE AND GARY WILLOUGHBY

ABSTRACT

A composite culture profile can be constructed displaying the amount of information available on each of the major culture categories in the Human Relations Area File. The records of twenty cultures are here compared with this composite profile in order to distinguish deviations of emphasis on various activities in each of the cultures. Working with this variety of emphases, we have stipulated, through scalogram analysis, those characteristics by which the cultures can be consistently compared. Further, by examining the extent to which and the ways in which the twenty cultures possess scalable features, we obtained rankings and typologies of cultures. Modern cultures seem to exhibit a correlation between extent and type of emphasis on modern categories of culture. No corresponding correlation between extent and type of primitive emphasis seems to hold for primitive cultures.

The editors of the Human Relations Area File have recently prepared an important document,² a listing in terms of percentages of the amount of information in their whole file on each general category of culture described in their *Outline of Cultural Materials*.³ HRAF is the largest compilation of cultural records arranged for cross-cultural comparisons. Therefore, it is interesting to know how it covers various phases of culture.

The listing of the allotment of information in HRAF is the first basic improvement in universal culture schemes since Clark Wissler published his famous outline in 1923.⁴ The listing adds particulars to such schemes, permitting the construction of a composite culture profile displaying differences of emphasis on the various aspects of culture. And this composite profile,

world-wide in scope, can be used in the appraisal of specific culture emphases and in developmental ratings of cultures. Thus very old questions in the comparison of cultures can now be reconsidered.

A composite culture profile is presented in Table 1. The records of twenty cultures are here compared to this composite profile in order to determine how particular accounts of culture deviate in emphases on different culture phases from the general profile. This information on particular cultural emphases may then be used through scale analysis to determine which aspects of culture appear to provide reliable and consistent distinctions between cultures. As will be demonstrated below, these distinctions can be put into terms of "primitivism" and "modernity." And thus the twenty culture records can be arranged in developmental schemes and their more primitive or more modern features noted.

Two recent studies of HRAF materials have brought forth systematic comparisons of cultures. Naroll has devised an index of social development and written an extensive review of the problem of culture comparison.⁵ And Freeman and Winch have

¹ We wish to thank Brahm Arrieh for assistance in the collection of our data and Rodney Elliott for assistance in scaling. A grant from the Council on Research and Creative Work of the University of Colorado helped us prepare this study.

² Ditto-processed information sheet: "Percentage of File Slips for Each Two-Digit *Outline of Cultural Materials* Category No." (New Haven: Human Relations Area Files, 1956).

³ George P. Murdock *et al.* (New Haven: Human Relations Area File, 1950). This outline will occasionally be referred to hereafter as "OCM." The Human Relations Area File will be referred to hereafter as "HRAF."

⁴ *Man and Culture* (New York: Thomas Y. Crowell, 1923), p. 74.

⁵ Raoul Naroll, "A Preliminary Index of Social Development," *American Anthropologist*, LVIII (August, 1956), 687-715. See also Harold E. Driver, *An Integration of Functional, Evolutionary, and Historical Theory by Means of Correlations* ("Indiana University Publications in Anthropology and Linguistics," Memoir XVII [Bloomington, Ind., 1956]), pp. 1-36.

TABLE 1

A COMPOSITE CULTURE PROFILE AND THE PROFILES OF THREE CULTURE RECORDS*

	OCM Cat. No.	HRAF Percentages of Information	Copper Eskimo	China	Czecho- slovakia
Total.....	..	100.000	1870	24,495	13,937
THE PRODUCTIVE ARTS.....	..	33.898	670	6578	5896
Facilities.....	..	4.181	139	590	580
Settlements.....	36	1.921	15	235	361
Structures.....	34	1.469	98	283	182
Equipment and maintenance of buildings	35	.791	26	73	37
The productive process	6.328	102	1179	1286
Energy and power ..	37	.565	13	193	62
Tools and appliances ..	41	.678	82	39	6
Machines ..	40	.339	0	20	26
Labor ..	46	3.390	7	637	739
Business and industrial organization.....	47	1.356	0	290	453
Food production.....	..	6.102	260	1318	1060
Food quest ..	22	1.017	206	62	43
Animal husbandry ..	23	1.243	19	230	266
Agriculture ..	24	3.051	0	928	653
Food processing.....	25	.791	35	98	98
Industries.....	..	5.424	62	725	1093
Exploitative activities ..	31	2.260	14	447	544
Processing of basic materials ..	32	.791	25	93	172
Building and construction.....	33	.452	8	19	13
Leather, textiles, fabrics.....	28	1.017	5	132	217
Chemical industries ..	38	.565	0	23	102
Capital goods industries ..	39	.339	10	11	45
Transportation	3.729	66	566	280
Travel and transport.....	48	1.356	47	201	72
Land transport ..	49	1.469	11	223	128
Water and air transport ..	50	.904	8	142	80
Value and exchange.....	..	8.136	41	2200	1597
Property.....	42	2.372	14	723	334
Finance.....	45	1.130	0	233	238
Exchange.....	43	3.955	27	1016	952
Marketing.....	44	.678	0	228	73
THE REGULATIVE ARTS.....	..	32.542	218	8158	5680
The family.....	..	4.293	75	1829	142
Marriage.....	58	1.356	21	433	81
The nuclear family.....	59	1.243	20	1010	19
Kinship.....	60	1.130	28	260	40
Kin groups.....	61	.565	6	126	2
Social constructs.....	..	8.023	67	2034	570
Living standards, routines.....	51	1.356	28	347	145
Community.....	62	1.921	13	287	159
Social stratification.....	56	3.503	5	943	233
Individuation and mobility.....	55	1.243	21	457	33
Government.....	..	11.525	16	2822	2834
Territorial organization.....	63	1.130	1	203	144
The state ..	64	4.746	15	1158	1201
Government activities ..	65	3.277	0	549	640
Political behavior.....	66	2.372	0	912	849
Legal control.....	..	3.842	18	234	557
Law.....	67	1.469	8	96	163
Offenses and sanctions.....	68	1.243	10	69	84
Justice.....	69	1.130	0	69	210
Magic, medicine, and welfare.....	..	2.599	34	421	483
Sickness (magic, medicine).....	75	1.695	33	251	80
Health and welfare (highly institutional- ized controls of health and social prob- lems).....	74	.904	1	170	403
Socialization and education.....	..	2.260	8	818	1094
Socialization.....	86	.452	8	107	17
Education.....	87	1.808	0	711	1077

* Excessive emphases on cultural activities in comparison with the composite profile are indicated in italics. Percentages and other figures in this table are drawn from counts of data slips found under each OCM category in HRAF.

TABLE 1—*Continued*

	OCM Cat. No.	HRAF Percentages of Information	Copper Eskimo	China	Czecho- slovakia
THE PERSONAL EXPRESSIVE ARTS.....	..	12.543	422	2557	750
The life.....	..	1.356	14	290	47
Infancy and childhood.....	85	.678	10	102	19
Adolescence, adulthood, and old age.....	88	.678	4	188	28
Vital experiences.....	..	3.842	150	864	108
Sex.....	83	.791	14	74	7
Reproduction.....	84	.565	25	138	17
Eating.....	26	1.017	74	191	56
Death.....	76	1.469	37	461	28
Conduct and display.....	..	2.486	72	256	155
Behavior processes.....	15	.904	28	134	46
Dress.....	29	.904	26	88	89
Adornment.....	30	.678	16	34	20
Interpersonal relations.....	57	1.695	48	440	76
Pleasure.....	..	2.486	126	503	289
Drink, drugs, indulgence.....	27	.791	4	86	86
Recreation.....	52	1.130	122	338	149
Entertainment.....	54	.565	0	79	54
Social problems (disasters, defectives, addic- tion, invalidism, poverty, delinquency, etc.).....	73	.678	10	204	75
THE INSTITUTIONAL EXPRESSIVE ARTS.....	..	21.017	560	7202	1611
Communication.....	..	3.164	24	822	242
Language.....	19	1.234	23	280	74
Communication (over space).....	20	1.130	1	236	111
Records (communication over time).....	21	.791	0	306	57
War.....	..	2.938	3	453	248
Armed forces.....	70	.904	0	173	75
Military technology.....	71	.452	0	65	11
War.....	72	1.582	3	215	162
Religion.....	..	6.554	275	3507	297
Religious beliefs.....	77	2.712	66	1849	59
Religious practices.....	78	1.921	153	492	44
Religious organizations and persons.....	79	1.921	56	1166	194
Fine arts.....	53	2.825	167	666	317
Knowledge.....	..	2.486	57	1161	96
Numbers and measures.....	80	.452	6	126	6
Exact knowledge.....	81	.678	5	631	31
Ideas about nature and man.....	82	1.356	46	404	59
Total culture (ethos, norms, participation in culture, goals, ethnocentrism, etc.).....	18	3.051	34	593	411

applied scale analysis to an array of primitive cultures and prepared a good account of scaling techniques applied to cultural data.⁶ Both these studies will be discussed in the concluding comments of this paper.

THE COMPOSITE CULTURE PROFILE

The composite profile in Table 1 is derived from the HRAF listing mentioned above. For the convenience of exposition the categories in the *Outline of Cultural*

Materials are arranged in the profile under four general classifications of activity: (1) the productive arts, stressing tasks and technical and economic activities; (2) the regulative arts, stressing social and political control and organization; (3) the personal expressive arts, celebrating direct human experience; and (4) the institutional expressive arts, emphasizing established forms of human experience.

Every cultural activity may have its productive, regulative, and expressive aspects. These four general classifications attempt simply to indicate the leading emphases that appear to hold for materials in

⁶ Linton C. Freeman and Robert F. Winch, "Societal Complexity: An Empirical Test of a Typology of Societies," *American Journal of Sociology*, LXII (March, 1957), 461-66.

HRAF. Consider, for example, the inclusion of magic and medicine under the regulative arts and of social problems under the personal expressive arts. Most HRAF materials on magic and medicine do not concern human ailments as such but, rather, magical and medical controls over ailments and predicaments. The HRAF records on social problems, on the contrary, deal more with direct experience in misfortune than with ways of controlling or avoiding misfortune. Thus our scheme separates magic and medicine from social problems. As a rational ordering of historical facts, the culture profile has depended upon a number of judgments, qualifications, and compromises. However, as we will show in a scalogram analysis, many OCM categories that are critical in culture comparison were brought together in the profile; thus it should not be viewed as a purely arbitrary device.

The records of three cultures drawn from HRAF are included in Table 1 for comparison with the composite profile. The most drastic emphases for the Copper Eskimo are on the food quest (hunting and fishing), fine arts, and religion. For China, the family, religion, and knowledge have received unusual attention; for Czechoslovakia, government and education appear to hold special significance. Indeed, in none of the twenty cultures examined in goodness-of-fit tests was a culture record found whose profile fitted at all closely the composite culture profile. The variability in the records of individual cultures is so great that it is highly unlikely that any culture will be discovered with a record corresponding to the composite profile. There appears to be no type-specimen representing a universal culture pattern.

This extreme variability represents one of two things: that many particular activities in every culture are of such significance that any observer may report on them at length or that observers have allowed their own particular interests to shape the cul-

ture records. It is not possible at this time to separate the sources of emphasis, but the following discussion on HRAF materials will provide a clearer understanding of the problem.

THE CULTURE RECORD

The composite culture profile is based upon a record of more than half a million file slips, and the twenty cultures examined here have contributed to a record of over a hundred thousand file slips (see Table 2). The HRAF editors have classified eight of these cultures as primitive and twelve as more advanced. The dimensions of the various HRAF records are grossly dissimilar. Table 2, for instance, reveals the differences in numbers of sources, in periods of time covered for each culture, and in the sizes of the culture records. Since the University of Colorado files are still limited and in process of formation, we have not been free in this initial survey to test the effect of these differences and to reduce them in setting up our sample. As HRAF records are improved and enlarged, some of these dissimilarities can be avoided or reduced.

Six general sources of error and bias appear in the analysis of HRAF materials: (1) native informants and documents screen information; (2) observers have pre-established interests and skills, differing associations with peoples and their informants, and different ways of reporting their discoveries; (3) the HRAF editors, though exercising the greatest care, have undertaken further selections of cultures, culture categories, sources, and compilation procedures; (4) HRAF analysts produce occasional mistakes and disagreements in the classification of data; (5) the writers of this paper have selected cultures, problems, and analytical techniques and may have made recording and computational mistakes; (6) readers, by their preferences for certain sorts of inquiries and results,

ultimately will determine what are to be accepted as results.

The last four sources of distortion can be estimated and corrected. Indeed, the reliability of HRAF analyst classifications has been estimated and, in the light of the number of decisions the analysts had to make, it is found to be remarkably high: 85 per cent agreement between analysts on detailed categories of culture.⁷ The contributions of the editors to HRAF are plainly stated and are systematic.⁸ For reasons of convenience they have chosen English-lan-

guage sources throughout the world and in deciding the order of work on these cultures. The *Outline of Cultural Materials* is, in effect, a rather haphazard catalogue but one, nonetheless, amazingly adaptable to the description of cultures.

In this study we were guided in the selection of cultures by the availability of data in the University of Colorado HRAF depository and by a plan to compare eastern European cultures with modern cultures of Asia and with widely separated primitive cultures. Our selection does not attempt to

TABLE 2
TWENTY CULTURE RECORDS*

HRAF CLASSIFICATION	CULTURE	PERIOD		No. FILE SLIPS	No. SOURCES
		Starting	Ending		
R-1.....	Soviet Union	1918	1956	10,219	82
EA-1.....	Poland	whole history	1954	4,357	30
EB-1.....	Czechoslovakia	1918	1955	13,937	70
ED-1.....	Rumania	1900	1954	1,985	9
AF-1.....	China	1911	1956	24,495	77
AJ-1.....	Tibet	1790	1953	10,215	19
AW-1.....	India	"modern"	1956	6,232	30
AP-1.....	Burma	1900	1954	4,731	26
AO-1.....	Thailand	1900	1954	9,657	33
AN-1.....	Malaya	1900	1954	6,331	91
AM-1.....	Indochina	1887	1953	5,763	118
OB-1.....	Indonesia	"modern"	1950	1,858	1
RU-4.....	Samoyed	prior to 1847	1939	1,679	10
AD-4.....	Formosan aborigines	prior to 1890	1954	2,183	7
FT-6.....	Thonga	prior to 1927	1927	3,813	2
OR-11.....	Marshall Islanders	prior to 1949	1949	891	1
SF-21.....	Siriono	prior to 1928	1950	1,162	5
NR-13.....	Northern Paiute	prior to 1924	1934	801	2
ND-8.....	Copper Eskimo	prior to 1797	1932	1,870	26
OI-19.....	Tasmania	prior to 1890	655	1

* In HRAF at the University of Colorado, March 1, 1957.

guage sources and sources translated into English. Naturally, they have given preference to the sources that have proved the most adequate. Circumstances prevented the drawing of a random sample of cultures for inclusion in the HRAF, and therefore the editors have been forced to use special judgment in compiling a file repre-

represent a world-wide cultural array. Our preconceptions placed Czechoslovakia and the Soviet Union as the most advanced cultures in our samples and Tasmania as the most primitive.

The most serious reliability problem is presented by observers. Since we shall here be concerned especially with differences between primitive and modern cultures, we must ask whether each of these sorts of cultures attracts, in turn, different sorts of observers, whose investigations will have differential effects on culture records. An ethnologist's bias, for instance, may affect culture profiles somewhat differently from

⁷ *Papers and Materials for Annual Meeting* (New Haven: Human Relations Area Files, 1956), pp. 58-61.

⁸ George P. Murdock, *Outline of World Cultures* (New Haven: Human Relations Area Files, 1954). See also *Guide to the Use of the Files* (New Haven: Human Relations Area Files, 1956) and *Outline of Cultural Materials*.

the way a political economist's will (see Table 3).

Table 3 discloses that sources on the twenty cultures treated here are equally divided between studies by social scientists and those of other observers. A remarkably large number of ethnologists have investigated the eastern European and Asiatic cultures. However, few social scientists, other than ethnologists and linguists, have dealt with the primitive groups in our sample, as the high chi square for Table 3 indicates. The test here is incomplete, of course.

TABLE 3
SORTS OF OBSERVERS OF THE
TWENTY CULTURES*

	Eight Primitive Cultures	Twelve East- ern European and Asiatic Cultures
Ethnologists and linguists..	7	94
Other social scientists.	1	200
All other observers.....	12	290

$$\chi^2 = 9.327 > \chi^2_{.01}$$

* Information regarding observers is not available on thirty-six sources used in this study. The chi-square value reported here should be accepted casually, since the expected values for primitive cultures are small.

It is not indicated that the description of primitive cultures is twisted by an ethnologist's bias. But if such a bias exists, it could easily have found expression in our sample.

Since in our materials errors and distortions arising in the observation process have not been separated from actual cultural emphases, this analysis, like most attempts to deal with historical materials, depends upon a balance of rationalizations, such as the following: (1) Records drawn from many sources are in general the most reliable. The cultural records for Indonesia, the Thonga, the Marshall Islanders, and the Tasmanians, though prepared by careful observers, lack the advantage of consensual validation, and on this account are more questionable. (2) No matter how dedicated and restricted an observer may be, a certain correlation may be expected between emphases in fact and emphases of observation. This assumption is based upon

the old adage that facts are stranger than fiction and thus are likely to command, in part at least, the attention of the observer. (3) Naturally, it is well to try to gauge the professional and the personal biases of an observer. Indonesia is a case in point here. Indonesia appears in the HRAF exclusively through the field notes of Raymond Kennedy, a competent ethnologist. A completely different culture profile would obtain for Indonesia had the editors chosen as their source *Netherlands India*, by J. S. Furnivall, an equally competent economic historian.

Despite serious difficulties in estimating the reliability of the data, we trust that the errors of observers and of analysts do not completely screen actual emphases in cultures reflected in emphases in the record. If we are not permitted this assumption, then our study can at least point to parts of the cultural record that must be clarified.

THE VARIABILITY OF CULTURES

Results of the tests for the twenty culture records of their goodness-of-fit to the composite profile are given in Tables 4 and 5. Tibet and India show the poorest fits. The best fit, appearing for the Samoyed, produced a chi-square of 439.0, which at twenty-three degrees of freedom greatly exceeds the .1 per cent level of significance. All the individual records profoundly deviate from the composite pattern.

Table 5 shows how deviations from the composite profile accumulated in each culture phase. Again no culture phase seems to approach the general pattern. The most extreme variations occurred in religion, government, and the family.

The 1 per cent significance level at one degree of freedom was exceeded in 348 out of 480 individual cells in these twenty goodness-of-fit tests. Religion displayed, in individual countries, the most fantastic disparity from expectations. In Tibet, India, and China emphases on religion produced chi-squares of 10,378.51, 5,514.71, and 2,253.96. The questions immediately arise:

Do such similar deviations reflect a general pattern of variability? And do any aspects of culture display similar or congruent patterns of variability throughout the twenty cultures?

THE ANALYSIS OF SCALOGRAMS

In response to the foregoing questions we undertook the analysis of scalograms.⁹ For scaling we took note of whether the record of an activity in each culture ex-

cessive deliberation on business and industrial organization would indicate a modern interest and emphasis. These decisions were based on ordinary impressions of history and the writing of history. Scalogram analysis reveals all discordant decisions when a pattern appears for any activity that is exactly the reverse of the pattern

TABLE 4

GOODNESS-OF-FIT TEST RESULTS FOR TWENTY CULTURES COMPARED WITH THE COMPOSITE CULTURE PROFILE

	Chi-square
Soviet Union.....	2975.82
Poland.....	1330.24
Czechoslovakia.....	5016.13
Rumania.....	1610.62
China.....	5186.52
Tibet.....	12,141.43
India.....	7776.48
Burma.....	1448.05
Thailand.....	1641.50
Malaya.....	2895.53
Indochina.....	2025.32
Indonesia.....	653.03
Samoyed.....	439.01
Formosan aborigines.....	1296.47
Thonga.....	3874.98
Marshall Islanders.....	1141.30
Siriono.....	1482.78
Northern Paiute.....	620.87
Copper Eskimo.....	1431.24
Tasmania.....	662.15

Compare each of the values above with a chi-square of 41.64 at the 1 per cent level of significance for 23 degrees of freedom.

ceeded or fell short of an expected record determined by the composite profile. And, for each activity, we decided whether an excess represented a primitive characteristic or a non-primitive or modern feature. For example, we decided that an excessive concentration on tools and appliances in any cultural record would constitute a mark of primitivism for that culture, whereas an

⁹ Samuel A. Stouffer *et al.*, *Studies in Social Psychology in World War II*, Vol. IV: *Measurement and Prediction* (Princeton: Princeton University Press, 1950), chaps. 1-ix; Matilda White Riley *et al.*, *Sociological Studies in Scale Analysis* (New Brunswick: Rutgers University Press, 1954); Linton C. Freeman and Robert F. Winch, *op. cit.*

TABLE 5

CONTRIBUTIONS OF VARIOUS CULTURE PHASES TO GOODNESS-OF-FIT TEST RESULTS FOR TWENTY CULTURES COMPARED WITH THE COMPOSITE CULTURE PROFILE

	Chi-square
The productive arts:	
Facilities.....	654.18
The productive process.....	2139.20
Food production.....	979.30
Industries.....	1597.53
Transportation.....	1111.07
Value and exchange.....	2035.32
The regulative arts:	
The family.....	3470.45
Social constructs.....	1383.82
Government.....	4509.28
Legal control.....	1095.50
Magic, medicine, and welfare.....	754.65
Socialization and education.....	2302.11
The personal expressive arts:	
The life.....	970.96
Vital experience.....	2723.65
Conduct and display.....	1485.45
Interpersonal relations.....	349.30
Pleasure.....	656.51
Social problems.....	218.10
The institutional expressive arts:	
Communication.....	765.06
War.....	1333.14
Religion.....	20,536.48
Fine arts.....	1852.02
Knowledge.....	1791.35
Total culture.....	1006.63

Compare each of the values above with a chi-square of 36.19 at the 1 per cent level of significance for 19 degrees of freedom.

discovered for other activities. It happened that we made no discordant decisions for the activities ultimately retained in our scale.

Of the seventy-two original categories listed in Table 1, sixteen proved to be scalable at or beyond the 90 per cent level of reproducibility in their distribution throughout the twenty cultures¹⁰ (see Table 6). A consistent distinction can be drawn between the twenty cultures only in terms of

¹⁰ Stouffer *et al.*, *op. cit.*, pp. 91-121.

these sixteen activities. Other activities displayed in the twenty records erratic distributions that failed to fit any general pattern at a reliable level of reproducibility. Thus, such features as social stratification and communication are not safe to use in classifying cultures because their distributions are peculiar. Non-scalable features, since they represent the idiosyncracies of cultures, may be remarkable and yet not helpful in general, systematic culture comparisons.

Primitive emphases on the food quest and on ideas about nature and man, and a modern emphasis on the exchange of goods, scaled at the 85 per cent level of reproducibility. These activities may, in

other culture arrays, help to specify a general pattern of variability of cultures.

In a further scaling analysis we compared the twenty-four culture phases devised for the culture profile of Table 1. Table 7 indicates that seven of these phases proved to be scalable at or beyond the 90 per cent level of reproducibility and that most of the scalable items in Table 6 have found expression in scalable combinations in the composite profile. Moreover, certain profile combinations of OCM categories point to particular aspects of culture—the life (age groups) and religion—that may prove to be useful bases for classifications of cultures. Thus the consolidation of information in the culture profile has pre-

TABLE 6
SCALOGRAM OF SIXTEEN CULTURE CATEGORIES* IN A COMPARISON
OF TWENTY CULTURES WITH THE COMPOSITE PROFILE†

	MODERN CATEGORIES OF CULTURE								PRIMITIVE CATEGORIES OF CULTURE							
	Property	Finance	Government Activities	Exploitative Activities	Agriculture	Political Behavior	The State	Business and Industrial Organization	Death	Eating	Kinship	Reproduction	Adornment	Tools and Appliances	The Family	Sex
Rumania.....	+	+	+	+	+	+	+	+	—	—	—	—	—	—	—	—
Czechoslovakia.....	+	+	+	+	+	+	+	+	—	—	—	—	—	—	—	—
Burma.....	+	+	+	+	+	+	+	+	—	—	—	—	—	—	—	—
Malaya.....		+	+	+	+	+	+	+	—	—	—	—	—	—	—	—
Thailand.....		+	+		+	—	+	+	—	—	—	—	—	—	—	—
Indochina.....		+	+	+	+		+	+	—	—	—	—	—	—	—	—
Soviet Union.....				+	+	+	+	+	—	—	—	—	—	—	—	—
Poland.....							+	+	—	—	—	—	—	—	—	—
China.....	+				+	+			—	—	—	—	—	—	—	—
Tibet.....							+			—	—	—	—	—	—	—
Samoyed.....								+			—	—	—	—	—	—
Marshall Islanders.....	+							+	—		—	—	—	—	—	—
India.....								+			—	—	—	—	—	—
Copper Eskimo.....											—	—	—	—	—	—
Northern Paiute.....											—	—	—	—	—	—
Tasmania.....												—	—	—	—	—
Indonesia.....													—	—	—	—
Thonga.....														—	—	—
Formosan aborigines.....															—	—
Siriono.....																—

* Scaling at the 90 per cent level of reproducibility (two errors) or better.

† Each plus or minus sign represents an excess or deficit in a culture record compared with an expected value determined by the composite profile in Table 1.

served most of the significant pattern of variability among cultures disclosed by Table 6 and has revealed as well how that significant pattern may be expanded.

Compare in Tables 6 and 7 the activities called "primitive" with the activities called "modern." The latter are all confined to

upon a narrow range of cultural interests limited in our profile to the productive and regulative arts. The developmental distinctions among cultures examined here bring to mind both Sir Henry Maine's status-to-contract summary of civilization and Tönnies' *Gemeinschaft-Gesellschaft* principle.

TABLE 7

SCALOGRAM OF SEVEN PRINCIPAL PHASES OF CULTURE* IN A COMPARISON OF TWENTY CULTURES WITH THE COMPOSITE CULTURE PROFILE†

	MODERN PHASES			PRIMITIVE PHASES			
	Industries	Value and Exchange	Government	Religion	The Family	Vital Experience	The Life
Rumania.....	+	+	+	-	-	-	-
Czechoslovakia...	+	+	+	-	-	-	-
Indochina.....	+	+	+	-	-	-	-
Burma.....	+	+	+	-	-	-	-
Malaya.....		+	+	-	-	-	-
Thailand.....		+	+	-	-	-	-
Soviet Union....			+	-	-	-	-
Marshall Islanders		+		-	-	-	-
Poland.....			+	-	-	-	-
Tibet.....				-	-	-	-
China.....		+			-	-	-
Tasmania.....				-		-	-
Copper Eskimo...						-	-
Samoyed.....							-
Northern Paiute..	+						
Siriono.....				-			
India.....							
Indonesia.....							
Formosan aborig-							
ines.....							
Thonga.....							

* Scaling at the 90 per cent level of reproducibility (two errors) or better.

† Each plus or minus sign represents an excess or deficit in a culture record compared with an expected value determined by the composite profile in Table 1.

the productive and the regulative areas of culture. Among the primitive activities in Table 6 only tools and appliances and the family and kinship appear in our classification "the productive and the regulative arts." Primitive emphases generally seem to have been directed toward the most clearly expressive cultural arts. Modernity, as shown by these twenty culture records, appears, in contrast, as a concentration

CULTURE RANKING

Scalogram analysis can result in a ranking of cultures based exclusively on characteristics that show generally consistent and congruent connections and distinctions between cultures. A great advantage of scalogram analysis is its deliberate specification of the proper terms of comparison. But culture rankings in Tables 6 and 7, though derived from systematic scaling, are crude, since they are obtained through abandoning the information on the *extent* of deviations from the pattern of the composite culture profile. If this information is brought back for all scalable features, developmental indexes can be devised that will yield more precise rankings of the cultures. These indexes can show intensities of emphasis on primitive or modern activities.

The scalogram in Table 6 is made up of eight modern activities complemented by eight primitive activities. To return to the information on the extent of emphasis on these features, separate counts can be made of the number of references to primitive and to modern activities in each culture record. These counts, along with the total number of file slips on record for each of the twenty cultures, are listed in Table 8. The simple ratios displayed in Table 8 form elementary indexes of intensity of emphasis on modern or primitive activities among the twenty cultures. It may be noted that the intensity of emphasis on primitive activities seems generally to vary inversely with the intensity of emphasis on modern activities. The indexing of cultures in terms of the extent of reference to primitive or modern matters appears to corroborate the scaling of cultures in terms of these matters.

A general developmental index can be

formed that utilizes information on both primitive and modern emphases. This index (see Table 8) can be treated as a summary ranking of the cultures. Since references to modern categories of culture exceed references to primitive categories by 13 per cent in the composite profile, a 13 per cent correction can be made in the general index to indicate how any of the twenty cultures exceed or fall short of the gen-

pattern of relationship and distinction between cultures. That pattern can be explored more thoroughly by asking how various cultures give special prominence to the primitive and modern culture categories associated through scale analysis. To discover this significant order of prominence and specialization and outline typologies of primitive and modern cultures, further scalogram analysis is indicated.

TABLE 8

COUNTS OF INFORMATION ON PRIMITIVE AND MODERN ACTIVITIES IN THE
TWENTY CULTURES; INDEXES BASED ON THESE COUNTS

Culture	Total Number of File Slips (a)	Primitive Refer- ences* (b)	Modern Refer- ences† (c)	Index of Primi- tivism (b/a) (b/a)	Index of Modern- ity (c/a) (c/a)	General Index (b-c/a) (b-c/a)	Corrected General Index‡ [(b-c/a) - .13]
HRAF Total.....		7.57%	20.56%	.075	.205	.130	.000
Twenty cultures.....	112,834	7360	25706	.065	.228
Rumania.....	1985	15	1105	.008	.557	.549	.419
Czechoslovakia.....	13937	193	4912	.014	.352	.338	.208
Indochina.....	5763	158	1913	.027	.332	.305	.175
Malaya.....	6331	119	2012	.019	.318	.299	.169
Burma.....	4731	83	1499	.018	.317	.299	.169
Thailand.....	9657	363	2784	.038	.288	.250	.120
Soviet Union.....	10219	180	2586	.018	.253	.235	.105
China.....	24495	2207	5240	.090	.214	.124	-.006
Poland.....	4357	171	875	.020	.201	.181	.051
India.....	6232	480	758	.077	.122	.045	-.085
Tibet.....	10215	653	1169	.064	.114	.050	-.080
Marshall Islanders.....	891	162	100	.182	.112	-.070	-.200
Indonesia.....	1858	240	170	.129	.092	-.030	-.160
Samoyed.....	1679	129	129	.077	.077	.000	-.130
Formosan aborigines.....	2183	398	159	.182	.073	-.109	-.239
Thonga.....	3813	818	205	.214	.054	-.160	-.290
Siriono.....	1162	332	34	.286	.029	-.257	-.387
Copper Eskimo.....	1870	296	43	.158	.023	-.135	-.265
Northern Paiute.....	801	165	9	.206	.011	-.195	-.325
Tasmania.....	655	198	4	.302	.006	-.296	-.426

* Outline of Cultural Materials Categories 26, 30, 41, 59, 60, 76, 83, and 84.

† Outline of Cultural Materials Categories 24, 31, 42, 45, 47, 64, 65, and 66.

‡ For comparison with total materials in HRAF.

eral emphasis on modernity of the whole HRAF record. (An index showing this adjustment also appears in Table 8. Eight of the twenty cultures exceed the general level of modernity in the HRAF inventory.)

A TYPOLOGY OF CULTURE

Every culture has its "specialties." But unique and erratically scattered specialties cannot be handled in a general comparison. For the twenty cultures we discovered only sixteen activities co-ordinated in a general

The cultures are ranked in Table 8 according to the simple ratio of the count of references to modern features related to the total number of references for each culture. The eight cultures classified as primitive by the HRAF editors, along with Indonesia and Tibet, make up the least modern half of the array in Table 8. The four eastern European cultures and the modern cultures of Asia fill out the remaining half of the array. The ten more primitive cultures can be compared in terms of their

relative emphases on primitive activities, and the ten more modern cultures can be compared in terms of their relative emphases on modern activities. These comparisons will not concern deviations from expectations derived from the composite profile but, rather, will require new bases for the reckoning of expected frequencies. In a comparison of the ten more primitive cultures, we can note whether counts of references to primitive features exceed or fall short of expected primitive features computed as in a chi-square test for independence between the ten primitive cultures.¹¹ Expected modern frequencies can be computed as in a similar chi-square test for modern cultures, making possible a comparison displaying varieties of modern emphases. This separation of primitive from modern cultures is appropriate to our study not only because of possible differences between observers (noted above) but also because this twofold division was the only classification that was clearly indicated for our small sample of twenty cultures. Tables 9 and 10 present scalograms of these separate treatments of modernity and primitivism.

Two scalograms were prepared for both modern and primitive arrays of cultures. For the modern cultures a primary scaling combines the distribution of five activities in a single scale in which each activity fits at the 90 per cent level of reproducibility (one error) or better (no errors). The remaining activities, along with property, form a secondary scale, meeting the same criterion of reproducibility but requiring a different ordering of the cultures from that in the primary scale. All eight modern activities fit into one or both of the scalograms of modern cultures. Primary and secondary scaling covered seven out of eight primitive activities distributed among the ten primitive cultures. Only the category of death failed to match these

scaling patterns at a high level of reproducibility.

Two poles of modernity are distinguished in Table 9. Malaya represents at one pole a type of modern culture in which governmental activities, finance, exploitative activities, and the state are stressed. India stands at the opposite pole; political behavior, business and industrial organization, property, and agriculture are stressed. Economics and politics are thus mixed at both poles. We cannot say which pole has the greater emphasis on production or regulation, and we cannot deduce from this particular analysis which pole presents a more modern aspect. At best, the scalograms of modern cultures may bear out the influence of colonialism and authority in such a country as Malaya, in contrast to what may be seen as a more independent development of productive and regulative effort in India.

Two poles of primitivism are distinguished in the scalograms of Table 10. The Formosan aborigines stand at one pole with their extreme emphasis on the family, kinship, and adornment. An opposite emphasis is shown by the Siriono on eating, tools and appliances, reproduction, and sex. Again we cannot say which pole represents a more primitive aspect. We get the sense of the elaborateness of primitive dynamisms among the Formosans. And perhaps the record of the Siriono forms a greater show of attention to the culture of survival and immediate human experience. If the two poles of primitivism are to be named, such terms as the "ornate" and the "vitalistic" are descriptive.

Our analysis has ended with a fourfold typology of cultures in which two contrasting sorts of modern cultures are matched by two contrasting sorts of primitive cultures.

To explore the relation between intensity and type of cultural emphasis a final summary can be drawn from Tables 8, 9, and 10. Figure 1 is this summary. Ten modern cultures and ten primitive cultures are arranged in Figure 1 according to their dis-

¹¹ Sidney Siegel, *Nonparametric Statistics for the Behavioral Sciences* (New York: McGraw-Hill Book Co., 1956), pp. 175-79.

plays of extent and type of emphasis. The arrays appear to be different. The Spearman rank correlation between sorts and extents of modern emphases yields the coefficient of .649, significant beyond the 5 per cent acceptance level. The sweep of modern cultures in Figure 1 could well reflect an association between character and extent of modern emphasis. The relationship between extents and kinds of primitive emphasis is not so clear and fails to show a significant correlation. Though more exten-

of primitive cultures. These records were selected from the University of Colorado HRAF simply in accordance with the requirements (1) that each record should exceed a thousand file slips, (2) that the cultures should be widely distributed throughout the world, and (3) that each culture should be classified as primitive by the HRAF editors. These fifteen additional records may be used for testing certain predictions that can be drawn from our study.

Sixteen particular activities that appear

TABLE 9
SCALOGRAMS OF MODERN CULTURES IN TERMS OF EXCESSIVE
EMPHASES ON MODERN ACTIVITIES*

PRIMARY SCALING						SECONDARY SCALING				
	Government	Finance	Political Behavior	Industrial Organization	Property	Exploitative Activities	The State	Agriculture	Property	
Malaya.....	+	+	-	-	-	+	+	-	-	Malaya.....
Thailand.....	+	+	-	-	-	+	+	-	-	Czechoslovakia.....
Indochina.....	+	+	-	-	-	+	+	-	-	Burma.....
Burma.....	+	+	-	-	-	+	+	-	-	Soviet Union.....
Rumania.....	+	+	-	-	-	+	+	-	-	Thailand.....
Czechoslovakia.....	+	+	-	-	-	+	+	-	-	Poland.....
Soviet Union.....	+	+	-	-	-	+	+	-	-	Rumania.....
Poland.....	+	+	-	-	-	+	+	-	-	Indochina.....
China.....	+	+	-	-	-	+	+	-	-	China.....
India.....	+	+	-	-	-	+	+	-	-	India.....

* Each plus or minus sign represents an excess or deficit in a culture record in comparison with an expected value computed as in a chi-square test for independence between the ten modern cultures compared in terms of the eight modern activities stipulated in Table 6.

sive records of primitive cultures may bring improvements, the summary chart leaves us at present with a question: Is modernity to be discussed principally in terms of a general, developmental, and co-ordinate scheme and primitivism, in contrast, approached in terms of a dispersed and disjointed variety of happenings?

SOME TESTED PREDICTIONS

After completion of the study described above, the records of fifteen additional primitive cultures were examined in preparation for the development of a primitive culture profile and an improved typology

to distinguish in a co-ordinate fashion primitive from modern culture records appear in our study. Presumably these activities represent congruent areas of differences between primitive and modern cultures. If the results of this study have general applicability, the fifteen newly collected records should show primitive characteristics in the sixteen activities listed in Table 6. In eight of these activities the records should exceed sizes determined by the general culture profile of Table 1; in eight other activities the records should fall short of expected sizes. Then, making use of the ordering activities in the scalogram

of Table 6, we offer the following predictions: (1) no modern culture aspects will appear in the sixteen activities in the fifteen newly collected records; and (2) to the extent that this prediction fails, most of the errors will appear in accordance with the scalogram ordering of activities indicated in Table 6. Thus we should be less surprised to find in the fifteen new records modern characteristics appearing in the accounts of the family and of sex than in accounts of property, finance, and government activities.

Table 11 shows only eleven prediction errors in 240 opportunities for error. Most of these errors appear on the right of the table. The likelihood of achieving 229 successes in 240 chance trials is infinitesimal—something less than one chance in 5.8 (10^{38}). Our study appears to have some predictive value. Either investigators are strongly inclined to agree on their emphases on primitive features as contrasted with their emphases on modern features or primitive cultures are, in fact, different from modern cultures in ways that are generally and consistently reflected in culture profiles.

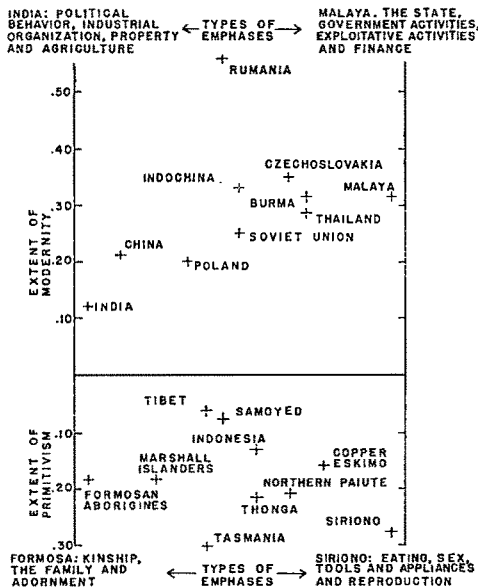


FIG. 1.—Cultures arranged according to extents and sorts of emphases on primitive and modern activities. The indexes of "Modernity" and of "Primitivism" in Table 8 are used here to express extents of modernity and primitivism. The primary and secondary scales in Tables 9 and 10 have contributed to average rankings of cultures by types of emphasis in the figure.

Table 11 gives the results of a test of the above predictions. In situations where our first prediction is supported, no modern culture aspects (M) will appear in Table 11, and instances of failure of this prediction should appear more on the right than on the left in Table 11, following the ordering of activities in the scalogram of Table 6.

COMMENT

Culture comparisons such as we have attempted here will be improved by a refinement of the composite culture profile, by tests of the reliability of observers, and by the inclusion of more cultures in comparisons. Our sample was obviously deficient in historical "high" civilizations and in the urbane cultures of the West. The involvement of Western civilizations in culture comparisons could produce quite different definitions of "modernity" and more elaborate scales than were obtained here.

Freeman and Winch compared forty-eight cultures in terms of the presence or absence of eight significant culture traits. Our study compared fewer cultures in terms of their total records arranged in gross categories of culture. But these studies are essentially complementary. As traits and culture complexes are added to the Freeman and Winch list and as the composite profile is refined to allow for more discrete comparisons, our approaches may coincide. Of course we may have to revise our conclusions: Freeman and Winch pointed to a factor of increasing complexity underlying their scale of cultures; we were more impressed by a narrow range of emphases among modern cultures that could well be regarded as a sort of simplicity.

Naroll achieved a ranking of cultures by the application of a developmental index relating settlement size to craft specialization and organizational ramification. In

TABLE 10
SCALOGRAMS OF PRIMITIVE CULTURES IN TERMS OF EXCESSIVE EMPHASES
ON PRIMITIVE ACTIVITIES*

PRIMARY SCALING						SECONDARY SCALING			
	The Family	Kinship	Eating	Tools	Reproduction		Adornment	Reproduction	Sex
Formosan aborigines.....	+	+	-	-	-	Formosan aborigines.....	+	-	-
Marshall Islanders.....	+	+	-	-	-	Tibet.....	+	-	-
Thonga.....	+	+	-	-	-	Tasmania.....	+	-	-
Indonesia.....			-	-	-	Marshall Islanders.....		-	-
Samoyed.....		+		-	-	Samoyed.....		-	-
Tasmania.....		+		-	-	Northern Paiute.....		-	-
Tibet.....			-	-	-	Copper Eskimo.....		-	-
Northern Paiute.....			-			Indonesia.....		-	-
Copper Eskimo.....						Thonga.....			-
Siriono.....						Siriono.....			-

* Each plus or minus sign represents an excess or deficit in a culture record in comparison with an expected value computed as in a chi-square test for independence between the ten primitive cultures compared in terms of the eight primitive activities stipulated in Table 6.

TABLE 11
RESULTS OF A TEST OF PREDICTIONS ON THE APPEARANCE OF MODERN ASPECTS OF
CULTURE IN SIXTEEN SELECTED ACTIVITIES IN THE RECORDS
OF PRIMITIVE CULTURES

	Property	Finance	Government Activities	Exploitative Activities	Death	Agriculture	Political Behavior	Eating	The State	Kinship	Reproduction	Business and Industrial Organization	Adornment	Tools and Appliances	The Family	Sex
Jivaro.....															M	M
Yakut.....															M	M
Southeast Salish...										M					M	
Koryak.....										M						
Crow.....								M			M					
Chukchee.....											M					
Aranda.....	M															
Tikopia.....	M															
Tiv.....																
Azande.....																
Mbundu.....																
Ojibwa.....																
Tupinamba.....																
Woleai.....																
Aleut.....																

Total failures in prediction (M): 11.

Total successes in prediction (blank): 229; $P < 1/5.8(10^{30})$.

choosing his index, he laid down the following requirements of comparison: adequate documentation, reliability, convenience in the use of the records, cultural freedom, and logical independence. By "logical independence" he means the avoidance of spurious correlations between indicators of development, and by "cultural freedom" he means freedom from the *limitations* of Western or any other special cultural requirements. Categories for classifying data should be equally applicable to all cultures. European cultures, for instance, should not be evaluated in terms of the lack of "true cannibalism," since Europeans are not free from the taboo on human flesh. Generally, the HRAF categories compared in this study have world-wide applicability and in this sense do not violate Naroll's rule of cultural freedom. He would perhaps agree with us, however, in not extending the requirement of cultural freedom to include freedom from external cultural *influences*. Our ready use of the term "development" by no means constitutes a revision of developmentalism. We are more prepared to see development simply as whatever ensues for a culture in consequence of whatever processes are at work upon or within it. And these processes include diffusions and intrusions. Rather than seeking situations of independence from external cultural influences in accordance with strict developmentalist interests, we are inclined to the opposite preoccupation with the interdependence of cultures. The modern cultures in our sample are all marked especially by

Western intrusive forces as well as by impressions upon one another. And yet, though our outlook could fairly easily be expressed at the present time in such traditional terms as diffusion, acculturation, and the like, we feel that diffusionist as against developmentalist arguments are not enough to take into account the larger possibilities in culture comparisons that improved records and techniques have brought about. It must be fairly plain that we are reaching toward an interaction theory of culture, in which development, diffusion, any of the traditional departures, could form critical parts, though not necessarily deciding parts, of understandings of cultures.¹²

If we are to explain the interaction of cultures, it is useful to know in what respects cultures are similar or different. As we view them, culture scales and rankings are no more than assessments of similarities and differences, hinting in some instances at cultural processes. Both similarities and differences present problems in the investigation of cultures. This paper has attempted to locate some of these problems.

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¹² Driver (*op. cit.*, p. 2 *et passim*) deals with processes of development and diffusion as they may be expected to affect correlations of culture trait distributions. He has noted how evolutionary and diffusionist theories need not be mutually exclusive, even in the interpretation of a single society. Driver's integration of functional as well as historical theory with a basic evolutionary treatment is the most comprehensive orientation on culture change yet expressly proposed in connection with the statistical analysis of cultural data.

SELF-ATTITUDES AND DEGREE OF AGREEMENT WITH IMMEDIATE OTHERS¹

CARL J. COUCH

ABSTRACT

This is a study of the relationships between self-attitudes and the degree an individual agrees with his estimate of an immediate other's evaluation when he evaluates his own performance in a small-group situation. Ninety-eight subjects were used. It is found that the tendency is for individuals who identify themselves on the Twenty Statements Test (TST) in terms of group membership rely less upon estimated evaluation of immediate others than do those who do not identify themselves in terms of group membership. It is also found that females rely more than males upon their estimate of immediate other's evaluation.

Turner notes: "For decades sociologists have made reference to 'taking the role of the other' ('role-taking' for short) as a basic explanatory concept in relating the acts of the individual in social contexts of his actions."² In the last few years several investigators have conducted research on the process of role-taking; most such research has been concerned primarily with (1) the accuracy an individual has in inferring the attitudes of another³ or (2) the problem of developing valid measures of role-taking accuracy (empathy).⁴ Most investigators have overlooked or ignored the

fact that in a given social situation an individual directs his behavior not in terms of the actual attitudes of others but on the basis of his estimate or guess of the other's attitudes.⁵ In effect, the individual guides his behavior by his *inferences* of the other's attitudes and not by the actual attitudes of the other. The concern of the research reported here is with an individual's inferences of the other's attitudes.

Humans take the roles of others in many ways, and in most social situations individuals play several roles: "By *role* we mean a collection of patterns of behavior which are thought to constitute a meaningful unit and deemed appropriate to a person occupying a particular position in society (or group)."⁶ One meaningful unit, which appears to be almost universal in group situations in our culture, is that of evaluating each other's performance. Such evaluation is often never explicit, but, nonetheless, one of the things we expect of others is that they are evaluating our behavior. Individuals who are engaged in evaluating each other's performance are, simultaneously, taking the role of each other in an attempt to understand how the others in the situation are evaluating one's own performance. Furthermore, individuals use their guess or estimate of the others' evaluation in directing their behavior in the ongoing processes of social interaction. On the basis of impres-

¹ Revised version of a paper presented at the 1956 annual meeting of the American Sociological Society. This paper is based upon results obtained in a Ph.D. thesis completed at the State University of Iowa, 1955. The author wishes to thank Professor A'Delbert Samson for criticizing the manuscript.

² Ralph H. Turner, "Role-taking, Role Standpoint, and Reference-Group Behavior," *American Journal of Sociology*, LXI (January, 1956), 316.

³ Rosalind F. Dymond, "A Score for the Measurement of Empathic Ability," *Journal of Consulting Psychology*, XIII (1949), 127-33; A. Scodel and P. Mussen, "Social Perceptions of Authoritarians and Nonauthoritarians," *Journal of Abnormal and Social Psychology*, XLVIII (1953), 181-84.

⁴ L. J. Cronback, "Processes Affecting Scores on 'Understanding of Other' and 'Assumed Similarity,'" *Psychological Bulletin*, LII (1955), 177-93; A. H. Hastorf, I. E. Bender, and D. J. Weintraub, "The Influence of Response Patterns on the 'Refined Empathy Score,'" *Journal of Abnormal and Social Psychology*, LI (1955), 341-43.

⁵ An exception to this is Turner, *op. cit.*

⁶ *Ibid.*, p. 316.

sionistic evidence it appears that some individuals rely more than others on their "estimates." A number of variables will influence the extent to which an individual will rely upon estimated evaluations of immediate others, but one of the crucial ones would seem to be the self-attitudes of the individual.

From the point of view of George Mead, one crucial variable in understanding human behavior is the self, for the self is the one variable present in all behavioral situations.⁷ If the self, then, exists in all social situations, and if in most social situations we guide our behavior in part on the basis of what we think others are thinking, it would follow that relationships between the self and the extent an individual relies upon these inferred attitudes would aid in understanding human interaction.

MEASUREMENTS OF THE SELF

One of the difficulties in using the concept "self" in empirical research has been the lack of instruments to measure the self. Recently, Manford Kuhn developed the Twenty Statements Test (or TST), which is designed to measure some aspects of an individual's self-conception by completion of open-ended statements.⁸ It consists of a single sheet which has at the top of the page the following instructions:

There are twenty numbered blanks on the page below. Please write twenty answers to the simple question "Who am I?" in these blanks. Just give twenty different answers to this question. Answer as if you were giving the answers to yourself—not to somebody else. Write your answers in the order that they occur to you, don't worry about logic or "importance." Go along fairly fast; the time is limited.

Who am I?

In this research the TST was administered in a classroom situation with an eight-minute time limit.

It is possible to obtain several dimen-

sions of the self from the TST, the most common being the locus score devised by McPartland.⁹ The locus score is a measure of the extent to which an individual defines himself in consensual terms. It is obtained by dividing responses made on the TST into two categories: a consensual category and a non-consensual category. Consensual statements are those which have reference to groups or categories whose limits or memberships are commonly known (i.e., responses which have a generally agreed upon meaning). Examples of consensual statements are: "I am a student," "The son of my parents," "A Baptist." Non-consensual statements are self-identifications which place the individual in categories the limits and membership of which depend upon the interpretation of the respondent. Examples of non-consensual statements are: "I am happy," "A good husband," "A person who likes girls."

Respondents to the TST tend to make all the consensual responses that they are going to make before they make any non-consensual responses. If the non-consensual statements are combined with no responses, the responses can be divided in such a way as to meet most of the criteria of a Guttman scale.¹⁰ There is a possible range in the locus scores from 0 to 20, with a score of 0 indicating that the subject made no consensual statements or, if he made any, that they were few and were not made as the first response to the question. A score of 20 indicates that all or nearly all of the responses were consensual.

Another set of measures of the self, derived from the TST, are references to a role in, or an attachment to, a major institution of our society. To measure this dimension of the self, the TST's were content-analyzed for responses that had a ref-

⁹ Thomas McPartland, "The Self and Social Structure" (Ph.D. dissertation, State University of Iowa, 1953).

¹⁰ See Kuhn and McPartland (*op. cit.*) for a discussion of the distribution of locus scores and how well responses to the TST fit the Guttman scale. The results in this research were essentially the same as those obtained by Kuhn and McPartland.

⁷ G. H. Mead, *Mind, Self, and Society* (Chicago: University of Chicago Press, 1934).

⁸ Manford Kuhn and Thomas McPartland, "An Empirical Investigation of Self Attitudes," *American Sociological Review*, XIX (1954), 68-75.

erence to religion, the family, or education. This form of content analysis presented little difficulty in categorizing the responses. For religion the only response with some ambiguity was "I am a Jew." This response, which occurred only twice, was categorized as religious, although it may be correct to categorize this response as an ethnic identification. The analysis in terms of a family reference presented only one difficulty: when the individual gave his name as a response, this response was not categorized as being a self-identification in terms of a family reference. The statement, "I am a student," was the only response categorized as a self-identification with the educational institutions. There were other "educational" responses, the most common of these being "I am majoring in ———." But almost invariably the "I am a student" was the first response made having an education reference.

MEASUREMENT OF DEPENDENCY

A standard situation was used to obtain a measure of the extent to which an individual relies upon his estimate of immediate other's evaluation as a basis for self-evaluation of performance. Individuals were scheduled to participate in the experimental situation in groups of two. Ninety-eight subjects (TST protocols were obtained on only 96 of the 98) took part. They were unpaid volunteers from courses in introductory social science. In an attempt to control some of the variables which might be operating, all subjects who took part were matched so that they were interacting with another individual of the same sex whom they had not met prior to the experimental situation. The subjects participated in this part of the study from three to seven days after taking the TST in classrooms.

Upon arriving at the room the subjects were introduced to one another by the author and told that they were to work together on a task and that several other pairs would also be given the same task: "To write a five-minute radio script on why

a course in social science should be required in all college curriculums." They were also told that a reward of five dollars would be given to the couple who did the best job. Each pair was then left alone in the room for fifteen minutes to carry out this task.

After the allotted time they were sent to separate rooms to fill out a questionnaire. This questionnaire had four parts, but only Parts I and III are of concern in this report.¹¹ Part I measured the subject's evaluation of his own performance. Part III measured the subject's estimate or guess of his partner's evaluation of the subject's performance. There were twelve items on each part of the questionnaire; each item on Part III paralleled an item on Part I. For example, on Part I the items read, "I did most of the work"; "I did a poor job"; "I made most of the important contributions." On Part III the parallel items read, "I would guess my partner thinks I did most of the work"; "I would guess my partner thinks I did a poor job." On both parts there were four possible answers to each item, ranging from "Strongly agree" to "Strongly disagree." All items dealt with the evaluation of performance of the actors in the situation.

In summary, Part I measured the subject's evaluation of his own performance, and Part III measured the subject's estimate or guess of his partner's evaluation of the subject's performance. On Part III the individual was engaged in role-taking, but the correctness or incorrectness of his role-taking is not of concern in this report.

To obtain a measure of the extent to which an individual agreed with his estimate of how the other person evaluated him, the responses on Part I of the questionnaire were compared with the responses on Part III. The number of items an individual marked in identical fashion on the two parts was the measure used to indicate the degree to which the individual agreed

¹¹ Part II measured the individual's evaluation of his partner, and Part IV measured the individual's estimate of how the partner evaluated himself.

with his estimate of his partner's evaluation of him when he evaluated himself. In the remainder of the report we shall refer to this as the "agreement score."

It is possible that a high agreement score does not necessarily mean that the individual relied upon or depended upon the estimated opinions of the immediate other as a basis for self-evaluation, since it is possible for a high agreement score to be obtained by an individual (A) projecting to the other (B) the evaluation of himself (A) that he has assigned himself (A) without engaging in role-taking. But there was a correlation of .33 between accuracy of role-taking and the extent to which an individual agreed with his estimate of how his partner evaluated him.¹² And, if the high agreement scores were the result of projection, there should have been negative correlation between these two variables, for it seems that the type of individual who would project into others his own self-evaluation would not engage as extensively in role-taking and would therefore be less accurate in his role-taking.

HYPOTHESES

Most consensual statements on the TST have reference to social groups in which the individual feels that he has membership. Therefore, the higher the locus score, the more the individual conceives of himself in terms of having membership in different social groups. It seems reasonable that, the more groups in which an individual feels he has membership, the more social anchorage he has and the less he will be influenced by the estimated attitudes of immediate others. On this basis the first hypothesis is: (1) Individuals with low locus scores will rely more than individuals with high locus scores on the estimated evaluation of immediate others in the self-evaluation of their own performance.

In line with the above, the second hypothesis is: (2) Individuals who make a

¹² Carl J. Couch, "A Study of the Relationships between Self-views and Role-taking Accuracy" (Ph.D. dissertation, State University of Iowa, 1955).

self-identification in terms of a reference to a major institution rely less than do individuals without such a self-identification upon their estimation of others' evaluation of them. This hypothesis must be modified in accordance with the situation in which the interaction takes place. For example, if the interaction takes place within a religious setting with people of the same religious group that the individual has indicated he feels he has membership in, then it would be expected that such an individual would rely more on his estimate of these others' evaluations than would those who do not identify with such a group. In this study the subjects were students interacting with other students within an educational setting; therefore, the hypotheses in regard to self-identification in terms of a major institution are: (2a) Individuals with a self-identification in terms of a religious reference will rely *less* than those without a self-identification by means of a religious reference upon their estimate of immediate others' evaluation. (2b) Individuals with a self-identification in terms of a family reference will rely *less* than those without a self-identification by means of a family reference on their estimate of the immediate others' evaluation. (2c) Individuals with a self-identification in terms of a student reference will rely *more* than those without a self-identification by means of a student reference upon their estimate of the immediate others' evaluation.

A third general hypothesis was: (3) Females will rely more than males upon their estimate of the immediate others' evaluation. Our culture, we felt, quite definitely teaches females to be more dependent than males upon immediate others.

RESULTS

In operational terms hypothesis (1) is as follows: Low locus scores will be associated with high agreement scores. This hypothesis was supported to a certain extent by the empirical data. The mean agreement score for those individuals with a locus score below 4 was 9.9, while the mean

agreement score for those with a locus score of 4 or above was 8.5. Using the two-tailed *t*-test, this difference is significant at the .01 level. Before any research was conducted, it was expected that there would tend to be a lineal relationship between these two variables; this was not borne out by the evidence, however. Individuals with low locus scores (below 4) tend almost universally to obtain high agreement scores; individuals with high locus scores (4 or above) may or may not have low agreement scores. Also, there was little if any difference between the agreement scores obtained by individuals with locus scores between 4 and 10 and those who had scores above 10. On the basis of this evidence, it appears that the individual with a median number of consensual responses on the TST is as little influenced by the estimated evaluation of immediate others as is the individual with a large number of consensual statements.

The second hypothesis dealt with the relationship between attachment to major institutions and the agreement score. Specifically, hypothesis (2a) indicated that individuals who made a self-identification on the TST in terms of a religious reference would have lower agreement scores than those who failed to make such a reference. Forty-seven of the 96 subjects who completed the TST made a religious reference. The mean agreement score for those who made a religious reference as a means of self-identification was 8.3; for those who did not, the mean agreement score was 9.3. This difference is significant at the .03 level, supporting the hypothesis.

In an attempt to refine the measure of religiosity further, a saliency score was assigned to those who made a religious reference. A saliency score of 1, the highest possible, indicates that the first statement on the TST was a religious reference. It was expected that those with high saliency would have lower agreement scores than those with low saliency. This hypothesis was not supported; in fact, individuals with high saliency of religious reference

had higher agreement scores than did those who made a religious reference but who made it after the first three responses. The difference, then, was in the unexpected direction, but it was not significant.

Another measure of religiosity was obtained by dividing those subjects who made a religious statement into two groups. The first group contained those who identified themselves in terms of a general reference to religion or religious beliefs without specifying the sect. Examples of this type are: "I am a Protestant"; "A Sunday-school teacher"; "A believer in God"; etc. The second group contained those who identified themselves as being members of a particular sect (e.g., "I am a Catholic"; "A Methodist"; etc.) as well as those who identified themselves as "A subject of God"; "A strict religious person"; "A divine being"; etc. The hypothesis was: Those subjects with the latter or more "intense" type of religious identification would obtain lower agreement scores than the other type. The mean score for this "intense" type was 7.7, while the other type had a mean score of 8.9. While the difference is in the expected direction and fairly marked, it is not significant at the .05 level.

Hypothesis (2b) states that individuals who made a self-identification by means of a statement which had reference to the individual's family would have lower agreement scores than those who failed to make such a reference on the TST. Fifty-two of the subjects made a family reference. The mean agreement score for those with a family reference was 8.8, while those with no family reference had a mean agreement score of 9.0. While this difference is in the expected direction, it is so slight that it does not approach significance. Considering only those in the sample who made a family reference, there was a tendency for those with high saliency of family reference to obtain low agreement scores. For those individuals who had a family reference on one of the first three responses, the mean agreement score was 8.1. The mean score for those who made a family reference on the

fourth or later response was 9.1. This difference is significant at the .05 level.

Hypothesis (2c) states that individuals who identified themselves by reference to the student role would obtain higher agreement scores than those who did not. As 92 of the 96 subjects identified themselves by the statement, "I am a student," no meaningful comparison of the two groups could be made; but a comparison of the scores made by subjects who identified saliently as a student with those that identified themselves as students *after* the first three responses shows that individuals with a salient self-identification as students had a mean score of 9.0 compared to 8.5 for those who did not. This difference in the expected direction is not significant.

Hypothesis (3) theorizes that males would have a lower agreement score than females. The results supported the hypothesis, since the mean score for females was 9.6 and for males 8.2; this difference is significant at better than the .01 level. Of the 98 subjects, 2 did not complete the TST; of the other 96, 3 failed to make a self-identification by a sex reference such as, "I am a male"; "Boy"; "Woman"; etc. If these 5 are dropped from the sample, the difference is still significant at the .01 level. Following the same line of reasoning, it would be expected that males who made a sex identification saliently would have lower agreement scores than those who did not. Males who identified themselves as males on one of the first three responses had an average score of 7.2, compared to an average score of 8.6 for the other males. This difference is significant at the .04 level. Similarly, it should follow that females who identified themselves saliently in terms of sex should have higher agreement scores than other females. This was not borne out by the empirical evidence; those females who identified themselves by a sex reference on one of the first three responses had a mean score of 9.7, compared to a mean score of 10.3 for the other females. This

difference is not significant. It might also be noted that twenty-nine of the 40 females made a reference to sex on one of the first three responses, compared to 29 of 56 males, indicating, perhaps, that self-identification in terms of sex is of greater importance in our culture for females than it is for males.

SUMMARY

This research demonstrates that (1) an individual with a low locus score (0-3) agreed more with his estimate of others' evaluation of him when he evaluated his own performance than did an individual with median or high locus scores; (2) an individual with self-identification in terms of a reference to religious institutions agreed less than an individual who did not give this type of self-identification in response to the TST; (3) an individual who saliently identifies himself in terms of a family reference agreed less than the individual who identified himself in this manner but did not do it saliently; and (4) females, when evaluating themselves, tend to agree more with their estimate of their partner's evaluation than males.

It must be remembered that the relationships obtained were obtained in a particular social situation and that, while it is felt that there are systematic relationships between self-attitudes and reliance upon estimated evaluation of others, the form this relationship will take is also related to the particular group structure and setting in which the interaction takes place. In this research not all the expected relationships were significant at the usually accepted level of confidence, but nearly all were in the expected direction. Taken together, they support the general contention that measurements of self-attitudes are predictive and that with further refinements measures of self-attitudes hold considerable promise of predicting social behavior.

MONTANA STATE COLLEGE

SOCIAL STRUCTURE AND INTERPERSONAL COMMUNICATION

OTTO N. LARSEN AND RICHARD J. HILL

ABSTRACT

The flow of information is related to the changing structure of group relations in two studies of small communities consisting of boys in a summer camp. In these situations message "starters" were selected according to their position in the social structure. The findings suggest a number of ways in which the spread of information may be guided by considerations of social status.

The process of sharing information is an integral part of social interaction. Social psychologists have suggested that, when a number of people come together for the first time, the communicative process tends to reflect influences immediate to the situation, the task, and the personal characteristics of the interacting individuals. No particular communication *pattern* emerges. However, as groups develop a stabilized structure, it is believed that the lines of communication become increasingly predictable in terms of the reciprocal relationships of the members. In discussing this process, Sherif contrasts communication in "transitory togetherness situations" with that in "stabilized group structures," noting that the "formation of a status hierarchy tends to polarize communication in the direction of the upper status levels."¹ Such work as that of Bales and associates,² Festinger and Hutte,³ and Hurwitz and associates⁴ also provides evidence of relationships between status organization and communication patterns.

¹ Muzafer Sherif, *An Outline of Social Psychology* (New York: Harper & Bros., 1956), p. 226.

² Robert F. Bales *et al.*, "Channels of Communication in Small Groups," *American Sociological Review*, XVI (August, 1951), 461-68.

³ Leon Festinger and H. Hutte, "An Experimental Investigation of the Effect of Unstable Interpersonal Relations in a Group," *Journal of Abnormal and Social Psychology*, XLIX (1954), 513-22.

⁴ J. R. Hurwitz *et al.*, "Some Effects of Power on the Relations among Group Members," in D. Cartwright and A. Zander (eds.), *Group Dynamics* (Evanston, Ill.: Row, Peterson & Co., 1953), pp. 483-92.

The present paper reports an exploratory analysis of the relationship between a changing social structure and the communication of a message. The central problem is the attempt to determine to what degree the communication network was related to the sociometric hierarchy as both structures developed in natural social settings.

OBSERVATIONAL SETTING

The following analysis is concerned with the person-to-person diffusion of a single message through populations of boys between the ages of twelve and fifteen who were attending a summer camp. Each ten-day camp period was devoted to boys from a different YMCA area. Thus there was a complete population change every eleven days, resulting in five different camper populations. The first, third, and fifth camper groups were selected as the "experimental" populations. However, owing to administrative difficulties, complete sociometric data were not obtained for the fifth group; therefore, the analysis was based on the first population (Study I, $N = 43$) and the third population (Study II, $N = 60$).

A sociometric questionnaire was administered to the populations one day after their arrival at the camp. From this questionnaire the social structure of the population was determined in terms of expressed preferences for residence, leadership, and intimate associates. On the basis of this information, certain boys were selected to act as message "starters." In each study the starters included a sociometric star, an isolate, and an individual of the median sociometric position.

The starters were assembled, and the camp director gave each a yellow celluloid pin on which was printed in black block lettering a large question mark and the name of the camp. The director told the starters the following:

Here is a special pin. It is important that you wear it today. We have a few more of them but not enough for all the campers. Those campers who find out about this and come to the "Old Lodge" will also get a pin as long as our supply lasts. Remember, it is important that you wear it today, and remember that we don't have very many pins.

When a camper learned of the pins and came in search of one, he was asked who told him about the pins and what he had been told. All conversations between the boys and the observers took place in an informal setting and were tape-recorded. At the close of each conversation the boy was given a pin without further explanation of its purpose, and he, in turn, became an additional possible source of information.

A second sociometric questionnaire, asking the same questions as the first but posed around the possibility of returning to camp the following year, was administered on the day following the message-diffusion situation.

With the diffusion of the message considered the "experimental" situation, the design called for the following sequence of procedures: (a) administration of a pre-experimental sociometric questionnaire on the second day of the camp period; (b) selection of message starters on the basis of this sociometric information; (c) observation of the communication pattern on the eighth day of the camp period; and (d) administration of a postexperimental sociometric questionnaire on the ninth day.

INITIAL STRUCTURE OF THE TEST COMMUNITIES

The basic social ingredients of the two communities were identical in certain respects but quite different in others. Both camp periods were organized around the

same set of tasks, which were regulated by a common set of rules enforced by the same formal machinery. Furthermore, these all-male populations were relatively comparable in terms of age, ethnicity, religious affiliation, and socioeconomic background. Thus, any contrast noted in the behavior of the two communities is not likely to be accounted for in terms of differences in social background or general community function. However, some differences did exist in terms of population size, number of residential subgroupings, previous acquaintance of the members, and certain sociometric interconnections.

Each boy was given a score on each of the following indexes:⁵

1. *Previous acquaintance.* The number of boys named in response to the question, "Name the campers whom you knew *before* coming to camp."
2. *Sociability.* The number of choices made in response to the question, "Please name the seven boys out of the whole camp whom you would *most* like to have as cabin mates."
3. *Popularity.* The number of choices received in response to Question 2 above.
4. *Prestige.* The number of choices received to the following two questions: (a) "What boy would you *most* like to have represent your cabin on the camp council?" (b) "Please name the two campers in your cabin that you would *most* like to sit next to at campfire."

All these questions were formulated around camp activities that were meaningful to the boys. However, a boy's score on previous acquaintance, sociability, and popularity could be a product of community-wide scanning, while the prestige score had a narrower social base, since choices were limited to cabin mates.

What were the major points of contrast between the two communities? The difference in the mere *number* of associated indi-

⁵ The details concerning definitions, scoring, and intercorrelational analysis of the following may be found in Otto N. Larsen, "The Mechanics and Effects of Direct and Socially Mediated Channels of Contact in Message Diffusion" (unpublished Ph.D. dissertation, University of Washington, 1955).

vidual affords one distinction. The Study I community included 43 boys organized into six residential subgroups, while Study II involved 60 boys in eight residential groupings. Not only was the Study II community larger but it had less basis for integration in terms of *previous acquaintance*. The 43 boys in Study I knew, on the average, significantly more of their peers before coming to camp than did the 60 boys in Study II (2.8 versus 1.4).

The pattern of previous acquaintance apparently was the basis for other differences in the initial sociometric structures of the two communities. In Study I the correlation between *previous acquaintance* and *popularity* was .83. In Study II these two measures were practically uncorrelated ($r = .03$). Thus the initial structure of the smaller community seems to have been more influenced by social relations developed prior to the camp period than was that of the larger community.

Another difference was found in terms of the relationship between *sociability* and *popularity*. The correlation between these two measures in Study I was .41. In Study II the correlation was .12. This contrast is interpreted as a further indication that the smaller community, with its more extensive previous acquaintance relationship, was initially organized around a more structured set of reciprocities than was the case for the Study II community. The latter community is seen as beginning its camp life under circumstances more nearly approximating the "transitory togetherness situation" described by Sherif.

THE CHANGING SOCIOMETRIC STRUCTURES

Both populations evidenced an increase in *sociability* during their respective camp periods, but the over-all increase was greater in the small community than in the larger one. Thus, there was a 39 per cent increase in the number of choices made in Study I and a 26 per cent increase in Study II. The correlation between *sociability* scores at the opening and closing of camp

was .50 in Study I and .34 in Study II—a finding that may be interpreted as reflecting the greater initial structuring of the smaller community and may also be in part a function of the larger size of the Study II community.

The increases in sociability had the net effect of making certain aspects of the two sociometric structures more alike at the close of the camp periods than at the beginning. For example, the mean *sociability* scores increased in Study I from 3.3 to 5.3, while the increase in Study II was from 4.0 to 5.0. The same equalizing trend was noted in the *prestige* scores. The increase in the mean number of choices made in Study I was from 2.3 to 2.5, while in Study II the increase was from 1.8 to 2.3. Such data suggest that conditions linked with time in the common social milieu had an equalizing influence on the structures of the two communities. In other words, we may hypothesize that newly formed groups, different in original composition with regard to size and previous association, confronted with a similar functional environment, tend in the course of time to develop relatively similar patterns of structural relationships.

The findings of the sociometric analysis raise certain problems when we turn to the consideration of the communication patterns that were observed. Originally, we desired comparable situations within which the communication study could be replicated. Theoretically, the findings suggesting that the Study I community was more structured at the beginning of its camp life than the Study II community would lead us to predict that the influence of structure on the communication network would be greater in Study I. However, the findings also suggested that the two communities became increasingly similar as a result of the camp experience. From this the following predictions may be deduced: (1) there should be similarities in the communication patterns of the two communities and (2), where differences occur, they will be in a direction which indicates a stronger

influence of social structure in the Study I situation.

COMPARATIVE PATTERNS OF DIFFUSION

Figures 1 and 2 depict the channels of information that developed in the two studies by showing how the information spread from the starters. The numbers in the squares merely identify a given boy within a cabin.

In Study I the low-status starter in Cabin B told the message to four persons, who, in turn, told thirteen others. In all, then, 50 per cent of the total diffusion was linked to the low-status starter. The middle-status starter in Cabin A also told the message to four persons, and this led to twelve additional tellings (47 per cent of the total diffusion is thus linked to the middle-status starter). The high-status starter in Cabin C passed the message to only one person, with no further transmissions stemming from the latter individual. Furthermore, of the six boys who did not indicate that they received the message, four lived in the cabin with the high-status starter.

Five message starters were selected in Study II.⁶ The low-status starter in Cabin A told the message to two other boys, but no further tellings resulted. The low-status starter from Cabin G also told two other boys, but these two boys transmitted the message to eight others. In total, 24 per cent of the diffusion can be linked to the low-status starters. The single middle-status starter (Cabin H) told five boys (10 per cent of the total diffusion). The high-status starter in Cabin C told the message to three boys, with no further transmissions. The other high-status starter (Cabin E) also told the message to three boys. However, from this start there followed a

considerable chain reaction—thirty of the total of fifty transmissions being linked to this one high-status starter. By reference to Figure 2, it may be seen that the lines of communication originating from the high-status starter in Cabin E extend into all the residential subgroups of the camp community. A paradoxical aspect of this pattern is that three of the total of five boys who did not indicate knowledge of the message lived in the cabin with this high-status starter. Thus in both studies, while there were only a few non-participants, the majority of them lived in a cabin with the high-status starters. In Study II, however, 66 per cent of the total diffusion can be linked to the two high-status starters.

Were these communication patterns related to or influenced by the sociometric patterns that had been previously measured? To answer this question a procedure was employed to test the null hypothesis that sociometric structure has *no relationship* to communication patterns. If the null hypothesis is true, then the probability of individual A telling the message to individual B, with whom he was linked sociometrically, would be the same as the probability of his telling C, with whom he was not linked sociometrically. If this is the case, then the expected proportion of the total telling that would occur between sociometrically linked pairs is the ratio of the number of possible teller-hearer pairs characterized by sociometric linkage to the total number of possible interacting pairs. The proportions so obtained are the "expected" proportions of telling between sociometrically linked pairs, assuming the null hypothesis is true. These expected proportions can be compared to the observed proportions of telling that occurred between members who were sociometrically linked. If the observed proportion of telling is significantly larger than the expected proportion, then it can be concluded that the influence of the particular variable being considered resulted in a significant departure from the null condition. In other words, the question that is being asked is: "Is it more

⁶ In order to meet certain requirements for the study of time factors, the number of boys selected as starters varied in the two studies, but the percentage of the population selected was relatively constant—7-8 per cent. For the detailed analysis of the time factors operative in these studies see Richard J. Hill, "Temporal Aspects of Person-to-Person Message Diffusion" (unpublished Ph.D. dissertation, University of Washington, 1955).

probable for an individual to tell a message to a person he sociometrically prefers than it is for him to tell a message to any chance individual?" The results of this analysis are shown in Table 1.

On the basis of this analysis, it can be concluded that potential communication channels were more likely to become active

if there were sociometric linkages between communicating individuals than if no such linkages existed.⁷ Having established this,

⁷ It would have been desirable to analyze certain compound relationships (e.g., between telling and preference as both cabin mate and leader). However, the joint probabilities involved became so small as to make such analysis unwarranted.

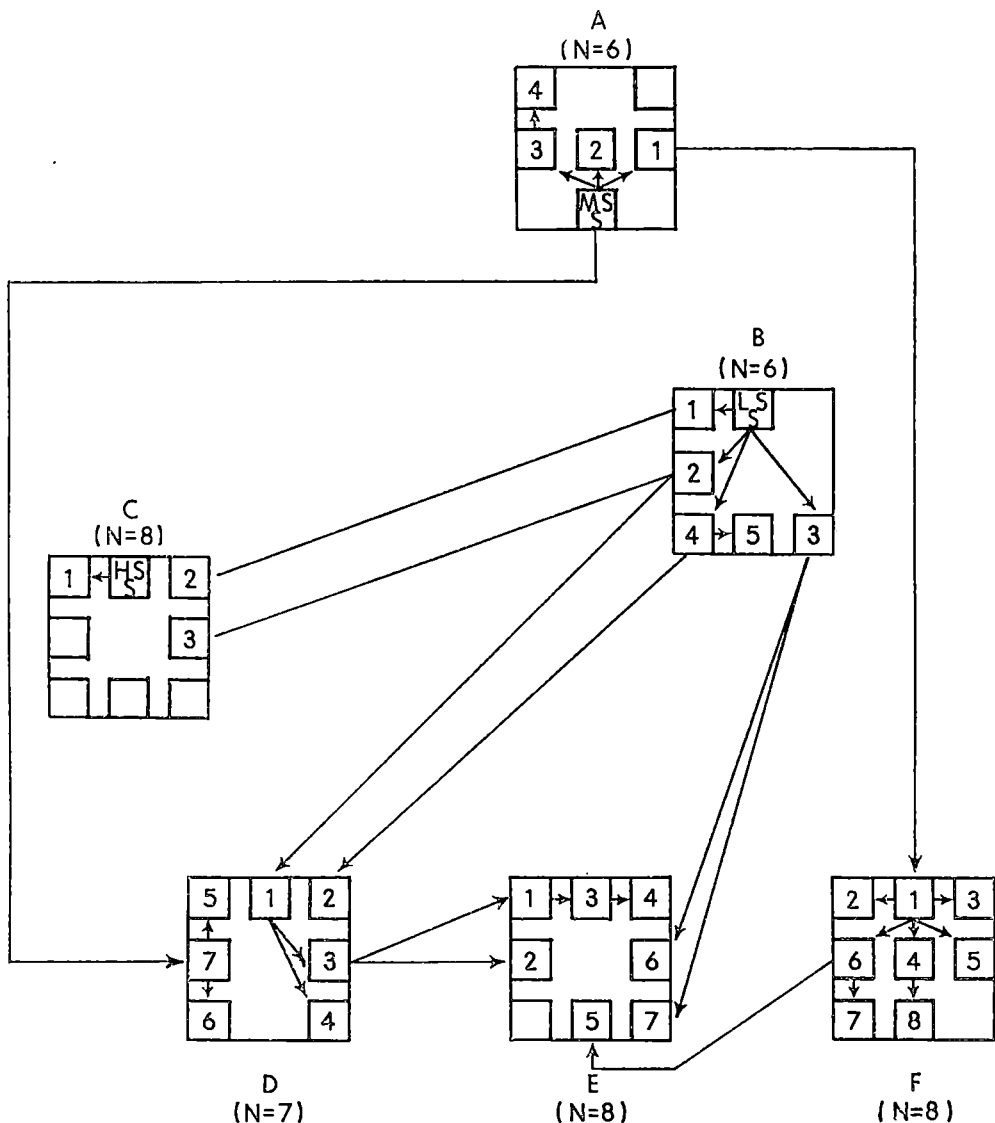


FIG. 1.—Message diffusion within and between groups, Study I. Legend: L.S.S. = Low-Status Starter; M.S.S. = Middle-Status Starter; H.S.S. = High-Status Starter; blank squares = non-participants. Total $N = 43$. Groups A through F also represent cabin residences placed in their relative physical camp locations.

we can now turn to the comparison of the two communication patterns.

The general patterns of diffusion in the two studies were similar in terms of the *amount of diffusion* and the *average number of tellings per transmitter*. In Study I, 85 per cent of the total non-starter popula-

tion heard the message and complied by calling for a pin and being interviewed. In Study II the corresponding figure was 90.9 per cent. The difference between these two percentages is not significant.⁸

⁸ In discussion of significance the .05 level has been used throughout this paper.

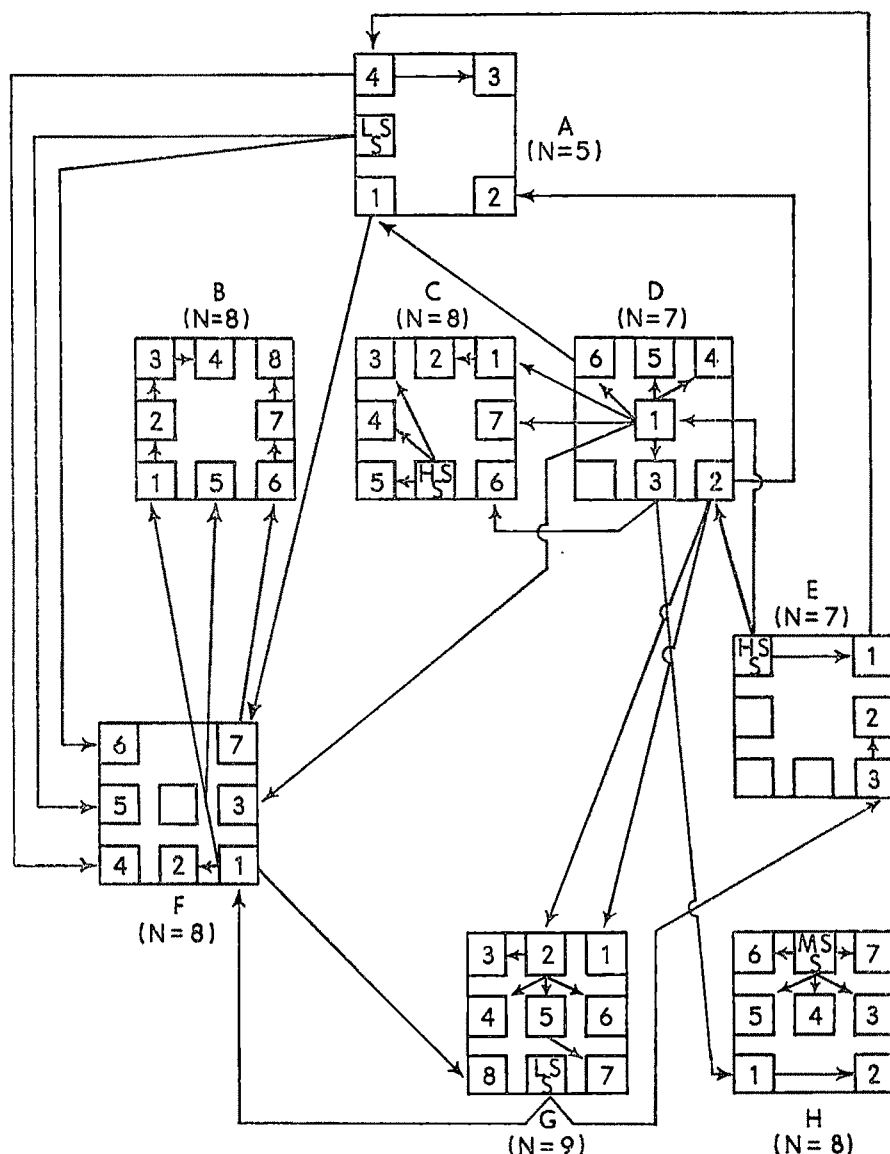


FIG. 2.—Message diffusion within and between groups, Study II. Legend: L.S.S. = Low-Status Starter; M.S.S. = Middle-Status Starter; H.S.S. = High-Status Starter; blank squares = non-participants. Total $N = 60$. Groups A through H also represent cabin residences placed in their relative physical camp locations.

In Study I, 17 transmitters told the message a total of 34 times, for an average of 2.0 tellings per transmitter. In Study II, the average was approximately the same, with 24 transmitters telling the message a total of 50 times, or 2.08 tellings per transmitter.

Another similarity in the two over-all diffusion patterns can be seen by comparing the relative number of persons in the various communication roles.⁹ Table 2 shows the distribution of communication roles in the two studies.

all organized camp activity. Within this period, however, the boys reacted to the message somewhat more rapidly in Study II than in Study I. In Table 3 the growth processes are shown in terms of the cumulative percentage of knowers who complied with the message by fifteen-minute intervals during the two-hour diffusion experience. About 8 per cent of the boys complied within the first fifteen minutes in both studies, but then the greatest difference between the growth rates of the two studies occurred. By the end of the first

TABLE 1
INFLUENCE OF SOCIOMETRIC VARIABLES ON MESSAGE DIFFUSION

SOCIOMETRIC VARIABLE	PROPORTION BOTH NAMING AND TELLING			
	Study I		Study II	
	Expected	Observed	Expected	Observed
Residential preference.....	.088	.382*	.130	.375*
Intimate associate.....	.040	.118*	.036	.146*
Leadership preference.....	.021	.029	.021	.062*
Previous acquaintance.....	.077	.265*	.048	.083

* Observed proportion significantly larger than the expected proportion at the .05 level.

Another aspect of the community structure was the fact of common residence or physical propinquity. To get some indication of how this factor might have operated, the message was defined as an "in-group" telling when it was transmitted between two boys from the same residential subgroup. Conversely, when the transmission involved boys from different cabins, it was called an "out-group" telling. If the relationship of this aspect of structure to the communication network was greater in Study I than in Study II, we would hypothesize more in-group transmission in the Study I instance. While the percentage of in-group telling was greater in Study I (67.6 per cent) than in Study II (56.0 per cent), the difference was not statistically significant.

The analysis of the time-growth patterns shows that the message moved at somewhat different rates through the two populations. In both studies the diffusion took place within a two-hour period which had been set aside by the camp officials, at the request of the researchers, as free from

half-hour, 24.3 per cent of the ultimate compliers responded in Study I, while 45.6 per cent had responded in Study II.

The earlier or more rapid spread of the message in Study II can be interpreted in

TABLE 2
DISTRIBUTION OF COMMUNICATION ROLES*

ROLE	STUDY I		STUDY II	
	N	Per Cent	N	Per Cent
Transmitter...	17	39.5	24	40.0
Non-participant.....	6	14.0	5	8.3
Receiver.....	20	46.5	31	51.7
Total.....	43	100.0	60	100.0

* At the .05 level there are no significant differences between the two studies in the percentages of persons occupying corresponding communication roles.

terms of the structural conditions previously discussed. To the degree that status

⁹ These roles were defined as follows: *transmitters*—persons who heard the message, complied by requesting a pin, and then went out and told others; *receivers*—persons who heard the message and complied by requesting a pin but who did not transmit the message further; *non-participants*—persons who did not comply by requesting a pin and who were not credited with telling anyone the message.

considerations tend to inhibit the use of certain interpersonal communication channels, the stronger these considerations, the less rapid would be the spread of information. In the communication situations here reported, the spread of information probably would have been most rapid if information was passed on every time a knower met a non-knower. To the degree that status considerations prevented certain potential channels from becoming active, the communication process would have been slowed down. If this was the case, then we would expect the information to have spread less rapidly in the more firmly structured Study I community.

TABLE 3

CUMULATIVE PERCENTAGE OF COMPLIANCE WITH THE MESSAGE

ELAPSED TIME IN 15-MINUTE INTERVALS	CUMULATIVE PERCENTAGE Study I	Study II
1.....	8.1	8.8
2.....	24.3	45.6
3.....	48.6	56.1
4.....	70.2	71.1
5.....	83.7	85.9
6.....	91.8	91.2
7.....	97.2	94.7
8.....	100.0	100.0

In an attempt to determine the *direction* of the flow of information through the two status structures, the comparative scores of the teller-hearer pairs were examined. Table 4 presents the data on the flow of information through the *popularity* structure. Only in Study I was the flow of information in the direction consistent with the Sherif conclusion discussed in the opening paragraph of this paper. This, of course, is consistent with the interpretation that Study I had a more mature social structure than did Study II. However, existing theory does not seem to explain the tendency toward a "down-status polarity" of the communication pattern observed in Study II. In an attempt to explain this discrepancy, the data were examined from the point of view of the *changes* that took place in the status of the teller-hearer pairs between the opening and closing of the camp. To what extent was the information

passed between persons whose status changed in the same direction, either upward or downward?

In Study I, 61.7 per cent of the telling took place between persons whose popularity status *changed in the same direction*. In Study II the corresponding figure was 48 per cent. Thus, when the transmitter-receiver relationship is evaluated in terms of *social mobility* or change in status rather than in terms of status at a given point of time, it decreases the observed degree of differentiation in interaction or increases the observed degree of like-status persons interacting with each other. We hypothesize that this is a more valid estimate of the manner in which status considerations guide the passing of valued bits of information. That is, the individual is conscious

TABLE 4

THE POPULARITY STATUS RELATIONSHIP
BETWEEN HEARERS AND TELLERS
IN BOTH STUDIES*

DIRECTION OF THE TELLINGS	STUDY I		STUDY II	
	N	Per Cent	N	Per Cent
Up status....	19	55.9	16	32.0
Same status...	8	23.5	6	12.0
Down status..	7	20.6	28	56.0
Total.....	34	100.0	50	100.0

* For 2 df, $\chi^2 = 10.4$, $P < .01$.

of the dynamic nature of status in this kind of setting, and perhaps the emphasis is not on what a potential receiver's status is at a given point of time but on where the transmitter perceives the receiver to be moving, in terms of status, relative to the transmitter's own status in the community.

How do occupants of key communication roles perceive their own status in reference to their communicative activity? A final examination of another aspect of social mobility in both communities produced a regularity suggesting a hypothesis regarding this question. While the direction of the information flow through the status structures of the two communities was not consistent (Table 4), a consistency in status disposition was found for the transmitters and receivers in the two studies. When *changes* were compared in the mean socia-

bility and popularity scores for transmitters as a class and receivers as a class, the trend was for the status of transmitters to *rise* while receivers *fell* in status accorded by their peers. At the same time transmitters decreased, while receivers increased, their sociability. Thus, in both studies, transmitters gained in popularity while decreasing in sociability; receivers lost in popularity while increasing in sociability. Possibly the telling of the message was a valuable enough social act to enhance differentially the status of the initiating agent—the transmitter. The hypothesis is suggested that transmitters—the key functionaries in a communication network—perceive the passing-on of valued information as an effective avenue for securing community-wide status recognition.

CONCLUSION

The findings of the present study are seen as consistent with the generalization

that, the more stable and firm the social structure of a community, the greater the influence of that structure on interpersonal communication between community members. Certain differences noted in the communication patterns studied are consistent with this general relationship. However, the data also suggest that the dynamic or developmental aspects of status must be considered in addition to the more static structural factors if a thorough understanding of the relationship between communication and status is to be achieved. Further, the issue of the perceived importance of the communicative act has been raised. Additional research is needed in order to determine the relative importance of status position, status change, and status perception in determining the communication network operative in a given community.

UNIVERSITY OF WASHINGTON
UNIVERSITY OF CALIFORNIA AT LOS ANGELES

THE IMAGE OF THE SCIENTIST IN SCIENCE FICTION

A CONTENT ANALYSIS¹

WALTER HIRSCH

ABSTRACT

A random sample of stories published between 1926 and 1950 in American science-fiction magazines was subjected to content analysis. Scientists comprised the major category of both heroes and villains, but businessmen were, proportionally, more villainous than scientists. The number of social scientists was negligible. The social role of the scientist, initially conceived in terms of saving humanity, becomes increasingly problematical through time; particularly after World War II, one finds the function of solving human problems performed by non-humans. Included is discussion of the possible effects of the perception of the content by the readers of science fiction.

In recent years sociologists have become increasingly concerned with the nature of "mass" or "popular" culture.² Paradoxically, in a "scientific age," science fiction has been neglected as a field of systematic study by sociologists and has been left largely to literary critics. The present paper aims to fill part of the gap between the stimulating but impressionistic approach of the humanist and the requirements of reliable knowledge which is the aim of the method of content analysis.

Science fiction should be of special interest to the sociologist. It concerns one of the basic focuses of contemporary culture—science—and it may serve as a vehicle for social criticism and for the construction of social utopias and counterutopias. In this paper we shall confine ourselves largely to a discussion of the image of the scientist presented in the genre and to the fluctuations of this image during the period 1926–

50. Apart from its intrinsic interest, our analysis also has a "practical" relevance. There has been increasing concern about the shortage of scientists and engineers in the United States, especially vis-à-vis the apparent success of the Soviet Union in this field. Several recent studies of high-school students indicate that their attitudes toward a scientific career are often negative and their views of scientific work unrealistic.³ The sources of these attitudes are still largely unexplored, but it is plausible that the reading of science fiction is such a source. Thus, Isaac Asimov, who is both a biochemist and a prolific science-fiction author, in an article entitled "The By-Products of Science Fiction" asserts:

[In this genre] science and intelligence are . . . represented sympathetically. Scientific research is presented, almost invariably, as an exciting and thrilling process, its usual ends are both good in themselves and good for mankind; its heroes are intelligent people to be admired and respected.⁴

According to Asimov, the results of this will be that the readers of science fiction will be motivated to begin scientific ca-

¹ Expanded version of papers read at the annual meetings of the Ohio Valley Sociological Society, April, 1957, and the American Sociological Society, August, 1957. The writer is indebted to Dr. Louis Schneider, Department of Sociology, Purdue University, for having suggested this study and for a number of hypotheses; to Mr. James Norton, of the Purdue University Statistical Laboratory, for aid in the sampling procedure and statistical analysis; and to Dr. Hanna Meissner and Lotte Hirsch for participating in the reliability analysis.

² See, e.g., the May, 1957, issue of the *American Journal of Sociology*, "The Uses of Leisure," and Bernard Rosenberg and David M. White, *Mass Culture* (Glencoe, Ill.: Free Press, 1957).

³ See Purdue Opinion Panel Poll No. 45, "Physical Science Aptitude and Attitudes toward Occupations" (Division of Educational Reference, Purdue University, July, 1956), and Margaret Mead and Rhoda Métraux, "The Image of the Scientist among High School Students," *Science*, CXXV (August 30, 1957), 384–90.

⁴ *Chemical and Engineering News*, XXXIV (August 13, 1956), 3882–86.

reers. But there is no consensus on its content, even among its purveyors. Thus Philip Wylie, another writer of science fiction, challenges Asimov in his characteristically subdued manner:

The science in science fiction is most commonly employed either ignorantly or for sadistic melodrama. Most writers' ignorance of human psychology is as abysmal as that of philosophy . . . and they but create a new and sinister folklore, in which the latest facts from Massachusetts Institute of Technology are superimposed on a human insight hardly more developed than that of bushmen.⁵

A glance through the welter of comments produced by practitioners and by friendly and hostile critics of the genre makes it obvious that there exists no consensus on the nature of its content. Before considering the putative effects on the readers, we must have relatively objective knowledge of what is presented to them. First, however, it is in order to give a brief description of the historic development of science fiction and of the people who produce and consume it.

SCIENCE FICTION AS A GENRE⁶

Connoisseurs often make a distinction between "science fiction" and "fantasy." The latter deals with themes of the weird and supernatural; the former, with phenomena explicable in "scientific" (or pseudo-scientific) terms. In practice, however, the distinction is rarely observed, and science-fiction magazines, so called, publish both types of stories, though some specialize in one or the other type. Specialization also extends to literary level and choice of

themes, ranging from "space operas," with their black-and-white treatment of characters and stereotyped plots, to the highly sophisticated use of "psychological" themes and the unraveling of scientific puzzles.

The start of science fiction as a mass medium is usually associated with the founding of *Amazing Stories* in 1926 by Hugo Gernsback, an electrical engineer. Other periodicals soon came into existence, and by 1953 sixteen publishers put out thirty-five science-fiction magazines. (The number has since decreased considerably.) The "boom" also manifested itself in the publication of science-fiction novels and anthologies, both hard and soft covered, and in science-fiction films and radio and television programs.

The early issues of *Amazing Stories*, *Thrilling Wonder Stories*, and others had to rely largely on "classic" writers such as Jules Verne and H. G. Wells, whose stories they reprinted. Gradually a new, indigenous generation of writers arose, some of whom, like Ray Bradbury, became known to a larger and more diversified public than that which comprised the "typical" science-fiction readers of the 1920's and 1930's. There are no reliable data available on the quantity and makeup of science-fiction-magazine readership, but we do have some evidence that it shifted from a public with rather narrow "technical" and engineering interests to one with broader scientific as well as humanistic concerns.⁷ The American readership was estimated at six million in 1954.⁸ According to a survey made by John Campbell, Jr., the editor of *Astounding Science Fiction*, the typical reader of this magazine is male, a college student or graduate, under thirty-five, and engaged in the technical, professional, or managerial occupations.⁹ It should be re-

⁵ "Science Fiction and Sanity in an Age of Crisis," in Reginald Bretnor (ed.), *Modern Science Fiction* (New York: Coward-McCann Co., 1954), p. 235.

⁶ For a more detailed treatment see Walter Hirsch, "American Science Fiction, 1926-1950: A Content Analysis" (unpublished Ph.D. dissertation, Department of Sociology, Northwestern University, 1957), chap. ii, "Science Fiction, Its Nature and Development." See also L. Sprague de Camp, *Science Fiction Handbook* (New York: Hermitage House, 1953), chap. iii, and August Derleth, "Contemporary Science Fiction," *English Journal*, XLI (1952), 1-8.

⁷ This evidence is partly inferred from content analysis (see Hirsch, *op. cit.*, chap. iv, "Time, Place and Characters").

⁸ See S. E. Finer, "A Profile of Science Fiction," *Sociological Review*, II (new ser., 1954), 239-55.

⁹ *Astounding Science Fiction*, July, 1949, pp. 161 ff.

membered, however, that this is not representative of the entire readership, since *Astounding Science Fiction*, together with the *Magazine of Fantasy and Science Fiction* and *Galaxy*, caters to a relatively highly sophisticated and educated audience.

Readers, writers, publishers, and critics of the genre communicate with one another through the editorial and letter columns of the magazines and by way of more or less organized fan organizations which partake of the nature of a cult. The flames of organization and controversy are occasionally fanned by the monetary motives of publishers, but there appears to be an unusually high degree of *Gemeinschaft*-like interaction among the "fen" (as they refer to each other). Their emotions and creative aspirations are disseminated by numerous "fanzines," which are non-profit, amateur publications in contrast to the "prozines."

To what extent are the authors of science fiction scientists? Again, no extensive reliable data are available, but it appears safe to say that, as a group, science-fiction writers contain a greater proportion with formal scientific or engineering training than is the case with authors of other types of fiction.¹⁰ In view of this information, we assume that there is a "universe of discourse" involving a rather high degree of intercommunication between writers and readers. Consequently, our analysis of content is no mere academic exercise but should aid us in formulating hypotheses about the social functions of this type of literature.

METHODOLOGY

The universe comprises all science-fiction stories published between 1926 and 1950, as listed in Donald Day's *Index to the Science Fiction Magazines, 1926-1950*.¹¹ The

¹⁰ Of the "eighteen leading contemporary writers of science fiction" listed in De Camp (*op. cit.*, pp. 145 ff.), eight have a "scientific background" (i.e., they hold degrees in science or engineering or have done technical work or both); four of the eighteen are presently engaged in such work, and two hold Ph.D.'s in chemistry.

time period was divided into six subperiods, roughly corresponding to important historical events, as follows:

Subperiod	Years	Characterization
1.....	1926-29	Prosperity
2.....	1930-33	Depression
3.....	1934-37	New Deal
4.....	1938-41	Fascism and War in Europe
5.....	1942-45	World War II
6.....	1946-50	Postwar period

For each of the subperiods a random sample of fifty stories was chosen from the listing of titles in Day's *Index*, providing a total sample of three hundred stories. The writer analyzed and coded each story on the basis of categories derived from about a dozen hypotheses.¹² Since the stories were read as they became available through loan or purchase rather than in chronological order of publication, an important source of possible cumulative biases was eliminated, so that they would not affect any generalizations based on time trends. The coded material was punched on Unisort cards and sorted manually for statistical and qualitative analysis.

FINDINGS

1. *Scientists as major characters*.—The proportion of scientists who are major characters in the stories declined steadily from 1926 to 1950. As indicated in Table 1, during the first (or prosperity) period, the proportion of *heroes* who were scientists was 44 per cent, but only 24 per cent during the postwar period (the difference is significant at the 5 per cent level).

Among the *villains* the scientists declined

¹¹ Portland, Ore.: Perril Press, 1952.

¹² In order to provide some check on the reliability and validity of the analysis, two other analysts were given ten stories to code with the aid of detailed instructions. It was not possible to give them the extensive training necessary for a meaningful reliability check, but we were able to eliminate several of the apparently least reliable categories and become sensitized to the intrusion of biases after comparing and discussing the results. For a detailed discussion of the procedures used and of the problem of validity in content analysis see Hirsch, *op. cit.*, chap. iii, "Methodology."

from 39 to 30 per cent of the total (the difference is not significant). It is difficult to account for this unexpected finding in terms of "reflection" of reality; more likely the decline is due to the broadening of the readership to include those less exclusively interested in scientific and technical matters. It may be argued that this long-range trend as well as the increase in heroic and the decrease in villainous scientists during the World War II period are attributable to the current involvement of

We had expected this to be equally true of politicians and military men, based on the hypothesis that scientists will be portrayed as being frustrated by these other elites. The results do not bear this out, statistically speaking, though themes of conflict between scientists and other power groups in the "garrison state" appear increasingly during the postwar period. At any rate, it seems safe to state that the often vehement attacks on the business elite and its culture that we find in science fiction is not duplicated at present in any other domain of popular culture, certainly not in the mass media.¹³ A more specific

TABLE 1
PERCENTAGE OCCUPATIONS OF HEROES

OCCUPATION*	TIME PERIOD					
	1	2	3	4	5	6
Physical scientist.	44	34	36	26	23	20
Social scientist...	..	2	2	..	4	4
Professional.....	11	17	12	10	13	10
Pilot, etc.....	2	9	9	9	8	8
Military.....	3	5	2	5	11	10
Total <i>N</i> for each period = 100						
per cent.....	61	58	56	58	47	50

* Other categories not shown.

scientists in nuclear and biological warfare. This is a plausible hypothesis, but a qualitative analysis of the stories in our sample fails to support it. We had expected a substantial increase in the number of social scientists, but their proportion of the total number of major characters never exceeded 4 per cent. Patently, the popular conception of the scientist as one who deals with "natural" rather than "social" phenomena is also found in the image presented in science fiction. However, it is likely that the role of social scientists will become more prominent after 1950, judging from our impressionistic reading of the literature beyond the regular sample.

2. *Heroes and villains.*—Throughout the entire time period scientists occupy the most frequent occupational category of villains (except for the wartime period, when they are replaced in first rank by businessmen). Businessmen are generally in second rank, followed by politicians and criminals. Evidently the businessman is the *bête noire* of science fiction (Table 2).

TABLE 2

DISTRIBUTION OF HEROES AND VILLAINS
BY OCCUPATION

	Heroes	Villains
Scientists.....	109	53
Businessmen.....	9	22
<i>N</i> = 193	Chi square = 14.46	<i>P</i> < 0.001

TABLE 3

AUTHORS' TREATMENT OF SCIENTISTS AND
NON-SCIENTISTS IN IMAGINARY SOCIETIES

	Favorable	Unfavorable
Scientists.....	24	9
Non-scientists.....	20	25
<i>N</i> = 78	Chi square = 5.10	<i>P</i> < 0.05

test of the value orientation of science-fiction writers is afforded by the analysis of the status of scientists in imaginary societies. The construction of such societies is a traditional device for expressing values which may not be safely stated in reference to actual societies. What is the status of scientists in either utopian or antiutopian societies depicted in our sample? Table 3 shows that, in general, scientists are represented as the "legitimate" elite (i.e., they are portrayed favorably) in comparison

¹³ This finding is relevant to the proposition that science fiction is the major contemporary popular medium for the expression of social criticism and the building of social utopias and counterutopias. This aspect of our study will be treated in another publication.

with non-scientists. We had expected these results, since it was our assumption that science-fiction writers tend to be spokesmen for scientists and for the scientific ethos. The vigorous antiscientific stance found among British authors like Aldous Huxley in *Brave New World*, George Orwell in *1984*, or C. S. Lewis in the trilogy comprising *Out of the Silent Planet*, *Perelandra*, and *That Hideous Strength* is not typical of American science fiction in spite of the critical success of these authors and of American writers like Ray Bradbury. On the other hand, as we shall point out below, the naïve adulation of the omnipotent and omniscient scientist is no longer a feature of the genre as it had been in its childhood.

TABLE 4

SCIENTISTS WORKING IN INDEPENDENT
AND BUREAUCRATIC SETTINGS

Time Period	Independent	Bureaucratic
1-3.....	85	13
4-6.....	44	20

$N=162$ Chi square=6.65 $P<0.01$

3. *Occupational setting*.—Increasingly, from earlier to later stories, scientists are pictured in a "bureaucratic" setting rather than as "independent." (We categorized college professors as "independent," unless it was shown explicitly that they were involved in bureaucratic problems in their university.) Table 4 shows the relevant data. The "gentleman scientist," unencumbered by problems of "human relations," is slowly replaced by the scientist involved in a network of interpersonal and institutional pressures. In this instance science fiction tends to give a relatively realistic picture of the setting in which contemporary scientific activity takes place.¹⁴

4. *The social roles of scientists*.—The image of the scientist is portrayed by a number of more or less conventional themes.

¹⁴ See, e.g., William H. Whyte, Jr., *The Organization Man* (Garden City, N.Y.: Doubleday & Co., 1957), chap. xvii, "The Bureaucratization of the Scientist."

The most frequent are the "Frankenstein" and the "Scientist as Savior" themes. Both these tend to decline during the later phase of the time period, while there is a relative increase in themes of Hubris—the scientist blasphemously attempting to attack natural or divine law—or the "Scientist as Martyr." What are the social problems depicted in the literature, and what part do scientists take in their solution?

The major social problems rank as follows in order of frequency. First, interpersonal relations, involving for the most part conventional "love interest," and, second, the effects of technology. These are basically of an unanticipated nature (e.g., biological mutations which have been produced experimentally and then get out of hand). The "Frankenstein" theme is, of course, associated typically with this type of problem. International conflict ranks third, followed by interplanetary conflict. The fluctuations indicate to same extent reflection of reality (e.g., there is an increase in problems of international conflict during Period 5), but reflection is not observable in all problem areas. Thus there is less concern with problems of poverty and unemployment during Period 2, the depression period, than in any other! Presumably, the "escapist" function of literature is operating in this case.

How are science and scientists involved in the solution of social problems? We had hypothesized that there would be a decline in the theme that utopia can be reached by the simple application of technology and natural science. Instead, human problems would be increasingly solved by (a) the application of social science (or natural and social science combined), (b) by the magical or charismatic powers of human beings, and (c) by the intervention of "aliens" from outer space and other non-human characters. The rationale for this hypothesis is as follows: Recent historical events have cast increasing doubts on the omnipotent role of technology and natural science. Tremendous strides have been

made in the conquest of nature, yet we are still beset by wars, crime waves, depressions, mental illness, etc., none of which has been amenable to controls. To the extent that the scientific ethos still holds sway, the answer to the problem is "human engineering," or the application of the social sciences. But it seems equally possible that there has occurred a more radical disillusionment with any sort of rational scientific means and hence a recourse to "magical" solutions. Many observers of contemporary culture have attested to the rise of irrational tendencies in Europe and to the "loss of nerve" which has displaced

gicians or scientists, are being supplanted by "higher" powers.

Much could be said about these findings as a reflection of general cultural trends. However, our present concern is specifically with the role of science. If our analysis has any validity, it would seem that the public, which could be expected to view science as the obvious means for the solution of social problems, does not in fact view science in this way. The contemporary authors who may be considered spokesmen for the scientific ethos have lost the sanguine belief in the omnipotence of science held by their predecessors.

TABLE 5
PERCENTAGE MEANS FOR SOLUTION OF SOCIAL PROBLEMS

TYPE OF SOLUTION*	TIME PERIOD					
	1	2	3	4	5	6
Technology and natural science....	46	28	38	26	28	26
Social science.....	3	..	4	3	6	2
Natural and social science.....	3	4	1	1	3	5
Courage of hero.....	15	34	20	29	20	2
Insight and ingenuity of hero.....	10	16	6	6	13	5
Human charisma.....	2	..	7	8	3	5
Intervention by "aliens".....	7	5	11	11	7	28
Total <i>N</i> for each period = 100 per cent.....	61	92	71	70	67	43

* Other categories not shown.

the sanguine and facile optimism so characteristic of the America of "unlimited opportunities."¹⁵

The data in Table 5 bear out the first part of our hypothesis: there is a distinct decline in the use of technology and natural science as a means for solution of problems (the difference between Periods 1 and 6 is significant at the 5 per cent level). But there is no corresponding increase in the use either of social science or of human magic and charisma; even the conventional attributes of heroes, such as courage and insight, do not fill the "vacuum." It is filled by the intervention of "aliens" (i.e., natives of other planets), especially during the postwar period (the difference between Periods 5 and 6 is significant at the 1 per cent level). Human actors, whether ma-

On the other hand, certain aspects of our analysis do not indicate a wave of resignation and irrational pessimism. The majority of the stories have "happy" endings, and there is no indication that contemporary science-fiction authors consider human nature essentially more evil or irrational than did earlier writers. Rather, there appears to be a greater recognition of the "reality principle," of the complexity of human behavior, of the "unanticipated consequences of purposive human action," as Merton has termed it. If the "aliens" represent any sort of principle or power, it would seem to be "chance" rather than some anthropomorphic deity. (In this connection it is of interest that in the stories we analyzed "chance" as a problem-solving device increases appreciably during the latest time period, while there is a notable absence of

¹⁵ *Ibid.*, chap. xx, "Society as the Hero."

any "return to religion" in the conventional sense.) Scientists are no longer either supermen or stereotyped villains but real human beings who are facing moral dilemmas and who recognize that science alone is an inadequate guide for the choices they must make.

CONCLUSIONS

Our analysis shows that the content of science fiction has undergone a number of significant changes. Some of these seem to indicate a reflection of actual historical and

social trends, but the reflection is selective rather than mechanical. On the other hand, we notice both critical and utopian treatment of the present and future position of the scientist. The next problem to be dealt with concerns the perception of the content of the literature by its readers and the effect it has on their motivations for engaging in scientific activity or for choosing other occupations.

PURDUE UNIVERSITY

IDEAS TOWARD A SOCIOLOGY OF THE CONCENTRATION CAMP¹

H. G. ADLER

ABSTRACT

There are five major possibilities for sociological study of the concentration camp: investigation of the social structure of the concentration camp; the concentration camp within the system of contemporary society; its role among all other modern institutions involving the deprivation of freedom; a comparative study of all institutions ever created for the deprivation of freedom; and a social-psychological study of the concentration camp. The concentration camp is seen as the most recent institution of oppression in which prisoners live without rights and under conditions of extreme want. The determining factors are always ideological and economic. The prisoners always fall into actual, if concealed, slavery carried on by police authorities. The concentration camp reached its most extreme and cruel stage of development in the totalitarian state.

Many years have passed since the collapse of German national socialism, and so many books have been written about its most notorious institution that even those extreme horrors have lost their power to shock. Fascinated disgust faded first into repugnance and then finally, especially in the Anglo-Saxon world, into indifference.

This is dangerous; these problems represent nothing less than an extreme—admittedly a fantastically extreme—special case of actual conditions, or, at least, they represent the latent possibilities found everywhere in modern society. As the problems of National Socialism have receded from their sphere of emotion, the time has come to look at them with sociological detachment. The concentration camp, in particular, deserves unbiased investigation, as I proposed more than ten years ago.²

I

There are five possible ways of studying the concentration camp.

1. One may look at the concentration camp as a society.

In contrast to other institutions involving loss of freedom, the concentration camp is conceived in principle as a means of rob-

bing men of their freedom permanently, not as a benefit of civilized criminal law (as in the civilian prison), or international law (as in prisoner-of-war camps), or as a measure to protect the public health (as in the case of the mental hospital). Social life in the concentration camp follows established regulations, analogous to those in homes for the aged, in barracks, hospitals, or dormitories. In addition, an autochthonous society emerges within the concentration camp. This society, to be sure, is subject to formal regulations and, indeed, reflects the social forms of the surrounding society to a fantastic degree; yet it develops according to its own laws, a phenomenon less marked or entirely absent in other isolated groups, whether they be nominally free or not. Thus, this line of investigation would focus upon the social structure specific to the camp, its internal relationships and accommodations. The sources would be the vast existing literature, of widely varying usefulness, on National Socialist, Communist, and other concentration camps.

2. One may look at the concentration camp within the system of contemporary society, especially in the authoritarian and terroristic states.

In a world made vulnerable, or at least strongly influenced, by the ideas of enlightenment, the purely secular state, the new democracy, and secular socialism, the fate of the man who dissents from the guiding

¹ This article was translated for the *Journal* by Wolf Heydebrand, of the University of Chicago, with assistance from Everett C. Hughes.

² "Concentration Camps To Be Investigated by Social Science," *Wiener Library Bulletin* (London), March-May, 1947.

principles and ideas of the ruling group has become important: how he is to be rendered innocuous as soon as the ruling group feels threatened by him and, how, under whatever pretense, that group discards all protections granted by the constitutional state and the democratic guaranties for personal inviolability. The dissenter is excluded from the community of those who conform, sent to the concentration camp, or killed. This approach to the study of the concentration camp would begin with the position of the ruling group, paying particular attention to political and economic conditions.

3. One might analyze the role of the concentration camp within the system of those contemporary institutions in which people are deprived of their freedom, permanently or temporarily, "lawfully" or "unlawfully."

Such an approach, for which the relevant prerequisite research is lacking, would necessitate the construction of a sociology of "the unfree." All known degrees of freedom would first have to be classified, described, and categorically determined, to show the extent of freedom granted, the restrictions on freedom, and the reasons given for the restrictions. Obviously, every person, according to his position in his society, is subject to institutionalized degrees of dependence which allow himself something less than the theoretical extremes of freedom. Every man belongs to a society; even his place in a family, or a school, or an occupation limits his freedom. He is continually more "institutionalized" by the community's striving for continuity, by its self-preservation, and even by its care of him. The extreme of institutionalization lies in the community's various forms of exclusion, provided he is allowed to live. In the modern state those extreme forms of exclusion are represented by the penitentiary and by the concentration camp.

4. Social-historical appraisal and social-morphological definition of the concentration camp would form the objective of a fourth kind of study.

This line of investigation should not only

describe the history of the modern concentration camp but seek out the institutions of earlier times that are akin to it and that exhibit elements likely to exist whenever men are significantly or totally excluded from a relatively free community. An understanding of the concentration camp is impossible without insight into the nature of slavery; the concentration camp is part of the history of slavery.

The will to rule is not necessarily also the will to oppress, but the opposition of the ruled leads inevitably to their oppression, given the initial violation of recognized principles of freedom. Whenever this occurs, and is not checked, the concentration camp, or a similar institution of slavery and extreme deprivation of freedom, may threaten. In the era of postenlightenment, of the pseudo-socialist state, and of the state that rests on democratic (and antidemocratic) principles, the concentration camp is an ideal instrument. For ideological and propagandistic reasons, as well as for reasons of tactics in dealing with other nations, this type of state cannot "officially" reintroduce formal slavery, and therefore it must do it by subterfuge. The concentration camp is an institution of *concealed* slavery in a regime which, for arbitrary and complex reasons, cannot realize its tyrannical goals without unrestrained mendacity.

5. This last line of inquiry leads to a social-psychological consideration of the concentration camp concerned with the critical examination of modern and, especially, of totalitarian-terroristic power structures. They may be identical with the state, but usually they are not, because the state is merely an instrument, that is, an executive organ for the decrees of the ruling group (e. g., the National Socialist or Communist party or, perhaps, a party unit like the SS or the NKVD).

In contrast to earlier autocracies, dictatorships, and terroristic regimes, the modern totalitarian state cannot appeal to sacred commitments, yet at the same time it cannot get along without obedient—nay,

blindly devoted—adherence on the part of its collaborators. The deep commitment to the regime is not of a sacred nature, yet the committed owe their co-operation not to love or individual convictions but to an ideological persuasion which forces the free man to renounce almost all freedom of thought. He who does not renounce it is in extreme danger of losing his social and material (physical) freedom, the very first predestined inmate of the concentration camp. The ideology as such is neither true nor false. It is a prescribed conglomerate of ideas with crude emotional appeal, the principles of which can be arbitrarily applied, revoked, or at least put aside. Thus, the followers, as well as the opponents of a totalitarian regime governed by an ideological “superstructure,” are forced constantly to adapt themselves to the “will” of the moment—the notorious “party line” of the Communists—and at the cost of perpetual dishonesty. He who does not succeed in achieving a flexible, well-timed adjustment to the arbitrarily chosen ideological—in fact, tactical—changes of line is disenfranchised as a member of the “free” society and sent to the concentration camp. Only there, relieved from extreme strain, may the person, under certain conditions, begin to behave “naturally”; the more hopeless his civil position, the easier it may be for him to re-establish his inner freedom, as has been documented by the inmates of Russian and German concentration camps. This relative freedom from falsehood can exist unless the living conditions of the camp itself reach a point that excludes all thought on the part of the inmates or unless the terror becomes so strong that not only the top functionaries of the “self-administration” but also the rest of the inmates cease to show any sign of spiritual freedom.

II

Slavery and serfdom were abolished almost everywhere in the nineteenth century and have not since been reintroduced in any country, even in totalitarian states. In

what sense, then, can the concentration camp still be categorized under the system of slavery in strict definition of the term? What is slavery? We shall have to touch on this briefly.

In the short but pregnant definition of my late friend, the social anthropologist of Oxford, Franz Baerman Steiner, slavery is *exploitation of men without contract*. Wherever slavery is found in unmitigated form, we must recognize in the slave a man deprived of all the rights society attributes to other men. In slavery one man has power over another without standing to him in any relation the law would consider as creating mutual rights. The result is a one-sided solidarity; the master can punish the slave, but not the slave the master. The legal relation is one of property, not of contract; *jus in rem*, not *jus in personam*.

The proprietary relationship brought about by the capturing and trading of slaves can be easily understood in regard to private slavery, where slaves are owned by a household or individual owner. (This form of slavery was widespread in Western cultures from antiquity until the nineteenth century.) The proprietary relationship is, however, usually obscured in modern slavery as maintained by the state; no individual has personal rights upon the slave, at least not in theory. There is no open slave market; behind the public's back, labor is officially leased but not sold. Here slavery is monopolized, organized, and maintained by special state agencies. By an administrative act, slaves are created, removed from freedom or legally justified imprisonment, and taken into absolute bondage. In totalitarian countries the maintenance of slavery is in the hands of the police and is neither officially admitted nor legally established in the traditional sense. Consequently, it is not a publicly recognized institution. Its true meaning is usually hidden from the people, the free as well as the enslaved; officially one refers to “prisoners.” For this reason it could more correctly be called “crypto-slavery” or, under the Nazis, “SS slavery,” where

the authorized slaveowners were the central security office of the Reich and its branches, *Wirtschafts- und Verwaltungshauptamt* (central office for economy and administration).

With the exception of a few humanized slave laws, state slavery always implies the isolation of the slaves from those who are free and from all social relations, especially kinship. (This is often the case, too, in private slavery, with the exception of the slaves who live in the master's household, but in private slavery laws and governmental actions set limits to a master's despotism.) State slavery makes the slave subject to the unlimited power of authorized officials and in most cases leaves him without protection and without control over his treatment. Such is the modern state slavery that keeps its victims in the concentration camp.

III

Although we are still far from having laid the foundations for a sociology of the concentration camp, we may now pursue a number of ideas implied in our sketch of the five main lines of inquiry.

In the form primarily developed by national socialism and bolshevism, the concentration camp is the latest institution of oppression reserved for our day and time. In the long run, and in its most extreme form, it can be maintained only under a totalitarian regime. The term "concentration camp," although new, is semantically revealing. Its very primary meaning designates so novel a way of setting people off for special captivity that it hardly suggests earlier institutions of imprisonment such as penal servitude or other forms of legally based custody or exclusion from the society of free men.

However, the institution as such does exhibit the characteristics of older methods of arbitrary deprivation of freedom, while adapting them to new tasks and needs. Actual or merely potential opponents of the existing order, even suspected or actu-

al members of ideologically stigmatized groups, are taken into custody and concentrated in a camp without legal procedure. An example is the type of internment camp established in Cuba toward the end of the last century and, later, by the English in the Boer War, who called them "concentration camps" for the first time. They were planned as emergency measures only for the duration of the war. Similar camps were known during World War I, especially in Russia; these were internment camps for alien civilians. Under international law the legality of such procedures to prevent escape, espionage, and undesirable political activity, where there is no reason to suspect any given individual, is at best questionable. Similar internments occurred during the wars of the nineteenth century (e.g., in 1870-71), although certain humanitarian principles were respected. As protective custody in the interest of the state or the rulers, this institution is very old and has been preserved in the tradition of taking hostages.

During the European wars of the eighteenth and nineteenth centuries, civil prisoners were usually treated gently; they were protected, if possible, from any excesses among their guards and, by and large, were subject to regulations that applied to prisoners of war. There was no obligation to work, a principle still upheld by many countries during World War II. But the situation of these detained aliens grew steadily worse during the twentieth century. During World War I, Russia arrested even its own citizens as a preventive measure and exposed them to inhuman treatment. France dealt harshly with the German civilian prisoners in Africa but still called it an "emergency measure against aliens." Russia went one step further—deportation—a practice she has never abandoned. This not only was a punitive measure, as it was, for instance, in France even in the twentieth century, but was linked to the security aspect of preventive arrest. National socialism re-established this form of captivity as "honorary custo-

dy" for distinguished citizens or aliens who were sent to concentration camps.

The arrest of innocent relatives of an opponent of the regime was a revival of the practice of taking hostages and of the ancient institution of kinship liability. Bolshevism has used this kind of liability on a large scale. In keeping with an ideology defined as the "party line," entire groups were labeled according to their nationalities, their membership in real or supposed social classes, and their political convictions, thus becoming enemies of the state, fit to be imprisoned and deported. They were used as enforced labor to make possible and maintain the economy of Soviet Russia.

In Russia the original significance of the concentration camp was changed by the devastations of World War I, by the methods of combat during the Revolution and the Civil War, by the rise of a police power unconcerned with human dignity, and, finally, by the establishment of a ruthlessly controlled economy and an autocratic despotism which depended on political "purges." Officially, the concentration camp was not called by that name, although it corresponded exactly to its designation today.

Russia has never undergone a period comparable to the epoch of humanism in the West. The transition from feudalism to a system of mechanical materialism, which distributed positions in state, economy, and party-like feudal tenures, occurred almost without intermediate steps. About sixty years after the abolition of serfdom, the concentration camp became a permanent institution. Unlike the German concentration camp, the Russian was aimed from the very first principally at economic exploitation of the inmates. Thus the Russian concentration camp never had so much an ideological function as an economic one—"socialist reconstruction." Although the reinforcement of the regime played an important and ideologically relevant role, it was soon superseded by the intention to secure cheap labor. It has already been indicated

that the concept "enemy of the state" lent itself to broad interpretation indeed, as documented by the fate of "Trotskyists," "Kulaks," or Crimean Tartars.

Some semblance of legal procedure was usually kept up. "Sentences" were passed; in Stalin's later period these were never under ten years, more often twenty-five years. The conditions in the camps varied according to climate, kind of labor, and local conditions. They could be unbearable, which almost certainly meant a quick end, but one could live in them, too. According to many accounts by witnesses, the conditions have improved since Stalin's death, but it remains to be seen whether the abolition or, to be more exact, the "significant reduction" announced by Khrushchev is true or merely propaganda and tactical interlude. If it should turn out to be true, then the concentration camp, according to the proclaimed directives, will be modified into a place of enforced residence, and slavery into bondage or serfdom.

In the Russian concentration camps, labor is usually exploited for life according to the "norm" of a controlled economy, one, in fact, quite without reason. In this case, as in all concentration camps typical of totalitarian states, inmates are subject to a special discipline. These are its main characteristics: The prisoners are strictly secluded from the rest of the world, although, when working, they may have occasional contacts with the free population; they may send out or receive little or no mail; visits are almost never permitted; they live in poor and crowded quarters and are insufficiently clothed, fed, and, in case of illness, poorly cared for; the sexes are kept separate; prisoners are subject to rigid regulations, watched inside and outside the camp, and subordinated to functionaries appointed from their own ranks. As a rule, they carry out enforced labor, generally arbitrary and excessive; they have on principle no rights and seldom the possibility of fashioning their individual lives, even in their too few hours of leisure; if not privileged as functionaries, they have

no acknowledged rights whatsoever and are therefore in no way legally competent and thus can be punished by the least violation of actual or simply fictitious regulations. In other words, they are utterly at the mercy of their two levels of superiors (those who are themselves prisoners and those who are free). In a concise formula we may state: The prisoner in the concentration camp lives without rights under conditions of extreme want. Often not a convict, he does not even enjoy the protection against abuses granted the sentenced convict in a constitutional state. There exists a gross disproportion, and usually no relation whatsoever, between the alleged or—much less frequently—real offense, on the one hand, and the prisoner's state of being temporarily or permanently deprived of freedom, on the other.

These propositions hold for any developed systems of concentration camps and thus for the National Socialist too. But in Germany the ideological function was predominant. In spite of the inscription over the doors—"Work makes man free"—economic exploitation over and above the needs of the SS played little or no role at first. It became increasingly important only in the war economy of the later years of the Third Reich. At first, the main function of the camps, as stressed by Eugen Kogon, was the elimination of any real or supposed enemy.³ Anyone who was, on ideological grounds, presumed dangerous was held captive. Here "ideology" refers primarily to the biological "Aryan-Jewish" myth, the witch mania of our day, which is all there is of National Socialist "metaphysics" and "doctrine of salvation"; whereas communism uses an economic utopia for its totalitarian aims. Because of this race ideology, the camps in Germany had to absorb, over and above political opponents, the members of allegedly inferior races, Jews and gypsies, who were in this manner insulated from society, together with so-called asocials, true criminals, and actual opponents

of the regime. Thus, wherever Hitler's power reached, all Jews were imprisoned.

The blind hatred against the Jews stood in the way of planned exploitation of their labor almost up to the end. This, however, did not exclude their economic exploitation on a more primitive level, ranging from stealing Jewish property down to the last miserable item to the shameless use of the corpses of these senselessly killed millions. Examples are the taking of the gold fillings out of teeth, the cutting-off of women's hair, and even the use of the ashes of the burned bodies as fertilizer.

In the course of time the principle of the Nazi camps—to extinguish all real enemies or all persons thus labeled—became more and more closely tied to Russia's prime objective: the exploitation of labor. From then on, however, the National Socialists used such extreme methods that the vital energy of their victims was exhausted considerably faster under their regime than under any other institution of oppression for which we have statistics, Negro slavery, for instance. This has two reasons. The isolation and the extinction of Jews and, secondarily, of other people were considered a mythical goal, an act of liberation and salvation, as Hitler imagined it. This demonic disdain for man did not allow for any change of policy, even when other interests became important. This explains the second reason. It is obvious that the link between cruelty and disdain for human life could not simply be turned off. Although this cruelty is not without precedent, it had never before been a principle in itself in any other system, certainly not in Russia.

As a result of the difference between Russia and Germany in the matter of slave labor, the methods of treating slaves are different too. In Russia, torturing individuals, beyond forcing them to live under certain general conditions, was avoided in theory, although by no means always in practice. In Germany, abuse, if not expressly ordered, was, however, an essential feature of the system. The SS was actually

³ *Der SS Staat* (2d ed.; Berlin: Druckhaus Teus-
helhof, 1947).

spurred on to continued mayhem. A further difference is geographical: camps located in jungles and the polar zone need not be as isolated or the slaves as strictly guarded as in Central Europe.

The differences between the National Socialist and Bolshevik systems also account for the kind of discipline exerted in the camps. A certain personal relation between a tormentor and his victim is necessary, even if cruelty has become a habit. Even the bestiality practiced by the SS on the lowest, most vile level was charged with affect and, in this abominable sense, was still human. The extinction of the prisoners was always the main function of the German camp. But, where every human relation is dispassionate and of little appeal, the situation is different. The bestiality of bolshevism was unconditional; it was not concerned with the individual, and it remained free of the terrible satisfaction gained from the victim's capacity for suffering. The utilization of labor has always been the principal aim of the Russian camp.

Both systems have in common an immense waste of human life, which is true of any form of slavery not moderated by legal or moral measures of protection. During the period of Stalin's absolute power, starting about 1936, a prisoner's average length of life was estimated at between two and five years, sometimes less, varying with the particular living conditions. The SS, as Kogon reports, figured the average length of life to be only nine months.⁴ (The transports of prisoners destined to be killed immediately in the huge slaughterhouses in Poland are not considered here; it should be noted that these figures certainly refer only to camps located within the German boundaries of 1937. In the East most prisoners, primarily Jews, literally wasted away within a still shorter period, often within a few weeks.)

Certain subspecies of the concentration camp developed. These were more pronounced in Russia, mostly because of the

longer history of bolshevism; yet, owing to insufficient information on the Russian camp forms, nothing final can be said. We know that there is every gradation, from camps with unconcealed slavery to relatively mild correction camps for short-term stay, even to those established simply to carry out the dictum "He who does not work shall not eat." The production levels of the controlled economy are applied throughout: fulfilment or surpassing of the "norm" determine what the employer gives in return—whether the worker is "free" or a slave in a camp. Thus "socialist competition" becomes the regulator of the standard of living for the whole people but graded in "freedom," as in servitude, according to certain prescriptions for different occupations.

Nationalist Socialist Germany was a socialist state neither in the sense of the Soviet Union and its satellites nor in any other. It was a leadership state with hierarchical levels of authorized subleaders who were always powerless before their superordinates but had full authority over their subordinates. This "leader principle" was applied to the concentration camp as well. Here, precisely because it was a closed society, this principle could develop in its purest form, side by side with the SS hierarchy it mirrored.

The "positive" levels of authority as formed by the SS in freedom were carried over to the "negative" levels of the enslaved prisoners. Their existence was less defined in terms of their output of work than by the degree of delegated power. Although subtypes of the concentration camp did exist, representing three grades according to the potential length of life of victims ([1] Sachsenhausen, [2] Buchenwald, and [3] Mauthausen), they were only taking discernible form at the time of the collapse of Germany. What was more important was that the "Final Solution to the Jewish Question" (a phrase standing for the extirpation of the Jewish people) could be accomplished without interference only in the concentration camps by deceiving

⁴ *Op. cit.*

both the victims and the rest of the world with the credible assumption that they were labor camps. Three types of camps were used to enslave or kill Jews: the "enforced ghetto" (a variation of the concentration camp, the *Zwangsghetto*); the regular concentration camp, where the Jews were the most oppressed group; and the annihilation camps (*Vernichtungslager*), which took two forms—the actual death camp, where almost everybody was killed upon arrival, and the attrition camp, where most persons died after a very short time from the consequences of extreme labor and abuse.

This treatment of the Jews makes the SS concentration camp a unique and incomparable institution within the general framework of slavery. Nowhere else has cold calculation, as known in the similar camp economy of the Soviets, been coupled so inseparably with such passionate hatred, a phenomenon encountered often in the history of mankind but probably never before intentionally institutionalized. This lent to the cruelty and terror usually—but not always—associated with slavery something uncanny, something that opened an abyss never dreamed of before.

The SS leased its slaves to capitalist entrepreneurs, but only the state or "socialized" corporations are permitted to be employers in Russia. In both countries, however, the authorized slave-keepers made their victims work on their own account and gained profits by leasing them to other parties. This became very important to the power of the police.

It is obvious that for the establishment and maintenance of the concentration camp only the police were suitable, and indeed only a special kind of police, one already organized to ferret out internal enemies of the regime. Concentration of power in a complex police system is part of totalitarianism. In Germany and Russia that police system was more and more organized like an army and often had authority over other military or civil organizations. The more people the police took under its absolute control, the more powerful it became.

Soon it had to consider the profitable utilization of the prisoners; it had to find replacements when their number threatened to decrease as a result of their miserable living conditions. Thus whole armies of prisoners were shut up for the most spurious reasons. The struggle against enemies of the regime became mere pretense. The security of the state had long ceased to be threatened. The only interest was to find new victims. In this way, the totalitarian secret police became the greatest slave-keeper of all times.

Thus one of the functions of power was transferred to the economic sector. Now, that power over human beings is certainly not primarily determined by or solely dependent on the economic basis of a given social order. But we must recognize, without subscribing otherwise to Marxist ideas, that, as soon as power is based on the oppression as well as exploitation of men by other men, then it can no longer be isolated from its economic implications.

The more workers the police were able to draw away from other employers, the more they could succeed in monopolizing the economic forces of the country and thus gain in political power. Although we still know too little about the development of economic relations between the Soviet secret police and other government institutions, it seems that in Russia struggles for power within the police organization itself and against and among other organizations of party and government have for decades focused on the exploitation of slaves and the continuation of an economy based on slavery.

The development of the concentration camp and the restoration of slavery did not arise from an original intent to exploit (as later became necessary) unfree men by an administrative act or from the conscious purpose of enslaving a whole group. At first, and then repeatedly, whenever the safety, the "permanent revolution" of the one and only "right" regime, demanded it, one simply rendered harmless real or possible enemies and then, finally, representa-

tives of any supposed opposition group. They were to be broken, cut off as independent, thinking personalities, and the still free population warned through anxiety and insecurity against all resistance or even against acts of mental maturity, for these were crimes which would be ruthlessly prosecuted.

Thus the beginning of the concentration camp is marked by terror, one of the essential bases of totalitarianism. It was decisive for the evolution of the concentration camp that the struggle against supposed enemies was impossible by ordinary procedure even if exercised by a judicial apparatus pledged to the regime—impossible in a formal sense and because of the great number of persons affected. They could not be tried for any objective offense by a jurisdiction unconcerned with the discovery of truth, even though the procedure itself was based on morally untenable legislation. Therefore, common jurisdiction had to be replaced by decreed secret proceedings as they had been institutionalized in Russia in the thirties on the occasion of the reputed “purges,” or, like the acts of violence commonly committed by the secret police in Germany, without any pretense to legal proceedings. If a man was not a criminal according to “objective” criteria, and thus could not be tried by an ordinary court or in any other way, then he could not be put into the traditional institutions of punishment. Different accommodations had to be chosen; the concentration camp offered itself as the appropriate expedient whose function for the pre-totalitarian period has been indicated above. Even the earlier concentration camp had not been established by legal enactment but was created by executive offices and thus was far removed from legislative and public control. Imprisonment became an administrative act of violence whose only justification consisted in an actual or alleged emergency which, once overcome, was to terminate the illegal deprivation of freedom.

We must note here another social-historical circumstance with relation to the

brutality of the concentration camp: the depersonalization of man, his transformation from a group-anchored individuality to an anonymous particle of mass. The terrifying union of brutality and leveling (*Vermassung*) corresponds to a mentality which no longer accords intrinsic value to the fellow man. The fellow man is reduced to a mere object whose value is determined only by the arbitrariness and interest of his oppressor. Brutality has always been associated with oppression, but depersonalization is a more recent phenomenon which emerges only in the critical periods of highly developed cultures, since it presupposes individuation as a step above the collective consciousness of early communities. The concentration camp is not possible without brutality and leveling; only with their combined systematic impact can the concentration camp exist.

If cruelty is a mental attitude with which society is often burdened and which sometimes determines the very social existence of a community, so are depersonalization and its modern expression—reduction to a mass—essentially social phenomena. Strictly speaking, the mass is a fiction, since no individual is a member of *the* mass or of any one mass but is always an individual in a group of men which is part of a society and a community.

But “mass” can be defined in the following way: mass is the elimination of personality involving any number of persons. For a people to be a mass, two things are required: men who renounce claim to personality and men who are forced to tolerate not being recognized as individuals. If a people, or a large percentage of them, degrade themselves to the point of becoming a mass, then one of the most important requirements is met for subjecting them to a totalitarian regime. (They may even be reluctant, although, unfortunately, they are often only too willing.) The subjection is completed in the concentration camp, especially among those reluctant individuals who resist the concomitants of *Gleichschaltung* (i.e., the process of “political co-ordi-

nation" or compliance with the "party line"). What brutality cannot accomplish either alone or together with oppression and slavery is obtained when these elements are combined with depersonalization, climaxing in the reduction of people to mass. Only this makes the concentration camp the place of utter human degradation.

How do these elements work for the society to which the concentration camp belongs? How do they operate in the *internal* society of the concentration camp itself?

These questions must be considered under the aspects mentioned earlier and then drawn together. This is the task of sociology in this field. Since concentration camps still exist among the primary dangers of contemporary life, the pursuit of all relevant problems should not be delayed. An adequate insight into these problems not only from a scientific but also from a political and pedagogic viewpoint is of utmost importance to humanity.

LONDON, ENGLAND

SYMBOLIC REPRESENTATION AND THE URBAN MILIEU

R. RICHARD WOHL AND ANSELM L. STRAUSS

ABSTRACT

The complexity of the city calls for symbolic management. Its complicated spatial features require representation in the form of devices for simplifying and for evoking images and sentiment. Verbal representation of cities has a formal nature. The available devices are popularly, frequently, and flexibly used. They are collective, as well as personal, representations.

If, as Robert Park suggested, the city is "a state of mind,"¹ then city people must respond psychologically to their urban environment; they must, to some extent, attempt to grasp the meaning of its complexity imaginatively and symbolically as well as literally. For many persons those very psychological demands make city life difficult to understand in emotionally satisfying terms. It is to this phenomenon that we address ourselves. We shall be concerned here with the manner and means whereby city people achieve social perspective on urban life and urban communities: how they learn to manage their impressions so as to symbolize whole cities; how they treat entire cities as evocative and expressive artifacts.

In analyzing these processes we shall be making them appear far more deliberate

and orderly than, in fact, they usually are. Ordinarily, the identifying characterization of a particular city, and the symbolic implications of that characterization for the quality of life it represents, are picked up, more or less incidentally, by the city-dweller as he works out his personal "life-style" in a particular community. The city is primarily problematical for him in limited, rather private terms. He must make a living in it, make friends, find a home for himself and his family, and work out a suitable daily round. In dealing with these tasks he senses some of the special qualities which seem to mark the city as a whole. Riding or walking about the city, reading its local newspapers, talking with people about it, he is exposed to a persuasive propaganda about its distinctive attributes. He builds up a set of associations which prepare him to accept and appreciate a shorthand symbolic characterization of the place.

I

¹ *The City* (Chicago: University of Chicago Press, 1925), p. 1. The passage in which this phrase is imbedded reads: "The city . . . is something more than a congeries of individual men and of social conveniences—streets, buildings, electric lights, tramways, and telephones, etc.; something more, also, than a mere constellation of institutions and administrative devices. . . . The city is, rather, a state of mind, a body of customs and traditions, and of the organized attitudes and sentiments that inhere in these customs and are transmitted with this tradition." Park, in 1925, was echoing in sociological phraseology, and in almost the same words, the more religious but equally psychological perspective of earlier generations. The Reverend Edwin Chapin preached to his congregation in 1853 that "the city is something more than an assemblage of buildings or a multitude of people; something more than a market or a dwelling-place . . . deeper than all, it has a moral significance . . ." (*Moral Aspects of City Life* [New York: Henry Lyon, 1853], p. 12).

Not only does the city-dweller develop a sentiment of place gradually, but it is extremely difficult for him even to visualize the physical organization of his city, and, even more, to make sense of its cross-currents of activity. Apparently an invariable characteristic of city life is that certain stylized and symbolic means must be resorted to in order to "see" the city. The most common recourse in getting a spatial image of the city is to look at an aerial photograph in which the whole city—or a considerable portion of it—is seen from a great height. Such a view seems to encompass the city, psychologically as well as

physically.² Actually, an aerial photograph is, for the layman, not only a rather vague but an extremely distorted image of the city: it cannot be read directly, since an exact interpretation calls for great specialized skill. Such a picture really serves to reduce the image of the city to a suggestive expression of density and mass. Furthermore, it simplifies the city by blurring great masses of detail and fixing the observer's attention on selected landmarks that emerge out of the relatively undifferentiated background.

Attempts to encompass the city at a glance preceded the airplane of course. "Bird's-eye" drawings, for instance, those pictures taken from the tops of tall buildings or from nearby hills and mountains, also attempted a view from a great height.³

² An English visitor, writing of Midwest America, has very graphically described the complex and coherent image offered by such a glimpse of a whole city. "The most impressive sight of the New World is when you sail into New York harbor—if it is on a clear day. But the most impressive first sight of the Midwest is when you fly into Chicago at night from the East, descending over the blackness of the prairie to the great, ruddy blast furnaces and steel mills, catching the first winkings of the Lindbergh beacon from the Palmolive Building away on the starboard bow, and watching the brilliant rectangles formed by a thousand square miles of straight streets and buildings. Huge sprawling city of swamp and prairie; one community of many communities, *communitas communitatum*" (Graham Hutton, *Midwest at Noon* [Chicago: University of Chicago Press, 1946], p. 140).

³ Some incidental remarks by a visitor to St. Louis in 1884 illustrate how common a practice it was to ascend to a high point in order to see and try to comprehend a whole city. In some cities, however, vision, hence comprehension, was impeded, with the result that the urban community was hidden from the interested observer. "But now for the city itself. Let us assume that we have entered it. . . . Whither first? Up into some high point, as the dome of the Courthouse, for a general view of the whole? Ah, except on some extremely rare occasion, that is useless to expect. The photographers take their pictures on Sundays when the chimneys have stopped streaming for the time being, and then some partial prospects are to be had; but, as a rule, St. Louis is as invisible as London. . . . No, St. Louis has got to be explored in detail" (Anonymous, "St. Louis," *Harper's*, LXVIII [1884], 498).

The same purpose has been served as well by a different manipulation of distance, by capturing a city from below. Some of the older delineations of Kansas City, for instance, are drawn from the Missouri River and mark out the profile of the city as it perches on the bluffs. Sometimes a horizontal vantage point may be used—as in drawings and photographs of a city across a river or a bay.

An alternative option for achieving the same kind of psychological distance is to manipulate the city rather than the point of observation. Traditionally, this has been accomplished through the use of models. Thus, in 1849, when New York was growing at an enormous rate, a group of craftsmen created an exact miniature replica of the city. The observer towered above the model, achieving much of the same effect that we now get from aerial photographs. Of course, a man who inspected such a model could probably see as little detail as we now see in a picture taken from an airplane. Hence the promoters of this particular miniature significantly emphasized that every detail of the city's houses and streets had been precisely reproduced, although no one could get close enough to the model actually to perceive this meticulous fidelity to the originals.⁴

These methods of portraying the city space are expressive declarations of its literal incomprehensibility. The city, as a whole, is inaccessible to the imagination unless it can be reduced and simplified. Even the oldest resident, and the best-informed citizen, can scarcely hope to know even a fair-sized city in all its rich and subtle detail. We can, here, take a cue from Robert Redfield's discussion of small com-

⁴ Ezekiel P. Belden, *New York—as Is* (New York: John P. Prall, 1849). This pamphlet includes excerpts from the local newspapers. The *New York Sun*, March 18, 1846, comments, apropos the manipulation of the city rather than the observer: "The whole expanse of streets, lanes and houses, will lie stretched out before the visitor, as it would appear to a person visiting it from a balloon—with this advantage—that he will be spared the nervous feeling incidental to an aeronautic expedition" (p. 16).

munities.⁵ He points out that small communities can be regarded from various points of view—their ecological and physical dimensions, their social structure, or their biographies. If we consider large communities in these same ways, it is apparent that the complexity of physical layout and structure is immense; that social structure is so complicated that even research teams of sociologists can do little more than grasp the outlines of significant groups and their interrelationships. And who, ordinarily, can hope to know or appreciate the whole social history of a city?

Is it surprising, therefore, that people will literally step back and away from the city to gain perspective on it?⁶ Distance clears the field of vision, even if it means losing some of the rich detail. In New York, for example, the observatory on the top of the Empire State Building serves as a classic vantage point for those who want to seize quickly an image of New York City. Approximately 850,000 people visit the observatory each year, and on most days slightly more than 2,000 come to look at the city. And even on those days when the city is invisible from a height because overcast with rain, fog, or snow, some still make the journey upwards to feed their imaginations with a view that they cannot actually see. From the top of the building the city is laid out like a diagram, pinpointed here

⁵ *The Little Community: Viewpoints for the Study of a Human Whole* (Chicago: University of Chicago Press, 1955).

⁶ Jacques Barzun underscores this point when he writes: "New York is a skyline, the most stupendous, unbelievable, man made spectacle since the hanging gardens of Babylon. Significantly, you have to be outside the city—on a bridge or on the Jersey Turnpike—to enjoy it." His remarks make clear also how difficult he himself finds it to represent the city conceptually without such distant physical perceiving. "Still one cannot enjoy it as the spectacle of grandeur it undoubtedly is. Not from the inside . . . none of these [buildings] is New York proper, which remains strictly invisible, a concept. . . . [Many of the buildings are architecturally bad,] yet together, what a sublime, unsurpassable skyline" (*God's Country and Mine* [Boston: Little, Brown & Co., 1954], pp. 240 and 252).

and there by landmarks. The panorama is divided off for viewers by visibility markers which bound imaginary concentric circles of vision at three, five, seven, ten, and up to twenty-five miles distant. From this height, the guides point out, anything within a mile radius constitutes "limited" visibility. What visitors grasp when they look out over the city is suggested by the artless eloquence of one of them: "The sun and the stars," she remarked "are the suburbs of New York."⁷ The principal psychological satisfaction, of course, is to perceive, somehow, the unity and the order that underlies the apparent hurtling disarray of the city⁸—to grasp it as a whole.⁹

But even an aerial view is for some purposes too large and too various to symbolize the city. Briefer, more condensed symbols are available which are often even more evocative, for all their conciseness. Thus the delicate and majestic sweep of the Golden Gate Bridge stands for San

⁷ This information is taken from two sources: the *New Yorker*, XXVII (July 28, 1951), 13-14; and *Reader's Digest*, LXI (August, 1952), 135-38. The quotation can be found on page 138 of the latter article.

⁸ "To the reader, the casual visitor, New York City must appear to lack the unity, the cohesion of older world cities. . . . Yet beneath the diversity of myriads of contrasts and conflicting evidences lies unity. If you look down from the vertical height of the Empire State building upon the city, congested here, splayed out there over marsh and plain and hillside, you observe the physical unity of the phenomenon, called New York" (Mary F. Parton, *Metropolis: A Study of New York* [New York: Longmans, Green & Co., 1939], p. 183).

⁹ In describing the truly tremendous sweep of New York that can be viewed from the Brooklyn Bridge, Alfred Kazin shows us how the visual representation of a city can embody both what is seen and the site from which it is seen: "[It] alone enjoys the full sweep of the harbor and the cities around it. . . . And the bridge that rises above it all . . . is from one point of view only the great idol to which all these streets are subjected. But from another it transcends them. . . . Brooklyn Bridge is beautiful—as a complex piece of machinery, as a work of architecture, and as the symbol and connecting tissue of the human history around it" ("Brooklyn Bridge," in Alexander Klein [ed.], *The Empire City: A Treasury of New York* [New York: Rinehart & Co., 1955], pp. 153-55 and 149).

Francisco, a brief close-up of the French Quarter identifies New Orleans, and, most commonly of all, a view of the New York skyline from the Battery is the standing equivalent for that city. (So well understood is this symbol that a movie can establish its locale by doing no more than flashing a picture of these skyscrapers on the screen for a moment and then directing the camera into the opening episodes of the film. This coded, shorthand expression is at once understood by the audience.) This familiar expression of the city's "essential" nature is as much accepted by native New Yorkers as it is by outsiders. Yet it is exceptionally difficult—and even unusual—for a New Yorker ever to see this part of the city. This sight is ordinarily available only for those who come into New York harbor from the sea. It is occasionally visible from an airplane, but if native New Yorkers wish to see this part of their city, they must take a ferry into the bay—perhaps to Bedloe's Island—in order to inspect the skyline.

For the purposes served by the symbol, however, it is not really necessary for anyone literally to see the view itself—the important thing is to be able to understand what it represents. The massed buildings, the solidity and density of the agglomeration, the gleaming roofs, the specious neatness and order that a far view lends the scene, seem to reflect all the energy, the crowdedness, the opulence and magnificence of the city. The skyline represents, in effect, the essence of New York, the great metropolis; New York, as the "greatest" and "richest" city in the world. Indeed, the imaginative impact of that skyline is sometimes so conclusive, so overwhelming, that to see the city in normal perspective, and in detail, may be anticlimactic. Thus, Rom Landau describes his disappointment at actually seeing New York after he had visualized it from its skyline. "New York's sea-front," he recalls,

shooting heavenwards like torches, is possibly the most exciting in the world. But the city itself might be described as a mere chessboard of

straight canyons imprisoned within gigantic walls, throbbing with a restless life, dynamic, stirring, yes—but beautiful . . . ¹⁰

These symbols provide another type of reductionism which makes it possible to encompass a city's wide expanse. Unlike the views from a distance, which distort, blur, and flatten a city's image, these panoramic views serve as masks. They achieve the simplifications and impose the limitations which come from looking at a façade. They blot out what lies behind, or invite the viewer to disregard it, in favor of the interpretation presented by the façade itself. Each large city contains a number of such façades—not all favorable. In Chicago the downtown skyline symbolizes one set of images, the Loop offers others, and Michigan Avenue or a South Side slum tenders still other partial vantage points.¹¹ None of them manages to perform as well as it promises. A large city is infinitely greater than its parts and certainly greater than its partial views, which mitigate but do not remove the pressures felt by an individual trying to understand imaginatively his urban milieu.

To a more limited degree than either device discussed so far, the spatial complexity and social diversity of a city, as a unit, is sometimes integrated by the use of sentimental history in selecting landmarks. Such history hardly ever follows the orthodox chronology of the city's actual development. Particular landmarks commemorate a symbolic past phrased around particular dramatic episodes of urban history.

¹⁰ *Among the Americans* (London: Robert Hale, 1953), p. 60.

¹¹ The editors of *Fortune* in their special issue on New York City make explicit for us this symbolic feature of façades. They offer their readers a view along Park Avenue, which they title "Metropolitan Symbol." Under this caption they comment: "Park Avenue is more a state of mind than a *must* address, since side streets are smarter, East River district pleasanter. This beautiful street . . . is the spine of residential metropolitanism. Along it, rich and nearly rich playboys and kept ladies. Within the symbol, the whole sleek East Side" (*Fortune*, XX [July, 1939], 84).

The Water Tower in Chicago, for example, is such a site; so is Telegraph Hill in San Francisco.¹²

II

Looking at the city, even if it be with an imaginative stare, is only the beginning of the search for the meaning and quality of urban life. What is seen, literally or in the mind's eye, must be expressed and interpreted. The crisis of awareness perhaps comes when one realizes that the welter of impressions will need conscious reflection. Such a moment has been caught for us by Charles Dudley Warner, who probably recorded it—as most of us would not—because he was a journalist making his way across the country to write a series of articles on western cities. "It is everything in getting a point of view," he decided.

Last summer a lady of New Orleans who had never been out of her native French city . . . visited Chicago and New York. "Which city did you like best?" I asked, without taking myself seriously in the question. To my surprise she hesitated. This hesitation was fatal to all my preconceived notions. It mattered not thereafter which she preferred: she had hesitated. . . . "Well," she said, not seeing the humor of my remark, "Chicago seems to me to have finer buildings and residences, to be the more beautiful city; but of course there is more in New York; it is a greater city; and I should prefer to live there for what I want." This naïve observation set me to thinking, and I wondered if

there was a point of view, say that of divine omniscience and fairness, in which Chicago would appear as one of the great cities of the world, in fact a metropolis, by-and-by to rival in population and wealth any city of the world.¹³

The city, then, sets problems of meaning. The streets, the people, the buildings, and the changing scenes do not come already labeled. They require explanation and interpretation. When it is argued in sociological literature or in the literature of city planning that cities are basically anomic and disorganizing or that cities represent optimal conditions for creative living, it is apparent that such remarks are simultaneously characterizations, judgments, and interpretations. The city can be variously conceived, by its citizens as well as its students: as a place in which to get ahead; a place where anonymity cloaks opportunities for fun, excitement, and freedom; perhaps as a place which undermines health and happiness but whose resources are usable from a safe suburban distance.

It is impossible, however, for the citizens of any city to comprehend it in its totality. But any individual citizen, by virtue of his particular choices of alternatives for action and experience, will need a vocabulary to express what he imagines the entire city to be.

Speaking about cities, in consequence, involves the speaker in a continual quest for the essence of his urban experience and for ways to express it. The language used, however, is a formal one. A fairly limited range of linguistic conventions has come into use whose formality is shaped by the fact that the form of the rhetorical devices employed does not depend on their content; their set phrasing is hospitable to any and all substantive statements about a city's qualities.

The urban environment is so obviously many-sided that one of the simplest and most obvious ways of giving it an underlying unity is simply to assume it and thus

¹² The very rapidity of change in many American cities destroys the value of certain landmarks for symbolizing civic history. Here is John Gunther standing amazed before this transiency of landmarks: "No city changes so quickly as New York; none has so short a memory or is so heartless to itself; it has an inhuman quality. Very few New Yorkers pay the slightest attention to the historical monuments that fill the city. . . . How many ever recall that Theodore Roosevelt was born at 26 East 20th Street, or that the oldest building in the city is on Peck Slip, or even that a three-million-dollar treasure ship is supposed to be lying in the East River near 53rd Street? My publisher lives in the east 30's. I had been in his delightful house fifty times before I learned that James Monroe had once lived in it" (*Inside U.S.A.* [New York: Harper & Bros., 1947], p. 557).

¹³ "Studies of the Great West, III: Chicago," *Harper's*, LXXXVI (1888), 870.

to speak of the city adjectivally.¹⁴ The speaker pretends that the noun modified—the place name—is fully expressed and completely explained by the sum of its modifiers. It is therefore possible to say of a city that it is brawny, lusty, cosmopolitan, smug, serene, bustling, progressive, brutal, sentimental, etc.¹⁵ For some, this is a permanently adequate method; they are content to feel that the quality of their own experience and the mood inspired by their own dimly sensed implications provide the cement that binds these attributes together.

The list of attributes may, however, grow so long and so quickly that confidence in the initial supposition that these qualities summarize themselves is undermined. Most obviously this occurs when contradictory attributes are assigned to a city, and these apparently conflicting interpretations seem to call for further explanation. Even so, it is still possible to shrug off the difficulty by claiming that the synthetic principle which reconciles these opposites is that the city in question is essentially paradoxical. Thus one writer comfortably concludes about Fort Worth that it "is paradoxically a metropolitan town."¹⁶

¹⁴ Cf. many of the selections, particularly those written after 1890, quoted in Bessie L. Pierce (ed.), *As Others See Chicago: Impressions of Visitors, 1673-1933* (Chicago: University of Chicago Press, 1933).

¹⁵ "Call Chicago mighty, monstrous, multifarious, vital, lusty, stupendous, indomitable, intense, unnatural, aspiring, puissant, preposterous, transcendent—call it what you like—throw the dictionary at it!" This frequently quoted characterization by Julian Street can be found in Bessie L. Pierce, *op. cit.*, p. 442.

¹⁶ Oliver Knight, *Fort Worth* (Norman: University of Oklahoma Press, 1953), p. viii. George S. Perry makes quite explicit this utilization of paradox to handle urban complexity: "A large part of the fun of making the acquaintance of Baltimore lies in trying to unravel its endless contradictions. Almost any sweeping statement you make about its character will be wrong. Most of its 101 consistencies are not simple, direct paradoxes, but oblique, chain-stitched contradictions which in the end lead one not merely around but over and under Robin Hood's barn" (*Cities of America* [New

Both the attribution of unity and the assignment of particular qualities can be organized around another, almost equally obvious, principle. The entire complex of urban life can be thought of as a person rather than as a distinctive place, and the city may be endowed with a personality—or, to use common parlance—a character of its own.¹⁷ Like a person, the city then acquires a biography and a reputation.¹⁸ Personified cities can be described with personal pronouns and, through the use of appropriate verbs, conceived of as having capacities for action and possession. And, following this fashion of speaking, we make the same allowances for and judgments of cities that we are ordinarily inclined to make for people.¹⁹

York: Whittlesey House, McGraw-Hill Book Co., 1946], pp. 45-46).

Paradox can also be suggested implicitly, as when Alvin F. Harlow titles his introductory chapter on Cincinnati "Dynamic Serenity" (*The Serene Cincinnati* [New York: E. P. Dutton & Co., 1950], p. 9). Likewise, in writing of New York City, Paul Crowell and A. H. Raskin literally deny the possibility of depicting adequately that city's great complexity, and merely remark: "New York is not a city. It is a thousand cities, each with its own life and spirit, all jam-packed into an area of two hundred ninety-nine square miles. It is Broadway and the Bowery; Wall Street and Union Square; Park Avenue and the lower East Side; Coney Island and the Stork Club" ("New York: 'Greatest City in the World,'" published in Robert S. Allen [ed.], *Our Fair City* [New York: Vanguard Press, Inc., 1947], p. 37).

¹⁷ Struthers Burt in his popular history of Philadelphia remarks: "This book . . . is a sort of civic biography, one of those novelist biographies where you treat the hero as a human being and try to find out what he is and how he got that way. There will even be occasional psychoanalysis, for cities are individuals" (*Philadelphia: Holy Experiment* [Garden City: Doubleday, Doran & Co., 1945], p. 14). Instances of the attribution of personality to cities are easy to find; indeed, it is difficult to avoid finding them.

¹⁸ Cf. Lloyd Lewis and Henry Justin Smith, *Chicago: The History of Its Reputation* (New York: Harcourt, Brace & Co., 1929).

¹⁹ "When this sense of the community emerges, Los Angeles will begin its maturity. Now it is adolescent, with the glandular imbalance of a young-

Urban complexity, which forces us to think in terms of unity or many-sidedness and personification, also leads us to conceive of cities as "really" or "essentially" like something else, something we already know and understand. In a word, complexity forces us to analogize. The analogy may be relatively implicit, for example, "Chicago is a city which must be dominated, as if it were a magnificent and severe animal that plunges and rears,"²⁰ or, one may use an explicit metaphor (e.g., "New York, city with a heart of nylon").²¹ The city may be termed or compared with a factory, a madhouse, a frontier, a woman.²² In all

ster who has grown too rapidly and wants glamour more than wisdom or enduring strength." These are the concluding lines of an article on Los Angeles by Maury Maverick and Robert Harris in Robert S. Allen (ed.), *op. cit.*, p. 387.

On occasion, cities will even be addressed as persons. Dorosha B. Hayes closes her popular history of Chicago with a frankly mystical mode of address: "Chicago. . . . You have grown big with an almost terrible American splendor—and in so short a time! Yes, short; until men christened you a town and pitched in to go to work on the idea in earnest, your spirit slept. Yet the first white man to come upon you and record the fact foresaw your future" (*Chicago: Crossroads of American Enterprise* [New York: Julian Messner, Inc., 1944], p. 302).

²⁰ See Bessie L. Pierce, *op. cit.*, p. 468. The quotation is by Walter L. George.

²¹ Paul Morand, "Rues et visages de New-York," *Les œuvres libres*, LXII (September, 1950), 97.

²² "Consider dear Old Lady Thrift. That is, the plump and smiling city of Milwaukee, which sits in complacent shabbiness on the west shore of Lake Michigan like a wealthy old lady in black alpaca taking her ease on the beach. All her slips are showing, but she doesn't mind a bit. . . . Old Lady Thrift is quite content to be recognized as the most safe and solvent city in America. On her hundredth anniversary she not only was out of debt, but had many millions in the bank.

"Tight and prosperous, yes, but the old girl has her contradictions. When her heart is touched, she will dig into her bulging reticule and give generously. But for the most part she is anything but soft. If city funds are the subject, she habitually puts on a flinty and defiant look. . . . She's an honest, good-humored, loveable old girl, but an odd one and no mistake" (Richard S. Davis, "Mil-

such phrasing the speaker draws upon the emotional and non-specific resources of language to make clear—in terms of something else with which we are already familiar—what seems to him to be the underlying meaning of an apparently confused and confusing urban world. Sandburg's Chicago as "hog butcher to the world" captures in poetic capsule for many people—to judge from frequent quotation—a salient quality of that city's life and air. Not a few descriptions of our cities have a poetic quality and a significant ambiguity which compose themselves into hymns of revulsion as well as into paeans of praise or devotion.²³

Analogies of cities, personifications of them, or mere lists of their attributes in a succession of adjectives—all these represent conscious efforts to establish those distinctive qualities which help explain or rationalize the swarming impressions that crowd in on the observer. These unique qualities, once established, can be elaborated in detail by an apparatus of illustration and pointed anecdote aimed at showing how these qualities lie behind and shine through typical events and institutions. This is not a language of mere illustration alone but of exemplification as well. For many years visitors to Chicago wrote about and saw the city expressed by its hotels, its crowds,

waukee: Old Lady Thrift," in Robert S. Allen [ed.], *op. cit.*, pp. 189-90).

²³ An example is Waldo Frank's conception of Chicago, "Hog Butcher for the World," published not long after Sandburg's poem: "On the one side, trains pour in the cattle and the hogs. On the other, trains pour in the men and the women. Cattle and hogs from the West. Women and men from the East. Between, stockaded off by the dripping walls, the slaughter houses stand mysterious, and throb to their ceaseless profit. . . . The spirit of the place—perhaps its soul: an indescribable stench. It is composed of mangled meat, crushed bones, blood soaking the floors, corroding the steel, and sweat. A stench that is warm and thick, and that is stubborn. A stench somehow sorrowful and pregnant, as if the seat of men joined with the guts of beasts and brought forth a new drear life" (see Bessie L. Pierce, *op. cit.*, p. 481).

its huge department stores, and its stockyards.²⁴ Kipling's famous assessment of that city contains a particularly brutal and effective description of the stockyards, as the archetypical symbol of the city, which concludes with:

And then the same merciful Providence that has showered good things in my path throughout sent me an embodiment of the City of Chicago, so that I might remember it for ever. Women come sometimes to see the slaughter, as they would come to see the slaughter of men. And there entered that vermilion hall a young woman of large mold, with brilliantly scarlet lips, and heavy eyebrows, and dark hair that came in a "widow's peak" on the forehead. She was well and healthy and alive, and she was dressed in flaming red and black, and her feet . . . were cased in red leather shoes. She stood in a patch of sunlight, the red blood under her shoes, the vivid carcasses tacked around her, a bullock bleeding its life not six feet away from her, and the death factory roaring all around her. She looked curiously, with hard, cold eyes, and was not ashamed,

Then said I: "This is a special Sending. I've seen the City of Chicago!" And I went away to get peace and rest.²⁵

It is possible to extend the notion of distinctiveness until the city's qualities—at least in combination—are thought to be unique. It is not only the booster who claims that there is no other city like his own; the ordinary citizen may feel this too, regardless of whether he approves of his city or not. The city's exceptional character can be declared openly as when it is asserted that "only in San Francisco" or by some such query as "where else can you find?" some particular institution, experience, or kind of person.²⁶ The same distinc-

tion can be claimed by stating categorically that everyone in a particular city acts in certain typical ways: All Baltimore is proud; all Boston is proper; all San Francisco is fun-loving.²⁷ Essence and uniqueness can be asserted most subtly, perhaps, by claiming that essence is masked by appearance or that appearance can be mistaken for essence. A visitor to St. Louis some years ago warned that

St. Louis is the only large Western city in which a man from our Eastern cities would feel at once at home. . . . And yet today St. Louis is new-born, and her appearance of age and of similarity to the Eastern cities belies her. She is not in the least what she looks.²⁸

Common speech expresses the essence-appearance inversion more directly than this oblique statement. We often say to visitors or newcomers that they must live a while in our city before they can see it as it really is. But making contrasts between cities does not automatically distinguish the peculiar atmosphere and quality of life in a particular city. We may, indeed, consider that distance does in fact lend enchantment, that when people visit other cities they return to their own with a sharper appreciation of the individuality of their native place, however discrepant these appreciations may be.

olies up a two-blocks-long stairway climbing the uppermost reaches of that near-Alp, Mount Adams . . . to a church on its brow . . . ?" (p. 14).

²⁷ For many examples see the volumes in the "Society in America Series," published by Dutton & Co. (for instance, Cleveland Amory, *The Proper Bostonians*; Alvin F. Harlow, *The Serene Cincinnatians*; and Robert Tallent, *The Romantic New Orleanians*). Almost any popular urban history is likely to use this kind of phraseology.

²⁸ Julian Ralph, "The New Growth of St. Louis," *Harper's*, LXXXV (1892), 920. Edward Hungerford, in his volume *The Personality of American Cities* (New York: McBride, Nast & Co., 1913), concludes his comments on Chicago with a similar phraseology: "There, then, is your Chicago spirit, the dominating inspiration that rises above the housetops of rows of monotonous, dun-colored houses and surveys the sprawling, disorderly town, and proclaims it triumphant over its outer self" (p. 211).

²⁴ See Bessie L. Pierce, *op. cit.*

²⁵ *Ibid.*, p. 261.

²⁶ This kind of rhetorical query, couched so as to highlight a city's individuality, is used by Alvin F. Harlow, in *The Serene Cincinnatians*: "For a century and more it has been doing things in its own whimsical way. Where else will you find grand opera with full Metropolitan casts presented in summer in a zoölogical park? . . . Where else will you find a thing comparable to the Good Friday pilgrimage of prayer by thousands of devout Cath-

Thus far we have been looking at cities as they are characterized by an indigenous symbolism, or distinguished by comparison with other cities. It is possible, however, to evaluate a city and its meaning in terms of certain assumed fundamental characteristics of American culture. New York City, for example, is said not to be really an American city at all.²⁹ Chicago, on the other hand, has been described as the very embodiment of the "American spirit," as the heart of America.³⁰ And, more recently, Kansas City has been declared to be "the typical American city."³¹ The echo of this mode of characterization can be seen in the

²⁹ For some representative statements, pro and con, see the following: Mark Sullivan, "Why the West Dislikes New York: The Eternal Conflict between City and Country," *World's Work*, LXI (1926), 406-11; "New York City," *Fortune*, XX (July, 1939), esp. 73-75 and 85; Charles Merz, "The Attack on New York," *Harper's*, CLXIII (1926), 81-87; Earl Sparling, "Is New York American?" *Scribner's*, LXXX (1931), 165-73; Lillian Symes, "Our Last Frontier," *Harper's*, CLXIX (1929), 636-45.

³⁰ In 1891, the astute French observer Paul de Rousiers characterized Chicago as "the most active, the boldest, the most American, of the cities of the Union. . . . It is here, indeed, that the American 'go ahead,' the idea of going always forward without useless regrets and recriminations, with an eye to the future, fearless and calm—it is here that it attains its maximum intensity. . . . This confidence has a double origin, the personal energy of the Americans and their accustomedness to success. This is why it is strongest where success has been particularly brilliant; stronger in the meat-towns of the Western than in the Eastern centres, which are already old; strongest of all in Chicago" (*American Life* [New York and Paris: Firmin-Didot & Co.], pp. 73-74). Fifty-three years later, we find John Gunther writing: "About Chicago itself there is so much to be said that the task of compression becomes hopeless. This is the greatest and most typically American of all cities. New York is bigger and more spectacular and can outmatch it in other superlatives, but it is a 'world' city, more European in some respects than American. Chicago has, as a matter of fact, just as many foreign-born as New York, but its impact is overwhelmingly that of the United States, and it gives above all the sense that America and the Middle West are beating upon it from all sides" (*op. cit.*, pp. 369-70).

work of popular historians of such cities as Cincinnati and Philadelphia, openly defensive because their cities do not seem to be marked by the progressive, bustling, and booster spirit of certain other American communities.³² Cities therefore can be conceived as being symbolic—or carrying the values—of entities larger than themselves. From the very nature of symbolism, as Alfred Whitehead points out, such a process of extension is well-nigh inevitable; for the growth of symbolism is luxuriant.³³ It knows neither historical nor spatial bounds. And, if speculation continues in quest of a truly American city, the time may come when Los Angeles will openly claim to represent most adequately what America stands for in the twentieth century.

It seems safe to say that without the resources of rhetoric the city-dweller could have no verbal representations of his own or any other city. Characterization of the city, and of the life lived in it, is indispensable for organizing the inevitably ambiguous mass of impressions and experiences to which every inhabitant is exposed, and which he must collate and assess, not only for peace of mind but to carry on daily

³¹ Cf. Ralph S. Perry, who, reporting upon America's cities for the *Saturday Evening Post*, recently awarded the crown to Kansas City on the following grounds: "Kansas City is a kind of interior American crossroads and melting pot where the Southerner, the Northerner, the Easterner and the Westerner meet and become plain John American, with America unfolding, to use one old-timer's rapt expression, 'in oceans of glory' in every direction. It got its start on the riches of Boston banks and Western lands and Northern furs. It is not only America's approximate geographical heart, but the center of gravity for her taste and emotion. The soap opera, movie or national magazine that doesn't 'take' in Kansas City won't live long in the nation." See also Henry Haskell and Richard Fowler, *City of the Future: A Narrative History of Kansas City* (Kansas City: F. Glenn Pub. Co., 1950), pp. 16-17; and Darrel Garwood, *Crossroads of America: The Story of Kansas City* (New York: Norton & Co., 1948), p. 327.

³² Cf. Alvin F. Harlow, *op. cit.*, and Struthers Burt, *op. cit.*

³³ Alfred N. Whitehead, *Symbolism* (New York: Macmillan Co., 1927).

affairs. When the city has been symbolized in some way, personal action in the urban milieu becomes organized and relatively routinized.³⁴ To be comfortable in the city—in the widest sense of these words—requires the formulation of one's relations with it, however unsystematically and crudely. Uncertainty about the character of the environment can only engender deep psychological stress.

All such symbolic representations of an urban milieu, however, are inherently unstable. Cities change, forcing those who live in them to face the inadequacies of what once were tried and true conceptions. A city after an economic boom, or swamped

by an influx of immigrants, is obviously not the same city it was before. But whether change is dramatic and massive, or mundane and subtle, urban social perspectives—and their symbols—ultimately fail even their most ardent backers. The most insensitive city-dweller, moreover, cannot fail to discover new facets of the city from time to time. One of the fixed conditions of an urban existence is that it provides an inexhaustible store of surprises. And it does not matter whether the surprise is pleasant, challenging, or deeply discomforting; it must, like all such impressions, be managed; it must be brought into consonance with other impressions of city life.

As new conceptualizations of urban surroundings are required, rhetorical devices once more come into play; only their content changes. To assess the novel and manage the discordant, analogy or some other rhetorical resource must be brought to bear. At that moment new symbolic representations—embodied in anecdote, slogan, poem, or some more prosaic form—crystallize and become public property.

³⁴ We are here following out the implications of the symbolic interactionist view of language. "Language symbols not merely stand for something else. They also indicate the significance of things for human behavior, and they organize behavior toward the thing symbolized." "A category . . . constitutes a point of view, a schedule, a program, a heading or a caption, an orientation." The quotations are, respectively, from Alfred R. Lindesmith and Anselm L. Strauss, *Social Psychology* (rev. ed.; New York: Dryden Press, 1956), p. 63; and John Dewey, *Logic, the Theory of Inquiry* (New York: Henry Holt & Co., 1938), p. 273.

IN MEMORIAM

R. RICHARD WOHL

1921–1957

The world of social science lost a brilliant citizen when Richard Wohl died of cancer on the morning of November 15, 1957. He was but thirty-six years of age—not yet old enough to leave the impressive body of work that his friends are certain he would have left had he lived, but not too young to leave a lasting mark upon his colleagues through his presence and his writing.

He had an unusual intellectual situation at the University of Chicago, stemming from the nature of his training and the bent of his mind. One of the first to receive a degree in social science at Harvard—following a course of study organized by himself with the advice of such faculty friends as Arthur H. Cole—he specialized in economic development and entrepreneurial history. Earlier, under the influence of Thomas Cochran, he had majored in economics at New York University; later he had received his Master's degree from Yale in the same field. He was a valued member of Harvard's Research Center in Entrepreneurial History, during 1948–49 its *rapporteur*, and later the editor of its discussions under the title *Change and the Entrepreneur* (Harvard University Press, 1949). He proposed that the Center found an exploratory journal, and, as a result, he jointly edited with H. G. Aitken its publication *Explorations in Entrepreneurial History*. When he was brought to the University of Chicago, he helped found the Research Center for Economic Development and Cultural Change as well as that immensely useful journal, *Economic Development and Cultural Change*.

His deepest interest during his last years, however, lay in social history (even his doctoral work, of which a version can be found

in William Miller's *Men in Business* [Harvard University Press, 1952], bears the impress of this interest); and one suspects that this lay at the bottom of his specializing—if the word is appropriate—in social science while at Harvard. Before he left that university he had begun his studies of the Horatio Alger tradition and of success ideologies of nineteenth-century America. He published only one paper from this material before his death, "The 'Rags to Riches Story': An Episode of Secular Idealism," in *Class, Status and Power*, edited by R. Bendix and S. Lipset (Free Press, 1953).

This research apparently led him in two directions—one, the study of popular culture. During the winter of 1956–57 he conducted an absorbing seminar on this topic, using notes from which he anticipated writing a book. (He did publish one paper, with Donald Horton, "Mass Communication and Para-social Interaction," which appeared in *Psychiatry*. Vol. XIX [1956].) At the same time he gave a number of delightful weekly lectures on American popular culture and social history over Chicago's educational television station.

The second direction toward which his research turned was urban history. On a three-year grant from the Rockefeller Foundation, he organized a project at Kansas City, Missouri, to study the history of that town. His research group consists of Theodore Brown, Charles Glaab, and Mildred Cox. Richard Wohl's writing of urban history embodied the skills and perspectives of his interdisciplinary training. When the project, which today continues under his research group, comes to a close, it is to be expected that something unique will have arrived on the

American scene, for Richard Wohl was part economist, part historian, and part sociologist.

It was this very diversity of perspective that some of his colleagues found so stimulating and others disturbing. As he several times observed, sometimes with asperity, sometimes with amusement, the historians thought him a sociologist; the sociologists relegated him to unmentionable historical domains. (His thoughts on interdisciplinary research can be found in "Some Observations on the Social Organization of Interdisciplinary Social Science Research," *Social Forces*, Vol. XXXIII [1955].) Nevertheless, he had, in addition to his appointment

to the Committee on Human Development, an associate professorship in the social science program of the college and a courtesy appointment in the sociology department.

Those of his colleagues close to him mourn more than his scholarship. They can scarcely forget his striking vigor, quickness, and—what makes each man memorable—his personal style of speech, gesture, and walk. Those who knew him less intimately will probably best remember him for his rich use of language and his passionate interest in an amazing number of intellectual issues.

ANSELM L. STRAUSS

University of Chicago

LETTERS TO THE EDITOR

POLITICAL TENDENCIES IN LOUISIANA

November 26, 1957

To the Editor:

Political Tendencies in Louisiana, reviewed in your November, 1957, issue by Frances and Monroe Lerner, obviously was written for people who know something about southern society and politics.

I would welcome a critical review if it were competent, if it gave even an idea of the content of the book and its relationship to the field of political sociology. The Lerner's make no mention of the relation of the book to such works as V. O. Key's or Allan Sindler's. Neither do they point out that as a comprehensive survey of the political ecology of a single state it is the first study of its kind. There is no mention of the Appendix, which presents obscure, hard-to-find voting statistics, or the possible contribution of the book to Louisiana history.

I should like to correct misleading remarks the reviewers have presented as criticism, relieved by no kind words whatsoever. The theme of the study was stated on page 6 as "the search for basic political tendencies in situations of change." True enough, as the reviewers remark, "its framework and method identify it as a study in political ecology." (About the only factual statement in the review.) The attempt is made to explain political behavior rationally, not through "the conflict of rational interests," as the Lerner's put it. This much is stated in the Preface, had the reviewers read carefully that far.

The "evident bias" exemplified in my treatment of Huey Long and his movement is on the part of the reviewers who apparently prefer to perpetuate the popular conception of Longism as an "American rehearsal for dictatorship." Such aspects of the issue are not denied, but the contribution of this book as clearly stated is sociological. The point is—and the Lerner's ought to have

seen it, had they read chapter iv—that the book demonstrates the relation of Longism to the socioeconomic frustration of the poor and white. "Huey Long, where others had failed before him, had successfully won and sustained the support of the depressed lower classes of Louisiana and provided a vehicle whereby they might have a voice in politics" (p. 189). Southern historians have summarized the rise of "Dixie demagogues" in almost the precise words of the reviewers—"a victory for the farmers over the Bourbon elite." In this connection here is what is said about Huey Long: "Attention has all too often been focused upon the organizational apparatus of Long's machine, upon the personal appeal and ability of Huey Long, or upon the corruptions of Longism; far more important is the sociological basis of Longism" (p. 124).

Statistics are presented throughout the book where they are needed to emphasize the verbal analysis (although there is no automatic virtue following the presentation of numbers). It is alleged that data are limited to the 1950 Census reports, yet the Bibliography shows reference to many: 1840, 1850, 1860, 1890, 1940, 1950, for example. The Lerner's criticize the lack of statistics of colored population with apparent negligence in reading the work; for, although detailed analysis of the distribution of the Negro population is presented for 1950 alone, the reason was carefully pointed out: we have known that the Negro is found in residence in the river-valley system pre-empted by the plantation economy. Whenever the presence of the Negro was crucial to the voting analysis, as for the periods of 1860, 1880-96, 1948, and 1952, careful study will reveal additional use of population data.

The example of "poor style and loose formulations" quoted from page 22 ("planta-

tion economy establishes social relationships similar to those which exist where big business prevails") serves only to demonstrate the poor reading ability and loose comprehension of the reviewers! The relation between plantation economy and the tendency toward greater degree of social stratification is discussed on page 5, and this insight is related to stratification and big business on page 22. In our system of numbering, 5 comes before 22. (To escape the provincialism of New Rochelle, the reviewers are asked to observe the remark of Professor John K. Bettersworth on the subject: "The

planter who succeeded and was able to maintain himself . . . was a good businessman in every sense of the word" (*Mississippi Quarterly*, X [Spring, 1957], 79).

Finally, so far as the allegation is true that the reader has difficulty identifying the geographical features of Louisiana, these are discussed in detail on pages 6-13, and with the elementary aid of an atlas clearly recognizable.

PERRY H. HOWARD

*Northeast Louisiana State College
Monroe, Louisiana*

REJOINDER

December 29, 1957

To the Editor:

It is most unfortunate that Mr. Howard cannot accept what was on the whole a generous review of his book in the spirit in which it was offered. His ill-mannered *ad-hominem* accusations against the reviewers do not add in any way to the quality of his book.

We are not, and have never claimed to be, experts on southern society and politics. This book was reviewed from the standpoint of the professional reader in sociology and political science for a *national* publication. Local or specialized journals may indeed treat it differently. Nevertheless, because a book is called a monograph does not relieve its author of his responsibility for treating his subject adequately, which includes full presentation of necessary background. In brief:

(1) Sociologically, or any other way, the rise of Longism was considerably more than merely a "victory for the farmers against the Bourbon elite." To be properly understood, it must be seen in a much broader framework, including its relationship to national issues and national party politics. It is one thing to be supported by a specific segment of the electorate and quite another to "represent their interests."

(2) How should statements such as these be evaluated, if not as evident bias? "The

social situation existing at the turn of the century made it certain that it was only a matter of time before *the people would be heard*. Huey Long's appearance upon the political scene in the 1920's was merely a *happy circumstance*—despite our personal predilections. Huey Long succeeded where others had failed in uniting the many elements of the population with common grievances and in giving them a *more equal share* in Louisiana government" (p. 124; our italics).

(3) It is one thing to say that "large-scale agriculture is in essentials, the factory system in farming. . . . In the United States 'plantation' has become the accepted term for the large-scale agricultural system of the Southern region" (p. 5), but quite another, and patently absurd as well, to say that, as a result, "plantation economy establishes *social relationships* similar to those which exist where big business prevails" (p. 22; our italics). It would be hard to find a better example of "loose formulation" in the recent literature, and no appeal to authority will make it any more conceptually precise or accurate.

If the practice of reviewing books in professional journals is to be more than merely an empty gesture, authors must be prepared for the possibility of critical reviews.

MONROE AND FRANCES LERNER

New Rochelle, New York

NEWS AND NOTES

Boston University.—Victor Arthur Gelineau, visiting assistant professor of sociology, is replacing Dr. T. Scott Miyakawa, who is on leave of absence in Ceylon and Indonesia, participating in the Advanced Training Program for Industrial Relations of the World Assembly of Youth affiliated with UNESCO. Mr. Gelineau was formerly on the staff of Wheaton College, Norton, Massachusetts, and is completing his doctoral work at Columbia University.

Mrs. Elizabeth Cohen has been appointed instructor in sociology. Mrs. Cohen is a candidate for the Ph.D. degree at Radcliffe College and was formerly Teaching Fellow in Social Relations at Harvard University.

Dr. Howard E. Freeman, research associate at the Harvard School of Public Health, has been appointed lecturer in sociology in the Evening Division.

Bowdoin College.—Richard Emerick, of the University of Pennsylvania, has been appointed instructor in sociology.

Burton W. Taylor will be on sabbatical leave during the spring semester.

Brown University.—Vincent H. Whitney is on leave of absence during the current academic year. He is serving as associate director of the Demographic Division of the Population Council, Inc., New York City.

Milton M. Gordon has been appointed visiting associate professor for the spring semester, 1958. He will teach a graduate course, "Race and Class in American Life."

University of California, Berkeley.—Reinhard Bendix is working on a study of the development of capitalist-class patterns in Germany in the nineteenth century on a grant from the Comparative Labor Movement Project supported by the Ford Foundation's Division of Economics and Economic Development. He is also currently writing a book which seeks to present in detail the substantive works of Max Weber.

Herbert Blumer has recently completed an inventory of research in the field of race relations which was commissioned by UNESCO.

Kenneth Bock is completing a book on the theory of social change.

Kingsley Davis, who is back in residence after a year at the Center for Advanced Study in the Behavioral Sciences, is continuing to direct the work of the International Urban Research Office which is made possible by a five-year Ford Foundation grant. He is also editing a volume of the *Annals of Political and Social Science* which will deal with population trends in the Western Hemisphere.

Wolfram Eberhard has just returned from a year and a half in Pakistan, where he has been engaged in the study of village social organization under a grant from the Asja Foundation.

Erving Goffman joined the department in February, 1958. At Berkeley he will continue research under the sponsorship of the National Institutes of Health and will teach courses in social psychology.

William Kornhauser is completing his analysis of theoretical and empirical materials bearing on the social bases of authoritarian movements. The project is one of a number supported by a grant from the Behavioral Sciences Division of the Ford Foundation.

Seymour M. Lipset is engaged in an analysis of the sources of political diversity as part of a comparative analysis of the social prerequisites of democracy. The study is supported by grants from the Behavioral Sciences Division of the Ford Foundation and the Committee on Comparative Politics of the Social Science Research Council. He is also working on an analysis of the variations between members and non-members of trade unions among American workers.

Leo Lowenthal is engaged in an analysis of problems created by the rise of popular culture in Victorian England.

Talcott Parsons, currently a Fellow at the Center for Advanced Study in the Behavioral Sciences, is giving a graduate seminar in social theory which will deal with approaches to the study of complex social systems. Professor Parsons is visiting for the Spring Semester.

Leo Schnore is engaged in an extensive program of research on urbanization which includes analyses of the timing of metropolitan decentralization in the United States; the compara-

tive study of urban and metropolitan development in the United States and Canada; and a study of the components of population change in metropolitan suburbs and satellites.

Herbert F. Schurmann has completed a study, supported by the Ford Foundation, comparing successful and unsuccessful acculturation of previously nomadic groups which settled in the mountain regions of Afghanistan. As part of his work in comparative sociology he is currently dealing with the relation of village structure to national behavior patterns in Arab countries and is studying the factors underlying the success or failure of Communist organizational forms in China and other Asian countries.

Hanan Selvin is engaged in a study of group influences on political behavior under a grant from the Institute of Social Sciences. He and Nathan Glazer, visiting lecturer, are also working on an analysis of attitudes toward civil liberties which is supported by Stiles Hall, the University YMCA. Professor Selvin is also working on a series of papers dealing with the analysis of survey materials.

Philip Selznick is engaged in research on job rights and due process in modern industry, a study in the sociology of law. Howard Vollmer is in charge of a field study of workers' attitudes toward job rights, which will form the basis for his dissertation. Patrick McGillivray is collaborating on the arbitration phase of the project. The research is supported in part by the Institute of Industrial Relations. Professors Selznick and Lipset have received a grant from the Fund for the Republic to plan a study of the reactions of workers to constraints on their freedom.

Tamotsu Shibutani, on leave of absence during 1957-58, has completed a study of rumor and is finishing a study of ethnic segregation and military morale.

Martin Trow has finished an analysis of the social determinants of McCarthyism and is now working on analysis of public opinion formation as a continuation of a community study which he and Robert Leigh, dean of the Columbia University School of Library Science, conducted at Bennington, Vermont, under a grant from the Behavioral Sciences Division of the Ford Foundation.

During 1957-58 Lewis Coser and Nathan Glazer have been serving as visiting members of the department. Professor Coser has just completed (together with Irving Howe) a book on the history of American communism and has begun preliminary research on the sociol-

ogy of revolution and of the intellectuals. Mr. Glazer is engaged in a study of communism as a social movement under a grant from the Fund for the Republic.

In addition to the foundation grants for specific projects listed above, the research work of Professors Davis, Eberhard, Lipset, and Selznick has been facilitated by so-called free grants from the Behavioral Sciences Division of the Ford Foundation, which may be used at the grantee's discretion to facilitate his basic research.

Columbia University.—The Department of Sociology has introduced a new integrated sequence of courses in basic methods of research, to be selected from four courses designed to cover the broad range of work in methodology: historical method and documentary analysis; laboratory and field methods; analysis of census and comparable data; and survey methods.

Alain Touraine, *agrégé* de l'Université de Paris and research fellow in the Centre d'Études Sociologiques, has been appointed visiting lecturer for the winter semester, replacing Professor Robert S. Lynd, currently on leave of absence.

Johan Galtung of the Institute of Sociology, Oslo, has been appointed instructor in sociology.

Greschan N. Sykes, of Princeton University, has been appointed visiting professor of sociology in Columbia College for the winter semester, replacing Professor C. Wright Mills, now on leave of absence.

Charles Y. Glock, director of the Bureau of Applied Social Research, is spending the academic year at the Center for Advanced Study in the Behavioral Sciences. Dr. David L. Sills has been appointed acting director of the Bureau.

Hans L. Zetterberg has been appointed associate professor of sociology.

Herbert H. Hyman is spending his sabbatical year as visiting professor of sociology at the University of Ankara, where he is conducting courses in methods of social research.

Natalie Rogoff, research associate of the Bureau of Applied Social Research, is spending the year at the Institute of Sociology, University of Oslo.

Allen H. Barton has returned to the Bureau of Applied Social Research, after three years at the University of Chicago Law School, as associate director of the Documentation Project for Advanced Training in Social Research.

Dr. Bo Anderson, of the Department of So-

ciology, Uppsala University, currently in this country on a Ford Foundation fellowship, and Mr. Frederick Schick have also joined the staff of the Documentation Project.

Robert S. Lee, of the Research Center for Human Relations of New York University, has joined the staff of the Bureau of Applied Social Research as research associate.

Margaret Bright, of the Bureau of Applied Social Research, is now in India as technical assistant with ECAFE and the Far East Centre for Training and Research in Demography.

The Columbia University School of Public Health and Administrative Medicine announces the following appointments: Joseph Greenblum has received the first traineeship given by the School to a sociologist. His studies and research will be focused on the area of epidemiology of schizophrenia. His faculty advisory committee consists of an epidemiologist (E. Gurney Clark), a psychiatrist (Ernest Gruenberg), and a social scientist (Jack Elinson).

Josephine J. Williams has joined the staff of the Administrative Medicine Research Unit as research associate. She is completing a pilot study of patterns of medical care and health insurance among union members and their families.

Bruce P. Dohrenwend has accepted an appointment as social scientist on a study of community psychiatry, jointly undertaken by the Psychiatric Institute and the School of Public Health and Administrative Medicine.

Harold Alksne is working on a follow-up study of juvenile narcotic addicts treated at Riverside Hospital.

Community Studies, Inc.—Blanche Geer was on leave during October, 1957, to assist Everett C. Hughes, who conducted a nursing study at Radcliffe College last fall. Dr. Geer trained interviewers for the field work of the study, which concerns the process of career choice of nursing students.

Peter New directed a National Nursing Research Conference in Kansas City on September 12-14, 1957, under a Public Health Service Grant to Community Studies, Inc. Nineteen sociologists participated. Mr. New also did a study on agency functions in the recent Kansas City tornado, under the auspices of the Disaster Research Group, National Academy of Sciences.

Comparative Studies in Society and History.—A new international quarterly, *Comparative*

Studies in Society and History, will appear in October, 1958. The purpose of the journal is to serve as a clearing house for substantive work on problems common to any two or more of the disciplines dealing with man's life in society. The editorial committee—Sylvia L. Thrupp, G. E. von Grunebaum, Everett C. Hughes, Edward A. Kracke, Jr., Max Rheinstein, Edward Shils, and Sol Tax—invite contributions from fresh research. They hope to achieve some testing of the ideas of the theoretical social sciences with regard to longer intervals of time and larger social units than is customary. The editorial board includes R. Bendix and G. C. Homans.

Correspondence should be directed to the Editor, Sylvia L. Thrupp, Box 22, Faculty Exchange, University of Chicago, Chicago 37, Illinois.

Hamline University.—Robert Ray Martin has retired as head of the department and has resigned as editor of the *Alpha Kappa Delta*.

F. James Davis, formerly of the College of Wooster, has been appointed head of the department. He is the co-author of *The Independent Study Program in the United States*, recently published by Columbia University Press.

Leland R. Cooper is serving on the Council of the Midwest Sociological Society.

Juliana Schmidt has completed a two-year term as president of the Minnesota Conference on Social Work Education.

The Department of Sociology is initiating a study to determine the type of preparation for graduate study in sociology now considered desirable by American graduate departments.

McGill University.—A seminar was held in Montreal on November 9, 1957, to commemorate the seventieth birthday of Carl Addington Dawson, pioneer sociologist of Canada and chairman of the Department of Sociology and Anthropology, 1922-52. William A. Westley, present chairman of the department, presided, and papers were read by Everett C. Hughes, Oswald Hall, Forrest Laviolette, and Nathan Keyfitz. Many sociologists, colleagues, past students, and friends, from out of town as well as from Montreal, attended the afternoon session and the informal dinner in the evening, at which Helen MacGill Hughes and Harold Fyles were the speakers.

University of Minnesota.—Professor Theodore Caplow has completed a study of faculty

mobility among major universities under a grant from the Fund for the Advancement of Education. The results of the study will be published by Basic Books, Inc., as *The Academic Marketplace* early next year. Dr. Reece McGee, now of the University of Texas, is co-author.

Mr. George Donohue has been appointed extension rural sociologist with the Agricultural Extension Division.

Mr. John Forster, formerly of the University of California at Los Angeles, has joined the staff as an instructor in the Departments of Sociology and Interdisciplinary Studies. During the past year he has been on the island of Maui, conducting a study on Hawaiian acculturation.

Professor Reuben Hill, formerly of the University of North Carolina, joined the staff of the department in August. Professor Hill's appointment as professor of sociology and child welfare was made possible by a grant from the Louis W. and Maud Hill Family Foundation to be used in an expansion of the teaching and research program in family life at the University of Minnesota. In addition to his teaching and research duties, he has been named director of the Minnesota Family Study Center. Professor Hill has accepted an invitation of the International Sociological Association to prepare the "Trend Report on Research in Marriage and Family Behavior, 1945-56," to be published by *Current Sociology* in 1958. He interpreted sociological research to the Round Table on Law and Family Stability of the International Association of Legal Science at the University of Chicago in September. In December, Dr. Hill consulted with the staff of the Social Science Research Center, the University of Puerto Rico, and with government officials about the implications of his recently completed research with J. Mayone Stycos, Cornell University, and Kurt W. Back, University of North Carolina, on family and population control.

Mr. Charles W. Martin is family relations specialist with the Agricultural Extension Service.

Professor Arnold M. Rose returned after a year as Fulbright Professor at the University of Rome. He is editor of *Institutions of Advanced Societies*, to be published by the University of Minnesota Press. The social structures of ten "developed" countries are described by sociologists living within those countries.

University of New Hampshire.—Richard Dewey, professor of sociology at the Univer-

sity of Illinois, has been named professor and chairman of the Department of Sociology. Professor Dewey will succeed the late Professor Raymond E. Bassett, who died last winter.

A member of the Illinois faculty for the last ten years, Dr. Dewey received the Ph.D. degree from the University of Wisconsin. He has taught at Butler University in Indianapolis, Lawrence College at Appleton, Wisconsin, and Aurora College. He is the author of *Introduction to Sociology* and is co-author, with W. J. Humber, of *The Development of Human Behavior*.

University of New Mexico.—The Air Force Office of Scientific Research (Air Research and Development Command) will continue to support the research program in the behavioral sciences initiated by the University during the summer of 1957.

The immediate objective of the program is to assemble for conference a group of behavioral scientists with interest and demonstrated ability in interdisciplinary research. These individuals will concern themselves with the advancement of theory and methods in the behavioral sciences. During the course of this effort it is expected that they will develop some research proposals and designs of importance to the Air Force.

During the conference participants will devote their efforts to thinking through research plans and strategies and to developing interdisciplinary approaches to problems related to the development of an integrated behavioral science. Empirical work (experimentation with subjects) will be held at an irreducible minimum.

Participants will assemble in Albuquerque at the University of New Mexico for an eight-week period from June 16 to August 9, 1958. Approximately twenty-five to thirty individuals will be selected from the areas of psychology, sociology, anthropology, philosophy, economics, statistics, behavioral physiology, psychiatry, and related fields. The following criteria will be utilized in weighing the qualifications of applicants: In general, they should be comparatively young, with the Doctor's degree, of demonstrated research ability, and possessing strong motivation for co-operative interdisciplinary research. Applications from individuals already organized or contemplating organization into interdisciplinary work teams from the above fields will be particularly welcomed. Interested scientists qualified by training and research ex-

perience in the behavioral sciences are invited to write and request application forms from Dr. Ralph Norman, Principal Investigator, Department of Psychology, University of New Mexico, Albuquerque, New Mexico.

Social Science Research Council.—The Council is now able to announce several additions to its programs of fellowships, grants, and other appointments to be offered during the current year. The Councils' "Supplementary Announcement" describes predoctoral fellowships for completion of Ph.D. dissertations, travel grants for attendance at the 1958 Congress of the International Association of Applied Psychology, and three postdoctoral research training institutes to be held next summer. Topics of the institutes are "Analysis of Electoral Behavior," "The Judicial Process," and "Simulation of Cognitive Processes."

The "Supplementary Announcement" may be obtained by writing to the Social Science Research Council, 230 Park Avenue, New York 17, New York.

Society for the Psychological Study of Social Issues.—The Society announces a program of grants-in-aid for research on desegregation. A total of \$2,500 has been made available for such awards, but no single grant will be made in excess of \$1,000.

Recipients of grants are requested to submit to the Society two copies of any study resulting from research done under the grant and to acknowledge the source of the support when publishing research results.

A committee of judges, composed of Drs. Gordon Allport, Thelma Alper, Daniel Levinson, Nathan Maccoby, and Robert Chin, chairman, has been appointed to evaluate applications. Applications specifying budgetary needs and giving sufficient detail to make possible an evaluation of the feasibility and desirability of the proposed project must be submitted to the committee chairman (Human Relations Center, Boston University, Boston, Massachusetts) before May 1, 1958. It will be helpful if applications are made out in quintuplicate.

Syracuse University.—A grant of \$15,000 has been made to the University's Maxwell Graduate School of Citizenship and Public Affairs by the Fund for the Advancement of Education to initiate a program survey that should contribute to the better preparation of college teachers of social science.

Dr. Roy A. Price, director of the study, announced that the funds will be used for a self-survey of the Maxwell School's Doctor of Social Science program and an appraisal of this doctoral program by a group of consultants from higher education. The planned study calls for an evaluation of the program's organization, content, administration, policy, dissertation requirements, and over-all effectiveness.

The Twentieth Century Fund.—The Fund has announced a research project on "Time, Work, and Leisure." The directors of the study are August Heckscher, director of the Fund, and Sebastian de Grazia, visiting fellow of Princeton University. Thomas C. Fichandler, research associate of the Fund, is associate director.

University of Washington.—Delbert C. Miller has resigned to accept a professorship at Pennsylvania State University.

Charles E. Bowerman has accepted an appointment as professor at the University of North Carolina.

Earle H. MacCannell, who received his Doctor's degree during the summer, is now assistant professor at San Diego State College.

Maurice Donald Van Arsdol, another recent Ph.D., joined the Department of Sociology of the University of Southern California this fall as assistant professor.

Calvin F. Schmid attended a joint meeting of the International Statistical Institute and the International Union for the Scientific Study of Population in Stockholm, Sweden, under a grant from the National Science Foundation.

After a six-month leave of absence as research sociologist with the Transportation Center of Northwest University, L. Wesley Wager has returned to the department as assistant professor in charge of the courses in industrial sociology.

Norman S. Hayner was elected chairman of the Committee on Crime and Delinquency of the Society for the Study of Social Problems.

Robert E. L. Faris will be on leave during the spring quarter to visit major cities in Europe, where he will consult with research sociologists.

William R. Catton, who was at the University of North Carolina last year, has returned to the department as assistant professor.

Mason Griff, formerly of Wayne University, is now an instructor in the department.

Clarence E. Schrag, who served as director of Adult Correctional Institutions of the State

of Washington during the last three years, returned to the department in October as associate professor.

Philip M. Hauser, of the University of Chicago, has received a Walker-Ames Professorship for the 1958 summer session. He will teach a course in population problems in underdeveloped areas and a seminar in advanced demography.

Andrew W. Lind, of the University of Hawaii, also will be a visiting professor during the summer session. He will give courses in race relations and social control.

William E. Noland, of the University of North Carolina, was a visiting professor during the 1957 summer session.

The Washington Public Opinion Laboratory has launched a major research program on measuring human value systems under George A. Lundberg and Stuart C. Dodd, with William R. Catton as project director. This "Project Concord" is financed for three years. Several research assistantships providing Ph.D. thesis opportunities will be open in 1958.

Calvin F. Schmid received a grant of approximately \$20,000 from the United States Office of Education for a research project on the "Effects of Population Trends and Social Change on Educational Institutions in the State of Washington." Vincent A. Miller is principal collaborator on this project.

Wayne State University.—Donald C. Marsh is acting chairman during the absence of Edgar A. Schuler. Mr. Schuler is on a Fulbright grant to Thailand.

Harold Sheppard is in Paris, France, on a Fulbright research grant.

Mel Ravitz is acting director of the newly created Urban Planning Curriculum. This curriculum leads to the degree of Master of Urban Planning.

Louis Ferman has been assigned to the department and to the Institute of Industrial and Labor Relations. He is directing the field study of the adjustment of Packard workers after the closing of the Detroit plant of the Packard Motor Company.

Gabriel and Bernice Kaplan Lasker are engaged in village studies in Peru. The Laskers received joint Fulbright research grants in anthropology.

The People of Panama by John and Mavis Biesanz will be published in a Spanish edition by Editorial Letras, Mexico. *Modern Society* by

the Biesanzes is now in press in a Spanish edition with Editorial Letras.

Meyer Barash, formerly with Hofstra College, joined the staff in the fall.

Denez Gulyas and Donald Stewart have been appointed predoctoral teaching fellows in sociology.

Eleanor Wolfe, doctoral candidate in sociology, has been appointed temporary instructor in sociology.

H. Warren Dunham has rejoined the staff after spending a year in the Netherlands as a Fulbright lecturer.

Edward C. Jandy has returned from Pakistan after spending a year as Fulbright research scholar.

Victor A. Rapport, dean of the College of Liberal Arts, and Albert J. Mayer are jointly teaching a course in urban ecology.

August Kerber (educational sociology, College of Education) and Leonard W. Moss are offering an intercollege seminar in field studies.

Stephen C. Cappannari and Leonard W. Moss will offer a culture-area course dealing with Italy in the spring semester.

Carl Butts will introduce a new course, "The Sociology of the Arts," in the spring semester.

Leonard W. Moss has been appointed to the Policy Planning Committee of the Detroit United Community Services and to the Italian Social Service Committee of the International Institute.

The department acted as host for the visit of eighteen Italian teachers of English who visited Detroit on an educational tour of the United States. The department also entertained Bianca Maria Tedeschini-Lalli and Leda Barbarossa of the Italian Fulbright Commission (Rome).

The University's College of Education and Graduate School again approved credit arrangements in connection with the Eleventh Annual European Travel Study Program in Comparative Education. Personally directed by Dr. William Reitz, professor of education, the group will leave Detroit on June 18 and return on August 28, 1958.

Visiting twelve countries during the seventy-two-day journey, this program is designed to provide teachers, students, and other professional people with an opportunity to survey selected highlights of the life and culture of western Europe. There are almost three hundred alumni of the last ten programs.

Further information may be obtained from

Dr. William Reitz, 727 Student Center, Wayne State University, Detroit 2, Michigan.

University of Wisconsin.—Professor Emeritus John L. Gillin continues his pattern of regular hours in his office in the department.

Professor Howard Becker has received a grant for a study of a youth organization affiliated with an American secret society.

Professor Marshall B. Clinard is now engaged in writing a study of delinquency and crime in Sweden. He has in preparation a monograph on public drinking houses and their functions. In November, Professor Clinard was a participant in the National Workshop on Community Research Personnel sponsored by the Lilly Foundation, Inc., and various social work agencies.

Professor William H. Sewell has returned after a year in India as professor of social science research methodology at the Delhi School of Economics, University of Delhi. While in India, Professor Sewell also conducted seminars on research methodology and was a consultant to social science research at Bombay and Poona universities. His work was sponsored by the Government of India Planning Commission, the Ford Foundation, and the Institute of International Education. Professor Sewell has been appointed consultant to the National Institutes of Health and a member of the Mental Health Study Section of the National Institute of Mental Health.

Associate Professor Hans H. Gerth, who was on leave of absence at the University of California, Berkeley, was hurt in an automobile accident following the American Sociological Society meetings in Washington, D.C., but has now resumed his normal routine. His research activities at present involve a study of self-hatred of revolutionary intellectuals during the nineteenth century and another study on the stylization of reality in trials of political justice from Socrates to Senator McCarthy. Among his forthcoming books are *Marx versus Bakunin: The Minutes of the Hague Conference of the First International, 1872* and a translation of Max Weber's *The Religion of India*.

Associate Professor William S. Laughlin is pursuing research on the prehistoric inhabitants of Oklahoma, Aleut and Eskimo skeletal remains, and the skeletons of the Arikara Indian.

He is also engaged in a research project with Professor David A. Baerreis under the sponsorship of the National Science Foundation.

Assistant Professor David Ames has recently returned to the department from two years of field research on the Menomoni Indian Reservation in connection with the University's effort to assist that tribe in preparing plans for the termination of federal supervision.

Assistant Professor Louis H. Orzack is pursuing research in role orientations of several professional groups and on occupational choice. He served as an invited participant in the National Nursing Research Conference held at Community Studies, Inc., Kansas City, Missouri, September, 1957.

Assistant Professor Norman B. Ryder is pursuing research on the methods of measurement of temporal variations in vital processes, the construction of models of demographic transition, and time as a variable in fertility and nuptiality patterns.

Assistant Professor Lyle W. Shannon has returned to the department from a leave of absence during which he taught at Wayne State University.

Robert L. Fulton and R. Robb Taylor, from Wayne State University and the University of North Carolina, respectively, have taken up their duties as acting instructors.

Morton Rubin has left the department for a position at Northeastern University; Robert L. Carneiro has been appointed to a curatorship in South American ethnography at the American Museum of Natural History; and Donald W. Olmsted is now at Michigan State University.

Yale University.—The sixteenth annual session of Yale University's Summer School of Alcohol Studies will be held June 29–July 24, 1958. An interdisciplinary study of problems of alcohol and alcoholism in society, the program will include workshops and lectures and seminars under the direction of specialists drawn from the social sciences, medicine and psychiatry, religion, education, and public health. Enrollment is limited to two hundred students. For a prospectus describing the course and information concerning admission and academic credit write to Registrar, Yale Summer School of Alcohol Studies, 52 Hillhouse Avenue, Yale Station, New Haven, Connecticut.

BOOK REVIEWS

Science in the Federal Government: A History of Policies and Activities to 1940. By A. HUNTER DUPREE. Cambridge, Mass.: Belknap Press of Harvard University Press, 1957. Pp. x+460. \$7.50.

This work, sponsored by the National Science Foundation and the American Academy of Arts and Sciences, is the first attempt to describe the relations between science and the American government from the time of the Constitutional Convention to the eve of World War II, when these relations become so large and complex that a separate volume would be needed to treat them. As a path-breaking history, Dupree's book deserves high praise. Not only does it bring together the large mass of already published material but it adds important information from archival sources. Dupree takes pains to point out, however, that "the history of science in the United States is still largely unwritten" (p. v). We may hope that further historical studies will soon fill up the gaps and make the larger picture more accurate.

Dupree's history illustrates two major themes: the first is functional—the increasingly essential contribution of science to the activities of the United States government; and the second, organizational—the difficulty of finding a place for science in the structure of the government in which it could be effectively used and still retain that degree of autonomy essential to its continued development. As recent events connected with scientific manpower shortages and with the launching of Russian earth satellites have shown, this difficulty has not yet been entirely overcome. Shall we one day, perhaps, have a Secretary of the Department of Pure and Applied Science sitting in the President's Cabinet? Will his responsibilities include social as well as natural science?

The primary mode of organization of Dupree's book, despite these two themes, is "historical" or chronological. For the sociologist with an eye for his own kind of problem, however, there is much incidental material bearing on things sociological. For example, the reciprocal relations, or interdependency, between science and government; the intertwining of natural and social science in government scientific activities

(when shall we have a sociological history of social science in the federal government?); the varying amounts of knowledge of science possessed by our top political leaders (our earliest leaders knew more of science than most of our later ones); the influence of political ideologies—such as "strict construction" of the Constitution—on government use of science; patterns of unintended and even surreptitious growth of scientific agencies in government; the functions of informal organization among scientists for making formal organizations work (there is some fascinating material on an informal elite group that called themselves the "Scientific Lazzaroni"); controversy, politics, and preferment within science; the social responsibilities of science in a democratic society; problems of the most effective relations among government, university, industrial, and foundation-sponsored science; and, lastly, the nature and functions of co-operative interdisciplinary research in government.

Everyone interested in the relations between science and government should read this book. And that ought to include a good many sociologists.

BERNARD BARBER

Barnard College
Columbia University

Social Theory and Social Structure. By ROBERT K. MERTON. Rev. ed. Glencoe, Ill.: Free Press, 1957. Pp. xviii+645. \$7.50.

The eight years that have elapsed since the original publication of this volume of theoretical and research essays have only confirmed Professor Merton's position as one of the most pre-eminent of the present generation of American sociologists. The qualities on which this pre-eminence is established are again clearly revealed in the new materials of this revised edition: "Contributions to the Theory of Reference Group Behavior"; "Continuities in the Theory of Reference Groups and Social Structure"; and "Continuities in the Theory of Social Structure and Anomie." All show the rare ability to render explicit and ordered fundamental ideas

all too often implicit and unordered; to relate and extend the meanings of apparently unrelated pieces of sociological research; and to codify both research procedures and theoretical ideas. Merton's capacity for modifying theory during the course of the research process, as the modes of classification and analysis provided by the original theoretical orientation prove unfruitful, appears in "Patterns of Influence: Local and Cosmopolitan Influentials." And, finally, in a "Bibliographical Postscript" attached to "Puritanism, Pietism, and Science" (first published twenty years ago) he makes use of the meticulous scholarship characteristic of his work in historical sociology to show that the continuing analysis of historical evidence adds support to the hypothesis of a causal connection between the ascetic orientation of Puritanism and the development of science.

The essays on anomie and on reference-group behavior illuminate the meaning of Merton's phrase, "theories of the middle range," by which he described his own theoretical efforts in the Introduction to the first edition, and which remains unchanged in the second. In a footnote to the Introduction the concepts in such theories are described as involving a "middling level of generality: that they are specific enough to be effectively utilized in organizing the evidence bearing upon determinate ranges of social phenomena and general enough to be consolidated into increasingly broader sets of generalizations" (p. 10). This would seem to suggest that the concepts designate categories of phenomena at some middle level of abstraction. Yet the theory of anomic behavior is cited as applying to a wide range of observable phenomena, and the conclusion is drawn that, "in terms of the general conception, *any* cultural goals which receive extreme and only negligibly qualified emphasis in the culture of a group will serve to attenuate the emphasis on institutionalized practices and make for anomie" (p. 181). So also is reference-group behavior found to be an aspect of all phases of group life. Thus, while such theories are capable of ordering a wide range of research, they also point directly to the highest possible level of theoretical abstraction. "Middle-range theory," then, must mean not a middle level of abstraction but rather theory at the highest level of abstraction in dealing with social systems but concerned only with selected aspects of those systems. "The broader sets of generalizations" into which such theories are to be consolidated then become not

more abstract theories but simply bodies of integrated, middle-range theories. Such theorizing involves not even the temporary abandonment of the search for an "integrated conceptual scheme" but a different way of conducting the search.

In this light Merton's approach to social theory is liberating, not restrictive. No single problem in theory is prohibited, but the theorist is not required to solve all problems at once. Furthermore, the focus on the single problem permits him to keep in touch with the relevant research and discern its relevance for the problem at its highest level of generality. Is this not, indeed, what Merton himself has accomplished with respect to each of the special problems with which he has concerned himself?

WILLIAM L. KOLB

Tulane University

The American Class Structure. By JOSEPH A. KAHL. New York: Rinehart & Co., 1957. Pp. xviii + 310. \$4.50.

This book has been criticized for not being a general text in social stratification. This is hardly legitimate ground for censure. The work is exactly what the author claims for it: an analysis of the American class structure. And it is an excellent one at that.

Professor Kahl has drawn upon community and society-wide studies to produce a description of contemporary class structure in the United States. His analysis is organized around six variables, or dimensions of class: positional prestige; occupation; possessions or style of life; interaction; class consciousness; and value orientations. One chapter is devoted to each of these six dimensions. In addition, there is a chapter on ethnic and race barriers and two chapters on mobility—one from the point of view of the occupational base, the other covering motivation and education. Among the features which recommend the book is the use of methodology. A critical review of the methods used in studying class structure is woven in wherever research results are discussed.

Kahl has a healthy respect for data and the ability not only to write a coherent sentence but to turn a good phrase. His book is short enough to be admirably suited for teachers of undergraduate courses who like to use more than one textbook. It presents complex data in

a fashion so lucid that the beginner in the field can understand, and it offers observations worth the time of the professional scholar. (His comments on the "situs" idea, for example, in a footnote on page 89, are as sensible as any I have seen.) The chapter on income, wealth, and style of life is especially fine. No student of social organization can afford to ignore this book.

RAYMOND W. MACK

Northwestern University

Social Characteristics of Urban and Rural Communities, 1950. By OTIS DUDLEY DUNCAN and ALBERT J. REISS, JR. New York: John Wiley & Sons, 1956. Pp. xviii+421. \$6.50.

This volume, jointly sponsored by the Bureau of the Census and the Social Science Research Council, is one of a series of monographs based on the 1950 decennial census. Four independent variables are studied as determinants of differences among communities: community size, location with respect to dominant center, community growth, and functional specialization. The work examines the effect of variation in each one upon certain dependent variables (e.g., marital status, labor-force participation, race, and income).

Despite its ponderous prose style, this study is an extremely sophisticated piece of research and should be exciting for every serious student of urban life. The authors set a high standard for other works in this series. Each finding is carefully examined to determine alternative explanations, and these in turn are subjected to empirical test. The limitations of the study are the limitations of existing census data.

Dedicated to William F. Ogburn, the study is faithful to his model of scientific research. The prefatory statement of theory reduces to the view that the four independent variables will show high correlations with other measures of social and economic behavior. There is little speculation; explanations are limited to the area under investigation and not imported from other fields; and no general analytical scheme subsumes the findings. Such methods can be either deficiencies or virtues. This research in the area of demography and ecology suggests that such low-level empiricism can be extremely valuable.

Beneath the grand design certain additional features should be noted. The Appendix lists an

improved functional classification of cities which provides basic data for other researchers. There are interesting analyses of the rural-urban fringe and a test of the Zipf rank-size rule. In addition, the authors note the deficiencies in current census classifications when they attempt to manipulate them.

This book is too good an example of its type merely to be used by specialists. Chapters i and ii can be assigned as supplementary reading without subjecting undergraduates to the detail of later analyses. These chapters provide a good overview of the findings. The students will not enjoy the assignment, but it will provide useful therapy for those who think of sociology as either common sense or vague speculation.

SANFORD M. DORNBUSCH

Harvard University

Population Redistribution and Economic Growth: United States, 1870-1950, Vol. I: Methodological Considerations and Reference Tables. By EVERETT S. LEE, ANN RATNER MILLER, CAROL P. BRAINERD, and RICHARD A. EASTERLINE. Philadelphia: American Philosophical Society, 1957. Pp. xix+759. \$5.00.

I have neither weighed nor measured this ponderous volume. Yet I doubt that its awesome size overrepresents the importance of the achievement it reports. The construction of an eighty-year (1870-1950) series of net intercensal migration for states, by age, nativity, and race, is itself a substantial accomplishment. But the volume also includes a like series for the labor force of each state, by age and sex; a set of series on manufacturing for states, including number of plants, average number of wage-earners, total wages paid, value of products, value added, capital invested, and other pertinent data; and, finally, a set of shorter series (1880-1950) on income, by states.

Those who are prone to criticize social scientists for their lack of historical interest and those, too, who minimize the importance of continuity of census definitions would do well to contemplate the contents of this volume. The enormous number of man-hours and the numerous methodological expedients required to compile each of what are, after all, very particular and limited statistical series should give pause to any who treat the historical problem lightly.

This is not to suggest the presence of any important methodological weakness. The authors are alert to the problems involved. They use the best techniques available, describe their applications fully and lucidly, and present a fair appraisal of the quality of each of the many estimates they are forced to make. Perhaps the most debatable matter concerns the choice of method for estimating net migration. The selection of the forward survival method, in preference to the life-table method, despite evidence of errors in census enumeration, is based on the lack of satisfactory life-table data for individual states. A comparison of results obtained with the two methods for the country as a whole reveals small differences in most age groups. An interesting use is made of state-of-birth data to check the results of the forward survival method and to indicate the directions of migration.

Although most of the methodological discussions cover familiar ground so far as the professional demographer is concerned, he will find the volume highly useful as a reference source for graduate students. The real contribution to the working scholar is to be found in the reference tables, in which the final products of the work are set forth. No doubt these will serve as points of departure for years to come. In the meantime, one awaits with anticipation the substantive volume to follow in which presumably the data developed in this volume will be put to use.

AMOS H. HAWLEY

University of Michigan

Bureaucracy and Society in Modern Egypt: A Study of the Higher Civil Service. By MORROE BERGER. Princeton, N.J.: Princeton University Press, 1957. Pp. xiii+231. \$4.75.

This pioneer work of empirical sociology in contemporary Egypt merits attention as a model of intelligent design, careful analysis, shrewd commentary, and lucid exposition. Professor Berger is unduly modest when he describes himself as "insensitive to romance and mystery." He is sensitive enough to make others' romance and mystery his own data. Thus he ferrets out of the sensationalized discovery in 1954 of a solar ship buried some five thousand years ago near the great pyramid of Cheops an "administrative subplot." This he makes into a concise

vignette illustrating some of the principal conclusions of his study.

The heart of the matter is a set of 249 long questionnaires which Berger administered to Egyptian higher civil servants in 1954, framed by a historical analysis of the past predicaments and present puzzlements of man's oldest recorded bureaucracy. The questionnaire contained sixty-two principal items and a large number of subsidiary or probe items. These varied from requests for routine face-sheet data to subtly engineered projective questions. In separate appendixes are given Arabic and English texts of the questionnaire, the covering letters, composition of the sample, and notes on construction of the scales and indexes used in the analysis.

The analysis proceeds by systematic comparison (1) of Egyptian with Western bureaucracies and (2) of groups within the Egyptian bureaucracy—groups differentiated by age, function, degree of exposure to Western models, etc. The range is indicated by the chapter headings: "Attraction of the Educated Man to the Civil Service"; "Changes in Status and Prestige"; "Loyalties: The Professional and Private Worlds of the Civil Servant"; and "Initiative and Subservience: The Range of Bureaucratic Behavior." The findings are interpreted with scrupulous attention to the limits of the data but, happily, since Berger is not really insensitive to the larger questions, with one eye on interpretations suggested if not established.

Thus, Berger may one day draw from his own data an illuminating essay for reference-group theory. His comparisons between Egyptian bureaucrats by degree of exposure to the West show this index to discriminate regularly and significantly on key questions. On the important question of what the qualifications for civil service posts should be, 83 per cent of those respondents educated in the West said that education and experience should be the *only* criteria, while nearly half of those respondents educated in Egypt said that "other factors" should be taken into account (Table 42). The greater confidence of the Western-educated in objective and impersonal standards and institutions recurs consistently in their expressed opinions—and, indeed, in their behavior. When asked how they learned about their first civil service job, 73 per cent of those scoring high on the "Exposure scale" said "through institutions" as compared with 58 per cent of the

low scorers (the rest of whom said "through personal contacts").

The more Westernized elements rely more heavily on mass media than oral networks for their information on public events; they judge themselves and others by more modern objective standards of conduct. They live out their lives, in short, with an image of the West as an important reference group. That Berger manages to convey so much on such complex matters in so little space is a tribute to his skill in index formation and his powers of concise exposition.

One could wish that he had given us a longer book. He does not discuss current Egyptian politics, yet his political acumen is clear from his concluding sentence: "The military regime has really been seeking to create a class to represent." Anyone with enough savvy to write this sentence owes his readers a political sociology of Egypt today. This is not to diminish the value of what *is* in the book. It is rather to express the hope that what is missing will soon be supplied in another book from the same hand.

DANIEL LERNER

Massachusetts Institute of Technology

The Economics of Discrimination. By GARY S. BECKER. Chicago: University of Chicago Press, 1957. Pp. x+137. \$3.50.

A brilliant young economist here sets forth a model based on the assumption that individuals act according to different "tastes for discrimination." The "discrimination coefficient" measures the value placed on the non-pecuniary cost of a transaction with a member of a group with whom one prefers not to associate. Relating discrimination coefficients to the economic analysis of market-price determination permits one to make inferences as to the relationship between the practice of discrimination and other variables—inferences of the kind that social psychologists have attempted by correlating direct measures of prejudice with other individual characteristics.

The author has some trenchant critical remarks about assumptions and hypotheses set forth in writings on the social psychology of discrimination. He seems less well acquainted, however, with demographic studies pertinent to his problem. For example, his analysis of

data on the economic position of Negroes in terms of occupations is naïve as compared with Ralph Turner's work on occupational patterns of inequality.

An important distinction is developed in the study: "Market segregation and market discrimination are separate concepts referring to separate phenomena." Each can occur without the other, though they often occur together. To study the interrelations of the two, one must, of course, recognize the difference. Unfortunately, the author does not really come to grips with the index-number problem involved in attempts to measure segregation.

Although a number of challenging conclusions are given in fairly accessible form, the reasoning which develops them can be followed only by readers thoroughly versed in the tools of economic analysis.

OTIS DUDLEY DUNCAN

University of Chicago

Leadership in Administration: A Sociological Interpretation. By PHILIP SELZNICK. With a Foreword by CLARENCE B. RANDALL. Evanston, Ill.: Row, Peterson & Co., 1957. Pp. xii + 162. \$4.00.

"The executive becomes a statesman as he makes the transition from administrative management to institutional leadership." This is Selznick's thesis, and its explanation consists of theoretical guide lines for organizational diagnosis. The essential distinction between the manager and the leader, Selznick argues, lies in the fact that the latter can recognize the difference between routine and critical decisions as well as the difference between an expendable organization (a technical instrument for task performance) and an institution (a value-infused organism with distinctive character and goals).

To be a leader, of course, is to be able not only to recognize these distinctions but to accept the responsibilities they imply—to become "an expert in the promotion and protection of values." Selznick encourages the executive to assume "the posture of statemanship," since, in his view, "sheer organizational achievement" is not an adequate criterion of success. Thus, two substantial chapters in this work seek to clarify the statesman's major tasks: the definition of the organization's mission and role

and the embodiment of these goals in institutional form.

The book is apparently intended as a popular work in the sense that it appeals more to the administrator than to the professional sociologist. Thus one may ask: "Would you want your own executive officers to read it and take it to heart?" My answer to that is a qualified "Yes." The book combines an effective style with soundly reflective content, but there are important drawbacks: (1) a tendency to overplay the leader's personal responsibility for defining ends, along with a corollary tendency to underplay group participation and to speak of "desired ends" without specifying precisely whose ends are under discussion, and (2) a tendency to stress the conservative character of the leadership role—to overemphasize, for example, the ideals of institutional integrity, identity, security, and survival.

I am voicing qualms that have to do with possible side effects on the audience of executives. The audience with more specifically sociological interest will find much that is stimulating (I found the section on the autonomy of elites fascinating) and will develop qualms of a different nature. The analysis of social structure, for example, includes an unusual blend of elements which are, alternately, psychological ("dependency"), ideological ("beliefs"), and organizational ("assigned roles").

MELVIN SEEMAN

Ohio State University

German Rearmament and Atomic War: The Views of German Military and Political Leaders. By HANS SPEIER. Evanston, Ill.: Row, Peterson & Co., 1957. Pp. xi+272. \$5.00.

The social structure of postwar West Germany presents a most confounding paradox of continuity with the past combined with radical social change. Despite a considerable amount of personal mobility—both upward and downward—the underlying social stratification closely parallels the prewar period. After a decade of occupation, the key German institutions, such as education, public information, and governmental bureaucracy, have changed only slowly and display many of the central features of the pre-Nazi period. However, West German leaders have developed comparatively stable

political arrangements and have placed their country in the forefront of the European movement—both important breaks with past traditions.

Since Germany is the big stake in the cold war, problems of military security are continually at the center of German reconstruction. Thus, another major modification in traditional German orientations is the resistance—both political and psychological—to the policies of German rearmament.

Hans Speier traces here the reactions of the military and political elites in West Germany to Allied policies designed to bring about rearmament. To what extent, he asks, has the continuity and discontinuity of German political and military institutions influenced the tortuous process of remilitarization? Speier, as head of the Social Science Division of Rand Corporation, is perhaps uniquely equipped to analyze the sociological components in the military defense of western Europe. He has been one of the few sociologists in the United States who has concerned himself with the study of military institutions and who has emphasized the consequences of military affairs on social change.

This volume is more than an analysis of social change in West Germany; it is a model of the potential contributions of sociological thinking to the tasks of assembling and analyzing data for the conduct of foreign affairs. Methodologically, Speier makes use of intensive interviews with a selected sample of German military and political leaders, who were not treated mechanically or handled as if they were consumers who might be induced to switch the color of their refrigerators. Formalized interviews could have produced little relevant information, since the very act of interviewing these people is a political act and since their responses are continually conditioned by their political expectations.

The German military leader has long been considered the epitome of the rational technician. Speier's analysis of the perspectives of the military elite reveals strikingly their reluctance or inability to orient themselves realistically to the implications of the new atomic weapons. The decimated ranks of the professional officer group seem frozen in their view of warfare as a mere extension of World War II. This is not to deny that, in West Germany, army officers still have some prestige and are still called upon for strategy and political ad-

vice. But as a group they are mainly concerned with re-establishing corporate identity; they have sought to close ranks by acknowledging that a German officer could have participated or could not have participated in the crucial July 20 attack on Hitler; today it should make little difference.

Thus the remains of the German officer corps, without having made even a nominal re-evaluation of their political past, and reluctant to concern themselves with the problems of atomic warfare, now find themselves subject to effective controls by civilian politicians and with little independent political power. (Speier does not present the orientations of a sample of the future—the younger officers who, in the next decade, will achieve positions of authority and influence. He does, however, report on the young reform elements, including such figures as Wolf Graf von Baudissim, who are in close liaison with the German youth groups and who have apparently achieved a definition of military service more compatible with a democratic political system.) In this process the control and direction of military policy seems to have shifted to political leadership groups from the south of Germany—from Bavaria. It is difficult to determine whether this is a political accident or, given the present German orientations toward remilitarization, could only have been accomplished by catering to regional forces and groups who oppose traditional forms.

The National Socialists have already demonstrated that the military can be displaced from independent political power. It remains to be seen if present German political leadership can make use of its military establishment as a positive contribution to western European stability and not as an instrument of traditional nationalism. Speier's evidence on the political perspectives of the German politicians seems to indicate that the chances are rather good, depending, of course, on the stability and relevance of United States foreign policies.

MORRIS JANOWITZ

University of Michigan

The Measurement of Meaning. By CHARLES E. OSGOOD, GEORGE J. SUCI, and PERCY H. TANNENBAUM. Urbana: University of Illinois Press, 1957. Pp. 342. \$7.50.

This important book contains six years' work on the objective measurement of meaning. It

reports some fifty studies done by some thirty workers at the Institution of Communications Research of the University of Illinois, of which Charles E. Osgood is now the director. Since these studies cover a wide range, the book will appeal to almost everyone interested in meaning and in communication.

The core of the book is the description, application, and evaluation of what Osgood calls the "semantic differential," though there is some consideration of the relation of this instrument to Osgood's view of meaning as a representational mediation process.

A semantic differential, as now used, is a device for exhibiting the meaning of something (word, picture, gesture, etc.) by indicating its position on each of a number of bipolar, seven-interval adjectival scales ("hot-cold," "pleasant-unpleasant," "hard-soft," etc.). Thus a number from 1 to 7 is obtained for a term such as "father," indicating how "hot" or "cold" it is, how "pleasant" or "unpleasant," etc., and these differentiations are taken as indexes of the meaning of the term. Positions on some scales go along with similar positions on other scales, and factor analysis reveals a number of dimensions of meaning, such as *evaluation*, *potency*, *activity*, *receptivity*, and *stability*. Hence a score can be found for the term "father" on each dimension and the meaning of the term assigned a point in the semantic space as defined by the various dimensions. Differences and similarities of meanings can then be expressed in terms of the numerical distances between points in the semantic space.

Only a reading of the book can convey a sense of the methodological care which has gone into the construction of various forms of the semantic differential and the power of such instruments in empirical research. Such an instrument makes it possible to study changes of meaning in individuals or groups, to compare meanings of various groups in and across cultures, to compare the meanings of a message to the sender and receiver, to study the relation of a compound meaning to the meanings of the units, and much else. The reports of studies on attitudes, on voting in the 1952 presidential election, on dream symbols, on a case of triple personality, and on some problems in experimental aesthetics attest, along with many others, to the sensitivity and fruitfulness of these experimental techniques.

The authors are well aware that their approach does not deal with all phases of mean-

ing (such as the purely denotative or referential phase) and that much remains to be done in relating the results obtained by the semantic differential to Osgood's theory of meaning as a representational mediation process. The book is presented as a progress report. But it is certain that this approach and these results make a major contribution to the science of signs.

CHARLES MORRIS

University of Chicago

The Dynamics of Interviewing: Theory, Technique and Cases. By ROBERT L. KAHN and CHARLES F. CANNELL. New York: John Wiley & Sons, 1957. Pp. viii+368. \$7.75.

Two psychologists associated with the Survey Research Center of the University of Michigan—Robert Kahn, program director, and Charles Cannell, head of the field staff—have collaborated to write a book “about the theory and practice of interviewing in a way that will be meaningful for both students and practitioners in . . . diverse fields.” The wide area covered is revealed in the chapter titles, which range from “The Interview as Communication” to “The Design of Questionnaires” or from “The Interview as a Method of Measurement” to “The Psychological Basis of the Interview.” Slightly more than one-third of the book, comprising Part II, is devoted to a presentation of tape recordings (unedited except in the interest of anonymity), representing various kinds of information-getting interviews—a medical interview, two personnel interviews, a supervisory interview, and a social work interview.

As a manual of practical interviewer training and technique, this book warrants high praise. Also, the amount of attention devoted to actual problems of interpretation of interview data as related to the psychological basis of the interview, respondent motivation, and especially problems of study design (questionnaire construction, organization, wording, etc.) is most gratifying to observe. From this standpoint the book is certainly to be recommended as highly to students of social research as to potential interviewers.

Throughout *The Dynamics of Interviewing* discussion, theoretical as well as practical, proceeds with a certain sophistication obviously born of much experience with, and reflection about, data secured through the medium of the

informational interview. But one feels that the writers have not done justice on the theoretical level either to their materials or to their own perceptions. Even a “piecing-together” of a series of the authors’ apparently routine and discrete observations on various facets of the interview process could lead to some very interesting theoretical formulations that are not to be found explicitly stated in this book.

An implicit assumption that researcher and interviewer are necessarily “one”—either in fact or in terms of the understanding of research objectives—obscures some of the emerging problems characteristic of public opinion polling as well as all large-scale social research. These situations contain no such simple dyadic relationship between a researcher-interviewer and a respondent as this discussion seems to visualize. Rather, the researcher becomes the study-architect (director) separated from the respondent by professional interviewers and coders. (The roles of the latter two are somewhat “schizoid” in that both interviewer and coder are often required to assume the roles of both researcher and respondent—and one another’s roles as well!) Each additional point in the chain of communication constitutes a possible source of misunderstanding and error. It also seems to be assumed throughout the book that the trained interviewer will surpass the respondent in understanding research objectives. Recent sociological investigations of “elite respondents” point toward special situations where this assumption breaks down. These are areas into which the theory of interviewing could be fruitfully extended. As it stands, however, the book is a welcome and valuable contribution to our theoretical understanding as well as to our understanding of technical research problems.

ELIZABETH JOHNS DRAKE

Chicago, Illinois

Womanpower: A Statement of the National Manpower Council, with Chapters by the Council Staff. By the NATIONAL MANPOWER COUNCIL. New York: Columbia University Press, 1957. Pp. xiii+371. \$5.00.

The Tables of Working Life for Women recently constructed by the Bureau of Labor Statistics show, among a number of interesting variables, that a cohort of 100,000 girls, born

alive and subject to the labor-force and mortality rates of 1955, would contribute a total of about 1.8 million man-years of work during their lifetimes, thus tripling the performance of their 1900 counterparts. This, perhaps, is as good a way as any of briefly indicating the context for *Womanpower*, the latest in a series of illuminating volumes on various aspects of the manpower problem in the United States emanating from Columbia University's National Manpower Council.

This book is divided into two parts: Part I presents the statement of policy and recommendations of the National Manpower Council itself; Part II, which makes up the bulk of the volume, presents chapters on various facets by the Council staff. The substantive part is organized into twelve succinct chapters which describe the basic trends, the current picture, and the outlook for the employment, labor-market status, education, and training of women, and some of the problems, both societal and individual, posed by their entry as a significant force in the manpower resources of the country.

Obviously, the emerging role of the woman worker is a major expression of some of the fundamental social and economic changes in the United States in the last fifty years, and this is clearly recognized and, in fact, basic to the Council's policies and recommendations. Thus, it urges a substantial and significant increase in the provision of education, training, and guidance facilities oriented toward the maximum sound development of womanpower; it calls for an end to sex discrimination in hiring, assigning, training, and utilizing personnel at all levels; and it very correctly stresses the need for much more research on the implications of all of this for many aspects of American life.

SEYMOUR L. WOLFBEIN

United States Department of Labor

The American Woman. By ERIC JOHN DINGWALL. New York: Rinehart & Co., 1957. Pp. vi+309. \$4.50.

Mr. Dingwall, an English anthropologist who believes his work to be the first of its kind, begins with a number of assumptions: that American middle- and upper-class women are dissatisfied, frustrated, resentful, and neurotic; that American society is a sex-obsessed, materialistic matriarchy; and that American men are henpecked and poor lovers.

The historical development that produced this situation is vaguely stated as a Puritanism gone sour—an industrial system where men reign supreme and an educational system dominated by women. However, none of these trends is studied intensively, and no rigorous documentation connects them with the position or attitudes of women.

No clear-cut solution is offered to women's problems, although one gathers that it would be a situation in which women would regain their femininity, a concept more or less equated with both sexual and social submission to men.

Dingwall states in his Introduction that he is attempting not a scientific and objective study but an analysis of various statements made about American women. As a result, the material presented is of extraordinary variety as to date of publication, sample, methods, and competence of the observer. Dingwall, however, not only fails to distinguish clearly the value of his material but also culls highly selective passages from standard works. For example, although he emphasizes the effects on women of the movies, the radio, and television, the only conclusion he draws from Merton's *Mass Persuasion* is that Kate Smith is (was) a mother figure. He also ignores a good deal of standard material on the history of women. Dingwall comments a great deal on advertisements, cartoons, and fashions, but the relevance of his comments are often obscure. (For example, he observes that women wear hats shaped like nests and that men wear ties that are obviously phallic symbols; but what relation this has to the main thesis is not evident.)

Most American sociologists would agree that American women do face serious problems in common with women in other highly industrialized societies. In general, the observations and insights of foreigners are valuable because things taken for granted in one culture are visible to the stranger. But this reviewer must conclude that this book has practically no original insights or useful material of the kind students of social problems expect.

CAROLINE BAER ROSE

Minneapolis, Minnesota

The Life and Work of Sigmund Freud, Vol. III: 1919-1939, The Last Phase. By ERNEST JONES, M.D. New York: Basic Books, Inc., 1957. Pp. xvi+537. \$7.50.

This is the third and final volume of Jones's monumental biography of the founder of psychoanalysis. Like the two preceding volumes, it is divided between a chronological account of Freud's life and an exegesis of his writings. Biographically, the concluding volume tells of the growing world acclaim, which came too late for Freud's personal satisfaction, and of his stoic fight against cancer and old age. Since the years covered include those of *Moses and Monotheism*, *Civilization and Its Discontents*, and *Group Psychology and the Analysis of the Ego*, the exegetical sections will be of particular interest to sociologists.

Kleidung, Mode und Mensch: Versuch einer psychologischen Deutung ("Clothes, Fashion, and Man: A Psychological Interpretation"). By FRANZ KIENER. Munich: Ernst Reinhardt Verlag, 1956. Pp. 280.

Clothing—its presence, its partial or total absence, variations throughout the world and through history—becomes a fascinating subject in these pages. Kiener writes with intelligence, sensitivity, and erudition in the best tradition of European sociological scholarship. In a time when elaborate claims for method unaccompanied by real investigations and creative findings masquerade as research it is a pleasure to read a work as methodologically unpretentious yet as substantively interesting as this.

Formally, the book is divided into two parts, the first general and the second devoted to modes of clothing and particular areas of the body. Thus Part II contains chapters on the head, neck, arm and hand, torso, loin and legs, and feet, while Part I deals with the following: (1) the "nakedness" of man as compared with animals and the various significances of nudity; (2) the historical relationships between clothing (and its production) and civilization; (3) clothing as protection; (4) shame and custom; (5) clothing and adornment; (6) color and form; (7) the psychology of materials; (8) essential differences (age and sex); (9) types of clothing; and (10) fashion.

Kiener's discussion of the differences between the sexes in their attitude toward clothing is provocative. He argues that women enjoy the autoerotic sensations of nudity (the play of sunlight and air, the feel of muscle and skin) much more than men. Women generally feel more casual toward body exposure than men,

who are more likely to regard clothing as protection and its removal a threat. Thus, what men may take for seductive exposure in women is simply masculine misinterpretation of feminine motives. (The difference between the sexes in their use of clothing as symbolic protection, Kiener says, is shown by the styles during the Reign of Terror. Men covered the neck with huge rolls of cloth; while women's décolletage became more extreme than ever.) Since women regard clothing more as adornment than protection, the removal of clothing has for them any of a complex number of meanings, including submission, defiance, seduction, and simplicity.

With the "New Look" and the "sack," we tend to be conscious of the irrational effect of fashion on women's clothes; the sharp switches that make ludicrous what was but a year ago the very point of elegance. Yet the tradition-bound masculine ensemble is equally irrational with its expensive and uncomfortable survivals, vests, sleeve buttons, hats, lapels, etc. Kiener has an interesting discussion also of why men, not women, tattoo themselves, and why women, not men, deform their bodies (e.g., Chinese footbinding, European corsets).

The book is subtitled "A Psychological Interpretation," and the author throughout is acute in his analysis of the role that clothing plays with regard to the ego. To this is added the occasional spice of a psychoanalytic interpretation. The author tends to be weaker in his treatment of the complex roles and social situations of modern society. There were numerous places where we felt that his analysis might with profit be pushed just a bit further. We think here of such matters as the influence of dress on the communication of role and the potential course of interaction or of dress as a way of modeling one's self after an ideal. But this is the mark of a good book—to stimulate the reader to continue the investigations of the author.

The book contains about a dozen pages of illustrations. These have been chosen with care and add much to the discussion. One striking series contrasts the great European architectural styles—Romanesque, Gothic, Baroque, etc.—with the concurrent styles of dress. Is it the glass house (and the picture window) or the rocket that is shaping modern styles?

MURRAY AND ROSALIE WAX

University of Chicago

Love in the Western World. By DENIS DE ROUGEMONT. Translated by MONTGOMERY BELGION. Rev. ed. New York; Pantheon Books, Inc., 1956. Pp. 336. \$4.50.

If a man tells you, as you open his book, that his "central purpose" is "to describe the inescapable conflict in the West between passion and marriage," that the documents he quotes and the juxtapositions he proposes "are intended to serve less as evidence than as illustrations," and that, in this second English edition of a book that first appeared more than fifteen years ago as *L'Amour et l'occident*, he has "replaced one or two wild flourishes of the pen by a little analysis" even though (he feels) that this worsens (his) case, how—I ask you and myself—should you look upon his case? With passing interest and amusement? As the declarations of a man whose main value and validity lie in whether they are true for him, and for you, if you (are) like him?

Amusement would seem disrespectful, for in the first of seven books that constitute the book, we are told that "happy love has no history." "Romance only comes into existence where love is fatal. . . . And passion means suffering . . . passionate love is a misfortune . . . and the community still drives passionate love in nine cases out of ten to take the form of adultery. No doubt lovers can invoke numerous exceptions. But statistics are inexorable, and they confute our poetic self-deception." In this instance, statistics are also absent. Instead, this is a book of poetry, "but to quote a poet," as M. de Rougemont says, "is either of no value or of too much."

Seven hundred years of the West are here described under the aspect of a distinction and a thesis which certainly deserve attention and refinement. M. de Rougemont is driven on by the transformations of an attitude which he presents to us at the outset through an account of the Tristan myth. There we are shown the logic of passion, its marriage to suffering, and its opposition to marriage. This opposition establishes its further link to a more general heresy. Passionate love from the twelfth century on—De Rougemont is suddenly exact and dates it with the year that Peter Abelard came to know Héloïse, in the biblical sense—became a religion rivalrous with Christian orthodoxy. Today we have lost the keys to this spiritual heresy, which includes not only the cultivation of passion but also varieties of mysticism and

even of warfare. ("There is a certain parallel between the love affairs of Bonaparte and then Napoleon . . . and . . . his campaigns in Italy and Austria.") Besides, the myth has become profaned. The accomplishment of this profanation De Rougemont celebrates for us by a literary journey which takes us many places: courtly love, Arabian mystical poetry, Meister Eckhart, the *Roman de la rose*, Corneille and Racine, Milton and Shakespeare, Don Juan and the Marquis de Sade, Hölderlin and Novalis, Stendhal and Wagner—we are reminded and given a taste of them all. The procession is instructive or suggestive. It keeps moving and so never wearies, but at times it almost makes one forget that a parade, however arranged and elegant, is not history and that the changing and competing forms in which we enact our relations toward one another cannot, by definition, be adequately represented through the presentation of a drift of ideas.

It is pointless to inveigh against De Rougemont's extravagance and unnecessary to argue the inadequacy of his thesis. We must assume that the admittedly competing ideals by which we constitute ourselves into different groupings of intimacy or impersonalness have to be understood in relation to *several* orders of fact. If we want a more sober account of the romantic and domesticated version of passion in relation to the exigencies of society, Hugo Beigel has provided a good start in the *Review* (June, 1951). But, aside from De Rougemont's particular religious commitments and undocumented diagnosis of the breakdown of modern marriage, he has provided excellent reminders of materials for those of us who want to know what characteristic varieties social relations can take.

KASPAR D. NAEGELE

University of British Columbia

Made in America: The Arts in Modern Civilization. By JOHN A. KOUWENHOVEN. Newton Center, Mass.: Charles T. Branford Co., 1957. Pp. xv+303. \$5.00.

The theme of this book—first published in 1948—is the dilemma of the American artist who works under the impact of two cultural traditions: (1) the "cultivated" tradition which to the author is European, and hence alien to America, and (2) the "vernacular" style slowly emerging from the native American scene. The

foreign norms in Greek, Gothic, and other architectural styles, the romantic novels, "court" music—all of which are "ornaments" to life—have been imposed from without and have captured a status of respectability. To most people "raising the cultural standards" means importing and disseminating more of the European arts. The immediate products of American civilization, of industry and technology, of democracy and the frontier, have enjoyed less prestige. This book is a contribution to the ideological struggle between the two schools of aesthetics.

The author embraces the vernacular taste and sees more beauty in the functional design of the George Washington Bridge or in a streamlined pressing iron than in the imitation Gothic of the Tribune Tower in Chicago. Whistler, Oscar Wilde, and Henry James, he argues, acted "sensibly" in transferring their residence to Europe, where they found a more congenial, effete atmosphere. But Ralph Waldo Emerson, Walt Whitman, Winslow Homer, Louis Sullivan, and the jazz composers cultivated vernacular simplicity, functionalism, and ocular verities. The author implicitly but unquestionably veers toward technological determinism, but he does not seem to appreciate its differential application to the various arts. The essence of the argument is expressed in Corbusier's paraphrase of an old romantic saying: the new skyscrapers of Manhattan (e.g., the McGraw-Hill Building) are "hot jazz in stone and steel."

From the sociological standpoint, this story, with all its tendentious social history, constitutes effective materials for a sociology of knowledge in the arts. But as is to be expected, it falls far short of the achievement of that destiny. We have no reason to believe that arts are limited to utility or that the American, Russian, or any other population will be satiated with the admitted enjoyment of the formal design of a bridge and eschew the decorative embellishments of cubist paintings, Italian opera, or cerebral music—all based on the institutionalization of leisure in modern life which no longer carries the Veblenesque derogation. Furthermore, although syncretism produces strange affinities, the passing anachronisms and incongruities injected by the processes of diffusions and acculturation are forgotten in a matter of decades.

The espousing of ethnocentric cultural revolt has occurred many times before. The study of

such cultural movements of the past would serve, methodologically speaking, as replications in a positivist sense. But there is no trace of such thought in this book—and, indeed, was not intended by the author. However, to make such materials useful to the sociologist, such supplementation is necessary.

This edition is well illustrated, handsomely printed, and documented with a bibliography of fine antiquarian sources.

JOHN H. MUELLER

Indiana University

Mass Communications: A Statistical Analysis.

By RICHARD E. CHAPIN. East Lansing: Michigan State University Press, 1957. Pp. viii+148. \$5.75.

We are presented here with a compilation of statistics on the newspaper, book, magazine, radio-television, and motion-picture "industries." For each an attempt was made to collect data on consumer usage and expenditures, number of producers, revenues, employment, investment, etc. For many of the tables "historical" series, mostly dating back to the late 1920's, are given. A useful and somewhat frightening feature is the author's evaluation of the accuracy and completeness of the figures for each table. Of the sixty basic types of data he sought to collect, only five are rated as "good" and twenty-two as "acceptable."

Social Problems at Midcentury: Role, Status, and Stress in a Context of Abundance. By JESSIE BERNARD. New York: Dryden Press, 1957. Pp. xi+654. \$5.75.

Analysis of "social problems" within the field of sociology meets with the difficulty that problems are concrete, whereas the sociological principles relevant to the explanation or alleviation of those problems are abstract. Traditionally, textbooks have been eclectic descriptions of various pathologies, and students have been forced to accept the limited understanding of the author and his instructor who are specialists. The more analytical books deal with selected aspects, giving the student something less than the full texture of the problems. If he wished deeper understanding, a student has had to take further courses in economics, psy-

chology, etc. Professor Bernard has succeeded to a major degree in bridging this chasm. She has attempted to remain within sociology by organizing the presentation around the concepts of role and status. In fact, this book might serve equally well as an introductory text.

Much of the freshness of discussion lies in her frank thesis that in this nation abundance is so general that the problems no longer lie in poverty and its derivatives but in "malfunctions" of role or status. The number and the felt seriousness of problems do not diminish because traditional problems relating to poverty are ameliorated.

Three types of role-related problems are dealt with: (1) some problems arise from impairment in role performances due to the actor's traits: disease, addiction, "stress diseases," mental deficiency, etc.; (2) there are problems arising from confusion in the role structure of the society even when the actors are adequate (e.g., confusion in feminine roles); and (3) there are also illegal roles and violations of institutionalized roles.

That portion of the discussion organized around the status dimension is especially acute, particularly in such sections as "the stresses of equality." A third main dimension used in structuring the materials is "stress" or threats to values.

Professor Bernard will not satisfy the profession in her attempts to cope with "the nature of a social problem." But her activist conception and her emphasis upon the criteria of humanitarianism, utilitarianism, and dysfunctionality are as serviceable as any alternatives.

The author has succeeded for the most part in remaining within the sociological perspective, adapting the materials from other disciplines to the chosen framework. The style is clear, though perhaps not within the grasp of the less able students. The layout and printing are beautiful.

C. ARNOLD ANDERSON

University of Kentucky

Collective Behavior. By RALPH H. TURNER and LEWIS M. KILLIAN. Englewood Cliffs, N.J.: Prentice-Hall, Inc., 1957. Pp. 547. \$6.95.

One may say with safety that few among us today would regard the subject of collective behavior to be in a satisfactory condition. So

long as courses are offered in the subject, however, textbooks are required—or at least deemed desirable—and it is to the creation of such a textbook that the authors of the present volume have dedicated their efforts.

There can be no doubt that in terms of certain pedagogical criteria they have achieved a commendable result. They have covered the ground with meticulous care and with a rigorous discipline. The materials run, as the Index shows, from "Abolition movement" to "Zoot-suiters," and in between one finds every subject remotely associated with the field. The five major divisions of the book deal, respectively, with (1) the nature and emergence of collective behavior; (2) the crowd; (3) the diffuse collectivity; (4) the social movement; and (5) the social consequences of collective behavior. Each division is introduced by a perceptive essay on the problem, and in most cases relevant conclusions are drawn.

The introductory chapter, which defines the field, is a competent piece of writing. The authors deal with sociology as a whole, with the development of interest in collective behavior, with the definition of collective behavior, and, finally, with recurrent issues in the field. It is a thoughtful chapter, and, clearly, Turner and Killian have given it careful attention and time. The book is admirably suited to the task for which it was designed.

Ordinarily, a reviewer could ask no more. Nevertheless, certain questions remain—questions prompted not by the norms of reviewing (which seem to require at least one negative comment) but by the character of the book itself. It must be said that, in a very important sense, Turner and Killian are not the authors of this book but only the editors. Most of this book is occupied not with the systematic development and exposition of a theory of collective behavior, with illustrations supplied and described by the authors themselves, but rather with extensive quotations from the writings of other men. Some of these quotations are so extensive as to be excerpts. Although the bibliographic references are complete, the authors of these contributions are not identified, and they receive listing neither in the Table of Contents nor elsewhere—except as alphabetized names in the Index. In short, the book is the work of many, many hands, not two or four; and, however well the editors have accomplished what they set out to do, there are certain limitations inherent in the form they have chosen.

This is not the place to expatiate upon these limitations. Many members of our profession have chosen this form of presentation, and it is one not wholly devoid of merit. But it has disadvantages, too, and one is inclined to wonder whether in the long run books of this kind can qualify as substantial contributions to the literature of sociology.

ROBERT BIERSTEDT

City College of New York

Clues to Suicide. Edited by EDWIN S. SHNEIDMAN and NORMAN L. FABEROW. New York: McGraw-Hill Book Co., 1957. Pp. xii+227. \$5.50.

Sociologists will find these pages strange territory. This volume is not in the sociological tradition of Durkheim, Halbwachs, and Dublin but grows out of Freud, Menninger, and Zilboorg.

In the first section of the book the editors introduce their own research project on suicide, which contributed a third of the volume's papers. Of particular interest here is that their project utilized not only the customary case-history and psychological-test materials but also 721 genuine suicide notes analyzed for possible clues to the causes of suicide. Examples of these notes are included in an appendix along with simulated notes for comparison.

Part I also contains a brief review of psychoanalytic, non-psychoanalytic, and socio-economic theories of suicide, which demonstrates clearly that the thinking in this area is both rich and unintegrated. Indeed, this is a lesson taught by the whole book. In one paper suicide is seen as a logical behavioral outcome, given certain assumptions; another depicts the suicide as "a dupe of the irrationality within himself"; one author explores the sociology of suicide without taking note of religion as a variable, while another finds religious belief a major factor; still another writer contends that most suicides are terminal acts of a prolonged "death trend," but a colleague of his concludes that the life-histories of the suicides he has investigated "brought to light no specific features significant of a high suicide risk."

Sociologists will find themselves more at home in the three concluding papers of Part I. Henry and Short summarize some of the material in their own book, *Suicide and Homicide*.

Ferracuti, a doctor, impresses us with a social-psychological approach to the relationship between Catholicism and the suicide rate, starting with over-all statistical data and interpreting them in the light of the impact on attitudes of specific religious tenets. Silving, a lawyer, takes laws as "an order of external human conduct" and treats them as data which reveal attitudes toward suicide held at different times and in different places.

Part II is concerned mainly with diagnosis and treatment of suicidal pathology and will be of less immediate interest to sociologists. But some familiarity with individual case materials may prove helpful to anyone trying to understand suicide rates. No matter how we resolve the question of levels of generalization, a closer look at the components of suicide rate data will shed some light on the whole.

Clues to Suicide is a generally readable access both to current psychiatric theorizing and to glimpses of psychiatric data in this area. As such, it can be recommended for quick reading to anyone interested in the problem.

MARTIN GOLD

University of Michigan

Workers and Industrial Change. By LEONARD P. ADAMS and ROBERT L. ARONSON. Ithaca, N.Y.: New York State School of Industrial and Labor Relations, 1957. Pp. xi+209. \$4.50.

Labor economics and industrial sociology have joined interests and methods in the study of local community labor markets, and a pattern of research now characterizes a score of these efforts. A significant event which unstabilizes the local market usually qualifies a community for study. A sample of workers is analyzed for such themes as job mobility, methods of finding a job, job satisfaction, wage changes, and the role of state employment agencies. Then various social and economic attributes of the worker are examined for associations with these themes. Last, some aspect of wage theory are re-examined in the light of the study findings.

The Adams and Aronson research follows this general pattern. Specifically, the authors examined the effect on the behavior of displaced workers of the abandonment by International Harvester of its Auburn, New York,

plant. The research systematically compared the behavior of displaced workers and a cross-section of the labor force over a period of four years. The data covered finding a job, workers' satisfaction, searching for employment, behavior on first entering the labor market, importance of education in job placement, and occupational and industrial mobility. A novel aspect of the inquiry concerned the effect of the plant shutdown on the occupational plans and behavior of high-school Seniors.

The general findings are confirmatory, that is, the main value of this study is that it confirms the findings of earlier studies, with more precise controls. While the authors were careful to compare their findings with other labor-market studies, they neglected references to pertinent sociological research on occupational and industrial mobility.

Adams and Aronson follow an accepted theoretical pattern of analysis. They take classical wage theory and marginal analysis as their working model. Their study findings are used to point to the "imperfections" in the classical model and to the importance of local structural features which account for some of these imperfections. It is not surprising that several types of phenomena remain inexplicable with this approach. The theoretical breakthrough for community labor-market studies has already been suggested. New theoretical models take into account the main structural and normative features of the community in a historical context. Community labor-market studies would profit by drawing their specific hypotheses from broader community models which consider both economic and other kinds of behavior.

WILLIAM H. FORM

Michigan State University

The A.F. of L. in the Time of Gompers. By PHILIP TAFT. New York: Harper & Bros., 1957. Pp. xx+508. \$6.75.

This comprehensive and scholarly work by Professor Taft throws a great deal of light on the activities of the AF of L from its formation until the death of Samuel Gompers in 1924, with particular reference to the role and influence of Gompers. Necessarily, it covers much of the ground that Lewis L. Lorwin surveyed in his *The American Federation of Labor*,

though Lorwin carried the story of the movement later, until the depression days of the early 1930's. Taft, however, has gone patiently through a mass of material, such as the Gompers correspondence, that was not available to earlier researchers. The book differs from another earlier work, the Perlman-Taft *History of Labor in the United States*, Volume IV, not only in the wealth of new material that Taft has amassed but in the focus upon the Federation rather than upon the particular unions in the forefront of events.

To gather the material for this volume, Taft worked through the archives of the AF of L and the collections of labor materials in various libraries. He made generous use of the letters of Gompers as well as of the minutes and vote book of the executive council. The mass of material is well organized, in part chronologically and in part topically. The result is an important volume, likely to become a standard reference book for students of the American labor movement.

While Taft is primarily a patient and careful collector of factual material, he has his strong likes and dislikes, as his earlier work has shown. He is a great admirer of the AF of L and particularly of its chief architect, Gompers. No doubt admiration is justified by Gompers' very substantial achievements, and yet Taft risks objectivity by ranging himself so consistently and enthusiastically on Gompers' side. On the rare occasions on which Taft differs with Gompers (e.g., on his opposition to government health or unemployment insurance), he tends to explain Gompers' views sympathetically even as he states his disagreement.

He does not follow the same procedure with Gompers' opponents, however, be they conservative AF of L unionists, socialists within or without the Federation, or leaders of rival organizations. With the Socialists particularly he is impatient and sometimes hardly fair—he gives their views through Gompers' statements of disagreement rather than from their own speeches or publications. Indeed, one may wonder while reading the book why opposition to Gompers appeared and how it managed to obtain substantial strength.

This is not said by way of minimizing Gompers' achievements; unquestionably, he towered above most of his associates and especially above his successor, William Green. It might well have been, as Taft believes, that, had Gompers been at the helm, he would have had the

patience and practical wisdom to prevent the great split in the labor movement that occurred only eleven years after his death.

Because the book is based so largely upon convention proceedings, executive council deliberations, and Gompers' correspondence, one loses something of the flavor of labor events—of developments in the shops, of organizing campaigns, strikes, and collective bargaining. Or, to be more precise, one sees them often at secondhand through Gompers' speeches or letters. Perhaps this is the loss that is bound to accompany extensive reliance upon such materials, valuable as they are for an understanding of history. More could have been done to capture the full flavor and excitement of labor history, however, just as the social-economic background against which labor events occurred could have been developed more fully. The volume would also have profited from a final chapter summarizing the achievements and failures of the AF of L in the Gompers era. Instead the book ends abruptly with Gompers' death.

JOEL SEIDMAN

University of Chicago

Population Theories and the Economic Interpretation. By SYDNEY H. COONTZ. London: Routledge & Kegan Paul Ltd.; New York: Humanities Press, 1957. Pp. viii+200. \$5.00.

The "failure of demography"—made evident by a cursory examination of population projections of the last two decades—"stems from its divorce from economics." The author's purpose is "to return population theory to its natural habitat, the field of economics"; he contends that "it is possible to develop a *general theory* of population dynamics which explains both long- and short-run demographic phenomena." Such a theory maintains that "population is a dependent variable" and that "labour-power is a commodity and demand for labour governs supply." The method of the study is to assume the hypothesis that "demand for labour governs its supply" and, on this assumption, to "explain and integrate" various alleged "facts of demography, previously established by empirical investigations." As one might expect, the "facts" are carefully chosen; the evidence that "established" them is never

produced and seldom even cited; and "no attempt will be made to apply in detail the economic interpretation of demography to a specific country." In short, to return population theory to its "natural habitat" is to return it to the methodological jungle inhabited by a Malthus or a Marx; significantly, the author seems much more at home in his polemical jousts with nineteenth-century theorists than in his erratic efforts to consider factual matters.

If the "failure of demography as a science of prediction" has been demonstrated, what hope has the "economic interpretation of demography" of greater success? Unfortunately, it appears that "there is a difficulty in attempting to predict with accuracy the future population of a country since economists differ widely in their views of the future." Whatever happens, though, we can be sure that the author will have an *ex post facto* explanation; if the "demand-for-labour analysis" does not fit the facts, he can appeal to the "relativity of population laws." Still, we are asked to agree that "the economic interpretation of demography is of value since it isolates the significant variable for population growth."

What is wrong here is what is wrong with any "interpretation." The norms of "interpretation" are not those governing scientific inquiry; perhaps they are aesthetic in character. As good use can be made of speculations on the "first primeval phase" of family life as of gratuitous assumptions about the maintenance of an unobservable "equilibrium." In either case, "absence of inductive proof will not disturb us." One is free to use such non-operational language as "population increasing at a moderately constant rate" or to discuss quantitative questions freely without introducing any quantities. A schematic diagram and a statement that "there is evidence" serve well in lieu of a table of statistics.

It is just possible that demography left its "natural habitat" as a means of adjusting to the competition of empirical science—science in which facts are studied inductively rather than selectively invoked to justify a premise. In all charity to contemporary economists, one must suppose that this work will get no better reception from them than from other scientific students of population.

OTIS DUDLEY DUNCAN

University of Chicago

Economic Institutions and Human Welfare. By JOHN MAURICE CLARK. New York: Alfred A. Knopf, 1957. Pp. 285. \$4.00.

These are the occasional essays of a distinguished American economist. They range in time over incredibly different periods from 1940 to 1955 and in subject matter from problems of pedagogy and freedom—in its several senses—to the indivisibility of social science. Although somewhat repetitious as such a miscellany must be, and a trifle passé in the early pieces, each chapter is absorbing and provocative.

Here a first-rate intelligence and an active conscience combine creatively to explore such fundamental questions as "Economic Means—To What Ends?" "Economic Welfare in a Free Society," "The Ethical Basis of Economic Freedom," and "The Relation of Western Economic Freedom to the World Struggle."

The point of view is truly multidimensional. Professor Clark has no patience with economics as a discipline insulated from the content of human values. Indeed, he charges parochial economists with a measure of responsibility for "the badly split personality" of our age. His own approach in this book may fairly be described as "institutional"—a term traditionally in bad odor among technical economists who by now could scarcely dissent from the proposition that there are "enabling attitudes, institutions and mechanisms without which the things we call economic laws and principles would not work." It is the Schumpeterian breadth of Clark's vision that one finds most impressive: he observes and usefully incorporates the findings of political science, sociology, psychology, and philosophy. More, this apparatus, with its formidable but comfortable erudition, is brought to bear on basic methodological and ideological issues so ably that a reviewer with quite different prepossessions must necessarily admire the skill and lucidity with which this author explicates his ideas.

Sociologists, who are often bewildered by the problem of delimiting their field, will find some comfort in the fact that economists are similarly perplexed these days. What to attempt in teaching economics is a matter of some concern to Clark, and he plumps vigorously for consideration of non-market factors, not least because the economy is governed more and more by such factors. He forcefully argues that the old model of competitive enterprise is no

longer fully serviceable and that the construction of a new model requires conscious commitment to all of science—and to a body of humanist principles. Accordingly, economic science should be *wertfrei*, but as science it merely brings us to the threshold of ethics where irrevocable decisions of a moral and political nature must be made.

It is always possible to quibble about details. Clark is perhaps too generous in his estimate of Cooley and too harsh in his judgement of Veblen. The author is excessively charitable in dealing with the medical profession, which he indicts for commercialism only in the proprietary medicine industry; with the automobile manufacturers, whose interest in protecting motorists from physical injury is less apparent to most of us than to Clark; and with the regulatory commissions (but his tempered tribute to them came before Sinclair Weeks applied the principle of *caveat emptor* to useless battery additives). Notwithstanding such minor points of disagreement, this is a good book, well worth reading and pondering.

BERNARD ROSENBERG

College of the City of New York

Cities and Society: The Revised Reader in Urban Sociology. By PAUL K. HATT and ALBERT J. REISS, JR. Glencoe, Ill.: Free Press, 1957. Pp. 852. \$7.50.

Professor Reiss' revision of a collection of readings in urban sociology, originally edited by the late Paul K. Hatt and Reiss in 1951, contains twenty-seven new articles. Twenty-two of the original selections have been dropped, however. The direction of change is generally toward the addition of the more recent, more statistical, and more "sociological," and the deletion of the older, more impressionistic, and historical-philosophical. In addition, summaries of recent trends and data have been added.

The Administration of Technical Assistance: Growth in the Americas. By PHILIP M. GLICK. Chicago: University of Chicago Press, 1957. Pp. xix+390. \$5.50.

This study of the technical assistance program in Latin America administered by the United States, the United Nations, and the

Organization of American States is one product of a three-year investigation by the National Planning Association Project on Technical Cooperation in Latin America financed by the Ford Foundation.

Philip Glick became general counsel for the Institute of Inter-American Affairs in 1948 and for the Technical Cooperation Administration in 1951, resigning in 1953 to prepare this book. He had the advantages of intensive field study, adequate financing, and competent colleagues. He appears acutely aware of the contributions social science can and must make to any effective aid program.

The United States pays the largest single share of all three programs. Are all three necessary? Decidedly yes! The OAS and UN, which can do things, especially in sensitive areas, which the United States cannot readily do, have the enthusiastic support of the other Americas; therefore, the United States is almost certain to continue its participation. We have exclusive life-or-death power only over the bilateral program, which is larger than the other two combined and also highly successful. Each of the three programs has much to offer that is distinctive and important; and Glick, expressing faith that "the future can be built," thinks each should be continued, expanded, and improved. The gist of the book is in fact a well-thought-out blueprint for improvement.

This aid should not be regarded as a bribe to keep other peoples from communism: "The United States will benefit from the development of the underdeveloped countries almost as immediately and as surely as will those countries themselves. Support for freedom is not being purchased; it is an expected result from growth in knowledge and decline in misery."

JOHN BIESANZ

Wayne State University

Aspects of Culture. By HARRY L. SHAPIRO. New Brunswick, N.J.: Rutgers University Press, 1957. Pp. 147. \$2.75.

These lectures, fourth in the Brown and Haley Series, were given in 1956 at the College of Puget Sound, Tacoma, Washington, to illuminate "some intellectual problems confronting the present age." Primarily a physical anthropologist, Shapiro has chosen to concentrate his attention on the relations between culture

and history, with results of interest to scholars as well as to the general reader.

After an initial discussion of the development of the concept of culture, and its relation to colonialism, environment, and the changing world, the author goes on to a consideration of culture as history and to the means by which the recovery of the past can be achieved. He suggests that the cultural anthropologist "has identified certain processes that can . . . be useful in historical analysis" and "that the comparative method has a distinct value for history" (p. 101).

The final section—on the recovery of the past—is concerned with our ideas of the origins and nature of civilization, as influenced by the findings of archeology. Shapiro argues for continuity in civilization, as over against the cyclical character which Toynbee and others have emphasized, and applies this view to the developing civilization of the United States.

These are problems that cannot be settled in a series of lectures, but Shapiro has stated them clearly, with interesting illustrations drawn from a wide and varied reading. The reviewer hopes that he will continue on a more substantial scale.

FRED EGGAN

University of Chicago

Studies in Applied Anthropology. By L. P. MAIR. ("London School of Economics Monographs on Social Anthropology," No. 16.) London: Athlone Press, University of London, 1957. Pp. 81. \$2.75.

This collection of essays is small but significant, dealing as it does with the ubiquitous problem of the impact of the secular, money-oriented West upon the more sacred, land-oriented societies in the so-called "under-developed areas."

The introductory essay, written in 1956, is, in some respects, the revealing personal document of a distinguished British social anthropologist who has been grappling with the basic issue of the role of the social scientist in social change and with those problems of "value-judgment" and the desirability of "ethical-neutrality" with which the present generation of American sociologists has also shown some concern. Reviewing two decades of British anthropology, Dr. Mair reflects upon the passing of "the confident 'social engineering' days" when

social scientists thought that research on African societies would "bear fruit in enlightened policies" and when anthropologists were expected to supply the data, and sometimes the advice, to make indirect rule work smoothly. She is keenly conscious today of "how much less easy it is to reshape society by deliberate action than has sometimes been supposed." Dr. Mair, however, is not in favor of a retreat into the study or the museum. She is still an active field anthropologist and a reader in applied anthropology at the University of London. She pleads for more applied anthropology, not less—and of her brand. She recommends it to the brash, new, native elites who are replacing governors and district commissioners, and to the new specialists on the development of "under-developed" areas. The task of all social anthropologists is the analysis of social systems, but Dr. Mair believes that "an anthropologist can 'apply' his special knowledge by making intelligible to the layman the forces at work in the society that he is seeking to change."

The "studies" in this book—of economic incentives, land tenure, education, and chieftainship—are based upon Dr. Mair's field work among the Ganda in East Africa and are models of how to write for the layman without "writing down." They, also, make some basic contributions in the field of general social anthropology, which the author defines as the sociology of tribal- and peasant-type societies. Sociologists as well as laymen will find them well worth reading, especially her penetrating analyses of economic values in African societies.

ST. CLAIR DRAKE

Roosevelt University

Trade and Market in the Early Empires: Economies in History and Theory. Edited by KARL POLANYI, CONRAD M. ARENSBERG, and HARRY W. PEARSON. Glencoe, Ill.: Free Press, 1957. Pp. xviii+382. \$6.00.

Historians, economists, anthropologists, and sociologists are gathered here to deal with Karl Polanyi's theme: that the reference of all economic behavior to the model of the self-regulating, price-forming market has obscured crucial problems in economic history and in the interpretation of non-market economies. The leit-motiv is woven through Part I, in which the historical problem of non-market organization

is raised; Part II takes Aztec-Maya, Berber, Dahomey, and India for case materials of economic organization that is not of the market kind; Part III develops a set of concepts as a prologue to a theory of economy and society.

The essays vary in quality from the insightful use of functional theory to the misapprehension of trends and goals in economic anthropology, but they do clearly underwrite the proposition that economics in the market image does not account for the diversity of economies in time and space. The substantive papers bring together material on societies usually beyond the purview of the non-specialists, and they do it well.

The major puzzle of the book is the editors' conception of it as a unity. The problem of the historical and areal identification of the economic type is a coherent section in the hands of the senior editor. The section describing actual economies is by nature diverse. But the third section—the theoretical and interpretative—bears small relation to the problem stated and draws almost nothing from the empirical material.

However, some new and provocative ideas are developed and some old ones restated in modern garb. Polanyi's trilogy of reciprocity, redistribution, and exchange mechanisms for integrating economic activity are given expanded meaning. The overtones come from two new ideas: the substantive economy and instituted process. Substantive economy is "empirical economy," the "livelihood" aspect of economics, or what an ethnographer usually collects and reports as the economic life of a people. "Instituted process" refers to the self-evident proposition that economic activities are part of a normative social structure. Together, these concepts enable one to talk about the "embeddedness" or "unembeddedness" of economic activity in a social system. They provide a new idiom about the relations of "economy and society" (as against talking about economics and sociology).

Despite their attractiveness, these concepts give me pause. Since the editors did not refer them to empirical material, and since they do not stem from the effort to order and interpret observation (of course, this is not a necessity), I cannot imagine how they are to be fruitfully employed. As I know the anthropological process of gathering economic fact (the substantive economy par excellence, often to the annoyance of economists), these concepts would not re-

direct or improve field work. In seeking generalization from fact, I tremble at the leap into a level of discussion equivalent to the third section of this book.

All this is to say that we have been presented with some challenging problems, some stimulating ideas, and some conceptual tools whose utility is yet to be assessed. Those interested in the attempts to make of economic life (though perhaps not of "economics") one of the subject matters of the "behavioral" sciences will profit from this book.

MANNING NASH

University of Chicago

The Great Siberian Migration. By DONALD W. TREADGOLD. Princeton, N.J.: Princeton University Press, 1957. Pp. xiii+278. \$5.00.

The author, a historian, has written a highly competent treatment of the migration of the Russian peasantry to Siberia during the period from the emancipation of the main body of serfs in 1861 to the start of World War I. There is a thesis: the examination of the Siberian frontier in the light of Frederick Jackson Turner's view of the American frontier. Treadgold contrasts and compares Siberia and the American West.

He has not undertaken a social history; there is little of peasant institutions and life presented here. He is not a demographer or economist; thus, while he has reason to suspect that the size of the peasant migration was underestimated, he goes no further than to indicate his suspicions. Nevertheless, good use of realistic fiction is made to give some social perspective to the peasant movement. There is a wealth of demographic and economic detail regarding the migration and the growth of the area in people and product. Detailed treatments in these directions fall outside the scope of the book. It is hoped that the published treatment will call forth further studies which will enter more intensively into these aspects.

The book is divided into five parts: The origins of the migration and the peasant economy; migration policy after emancipation of the serfs, and the beginnings of the movement; "The Trans-Siberian Railway, 1892-1906"; "Stolypin and the Duma," a description of the policies of Stolypin, the attitude of the Duma, and the final course of migration; and, finally,

"The Fate of Migration," the end results, the causes and effects of the migration, and the fate of the Russian peasantry are outlined.

The burden of the book is developed, then, in terms of a succession of governmental policies, and detailed treatment is given to the consequences of these policies. The more general aspects of the book are to be found in the opening and closing chapters. In the concluding part it is suggested that the basic motive to the migration was the quest for land and freedom.

A few errors mark an otherwise careful study. The Russian conquest of Siberia had its bloody passages, no less than did the American conquest of its frontier. In central Asia the Russians encountered more massive resistance over a long period than did the Americans. Yet, too, much can be made of this point in both directions. Again, while the Russian settlement of Siberia was by families, yet individual settlers—trappers, miners—left their mark on Siberian folklore and belles-lettres, including little touches of "wildness," as in the American West, although not on such folkloristic scale. Finally, the review of the causes of the migration proposes that the peasant's quest for freedom was "in part a consequence of his having already been relieved of the bondage of serfdom." This is highly disputable. The Russian peasant's search for freedom long antedated the emancipation of the 1860's and in part brought it about. The emancipation, rather than initiating his quest, made it more easily attainable.

The impact of these few objections should not detract from a general appreciation of a sound work on an important and interesting subject.

LAWRENCE KRADER

Washington, D.C.

The Indian in Modern America: A Symposium Held at the State Historical Society of Madison. Edited by DAVID A. BAERREIS. Madison: State Historical Society of Wisconsin, 1957. Pp. xx+70.

For the most part the remarks of the participants in this symposium on Indians held during the Draper Centennial Year of 1954 were directed more to the public at large than to a scientific audience. The contents, ably edited by Dr. David A. Baerreis, include papers on the legal and minority-group status of the Indian (Hoebel and Barnett and Baerreis, re-

spectively), Indian education in historic perspective (Beatty), and a general critique of the presentations (Fitzgerald).

All participants agree on the complicated nature of the "Indian Problem" and are in hearty disagreement with the easy legislative solution that would end it all by legalizing the citizen status of the Indian in all particulars.

Hoebel argues that the traditional "legal right of self-government as 'domestic dependent nations,' " established by treaty and supported by judicial interpretations like those of John Marshall, still constitutes the best guaranty for a secure Indian future.

After describing the social and economic aspects of discrimination in and about Indian reservations, Barnett and Baerreis take up the theme that only as the minority-group status of the Indian is "redefined by the dominant group" will participation increase and "successful integration" follow (p. 62). Their comparison of the isolated, economically depressed and disorganized Bad River Band with the thriving Lac du Flambeau group, where both Indians and whites alike contribute to the local economy, provides documentation for their thesis. While few Indians are proprietors, still they find employment as semiskilled workers, and those who guide fishers and hunters are "well-paid and admired" for their practice of traditional Indian skills. Unlike the Indians at Odanah, who demonstrate a "strong strain of *anomie* . . . in social relationships" and sensitivity to the "veil of stereotyped attitudes toward them," the Lac du Flambeau group are vigorously increasing their contacts with the larger society and are avoiding the malign influences that flow from stereotyping (pp. 65-68). The Lac du Flambeau, however, do not escape the net of stereotyping and of discrimination, which the authors, even in their case, consider a barrier that must be broken if they are to be assimilated.

Perhaps because his wide and intimate experience in organizing Indian education is filled with anecdotes that illustrate local problems and local adaptations, Dr. Beatty is able to demonstrate the necessity of a program flexible enough to adapt to local circumstances. Hoebel, Barnett, and Baerreis also imply that social and cultural heterogeneity between and on reservations militates against a blanket legislative solution. In their emphasis on a "historical approach" they seem to take a particularistic position, assuming that tribal experience in con-

tact has been so unique that each group must be handled as a separate problem. Perhaps this particularism may explain why only the educator, Dr. Beatty, compliments the Indian Reorganization Act for its strengthening of the Indian right of "self-determination" despite the general agreement that the Indian must have a prominent role in decisions that will shape his future.

In my opinion the particularist position is as much in error as the universalist commonly assumed by the public and by legislators. Regularities in acculturative processes do exist, and hence a program sensitive to the limits and anticipated effects of these regularities is not ruled out completely. Insistence that the tribal society be sustained as the sole legal and acculturative entity may bring as many difficulties as programs that ignore areal and tribal differences, since the acculturative differential within a society in turn is ignored. That is why preoccupation with the legal status of the Indian, while important, must give way to a consideration of his changing sociocultural status, as Barnett and Baerreis point out. Yet, as members of a legal-minded, contract-oriented society, we are much inclined to shift the burden of the problem, and of conscience, to the obfuscation of legal debate and interpretation. If there were any promise of beneficial change through appropriate control, the legal angle destroys it effectively. A preoccupation with obvious legalities, in my view, detracts from the otherwise good review with which Mr. Fitzgerald introduces the materials.

FRED W. VOGET

University of Arkansas

The Population of Jamaica: An Analysis of Its Structure and Growth. By GEORGE W. ROBERTS. With an Introduction by KINGSLEY DAVIS. New York: Cambridge University Press, 1957. Pp. xxii+356. \$7.50.

This analysis of Jamaica's demographic history is the first of three projected studies of Jamaica's population problems sponsored by the Conservation Foundation. The second, a sample survey nearing completion under the direction of J. Mayone Stycos and Kurt Back, is a study of family life in Jamaica, with special emphasis upon attitudes and behavior as they

relate to reproduction; and the third is a study of the resources and land-use patterns of the island as they relate to population trends. If the attention to detail here, the broad relevance of findings, thoughtful interpretations, and general level of competence of the two projected studies are equaled in the remaining two volumes, these interrelated studies will constitute one of the most important contributions in recent years to the understanding of population problems in underdeveloped areas.

Roberts begins with a discussion of the basic data underlying his demographic analysis and with an evaluation of their reliability. They include early slave records, censuses since 1841, general registrations of vital information since 1878 (when effective civil registration started), records of indenture migration since the early 1840's, and general collections of migration returns since 1910. He discusses the sheer growth of the population from the time of the conquest of the island by the Spaniards in 1494; analyzes the age, race, sex, educational, and occupational characteristics; describes mortality trends and the currents of external and internal migration; presents the changing patterns of reproduction, fertility, mating, and illegitimacy; and, finally, speculates on the prospects for growth of the island, making two projections each based on different assumptions.

Although perhaps carping, the following points are noted: Some of the statistical materials are given in the text only and not in accompanying tables or figures. This has the effect of burying some of the findings and reduces the volume's utility as a reference book. It could easily have been avoided by introducing more tabular materials. The discussions often contain such phrases as "the most striking fact," "the most significant change," "the outstanding difference," and "the most arresting development." Perhaps this style of expression adds a certain liveliness but occasionally what Roberts chooses to discuss as the most important seems somewhat arbitrarily chosen: other differences or trends appear equally striking. Finally, there are some discrepancies between the text and the tabular materials.

However, in general, the book is excellent. Kingsley Davis in his Introduction describes Roberts' work as "an unusually competent and original case study of a highly interesting population. He is to be congratulated by professional demographers, by specialists in under-developed

areas and above all by the Jamaicans themselves for his skill in catching their lives in his demographic net."

WENDELL BELL

University of California, Los Angeles

Elite Communication in Samoa: A Study of Leadership. By FELIX M. KEESING and MARIE M. KEESING. Stanford, Calif.: Stanford University Press, 1956. Pp. vii+318. \$4.75.

This book is a result of the authors' continuing and intensive anthropological work in Samoa stimulated afresh by the Research Program in International Communication of the Center for International Studies at Massachusetts Institute of Technology. The idea is to see, first of all, whether the rewriting of the Samoan materials from this new point of view might contribute to an understanding of the patterns of communication and decision-making in a society whose culture is largely orally transmitted. Second, the authors want to contribute to the study of communication between such a society and the Western societies with which it has contact. Third, it is suggested that some of the generalizations advanced may have universal applicability in situations of decision-making and deliberation.

Samoa is a society where, traditionally, every man is *not* entitled to his own opinion. The basic unit of the society, the household, is presided over by a chief who bears a hereditary "title" invested in the household. The chiefs and other titleholders in a village form a village council, and the more important of these, in turn, form district councils and, finally, the "all-Samoa" councils. The first five chapters of the book elaborate the idea (p. 6) that "talking in Samoa is carefully channeled. Everyone, as a matter of course, discusses everyday affairs: household activities, the catch of fish, the gossip of the neighborhood. But elite, authority-bounded subjects are correctly talked about publicly by the appropriate elite individuals only. By chapter vi, we have a good picture of the traditional table of organization of the Samoan governing elite and especially of the way in which the higher councils come together to make decisions. (We learn the functional importance of the symbols and ceremonies of decision-making in a society which depends on oral transmission and of the way in which distortion

and selective recall enter into the deliberations. We are treated to some rare insights into the ways of detecting smoldering discontent after the requisite "unanimous" decision has been reached [p. 120].)

The later chapters of the book address themselves to the question of how this traditional structure of Samoan communication and deliberation "hooks up" with the influences impinging upon it from "outside"—particularly from New Zealand and the United States, the two powers that administer the two parts of the island. Some important processes are pointed out and analyzed. For example, the importance of cultural "compatibility" as a prerequisite to successful international communication is interestingly illustrated with reference to the greater compatibility of Samoan culture with the American rather than the more puritanical New Zealand culture. Or a lesson concerning the mediating role of the elite is illustrated by the placement of the first radio sets in the homes of the leading chiefs, who then presided over listening and interpretation. On the other hand, it is pointed out that the contact between cultures also created new mediating roles, the most important of which are played by the children of mixed marriages (the part-Samoans) as well as by those who have studied or traveled abroad.

The authors are convinced of the continued integrity of the traditional system even in the face of the pressures from the West. Several detailed analyses of how the traditional system is trying to subsume changes that originated outside are particularly interesting. For example, traditional titles are being offered to individuals who achieve leadership in the various Christian churches, in business, and in the professions.

Undoubtedly, this kind of society gives rise to considerably less differentiation than modern society in the roles specifically involving communications. But, if one of the important jobs of communications research is to emphasize those channels which do not overlap the formal and normative ones, then this study is almost certainly understated here. Surely one of the ways to overlook systematically the brewing of social change is to accept the normative fact that people who ought not to have opinions about certain matters, or who have not the "right" to express them, need not be interviewed. In arguing against the techniques of cross-sectional opinion research in such situations, it is suggested instead that "an investigator needing

information must obviously go to the persons who not only command it but also have the right to impart it, i.e., a purposive and controlled rather than a random type of sampling. In the case of elite matters, this would be to the appropriate chief or orators at the highest level of authorized communication. This is one of the main techniques of the ethnologist in handling his informant samples: going to elite leaders on elite topics, to specialists on their own expert data, to women and even to children on appropriate concerns" (pp. 128-29).

The authors' relative lack of interest in differentiating what is being communicated and to whom is another reason one sometimes feels that the analysis is overly formal. One is left with the impression that all ideas travel along the same hierarchical routes. Yet one wonders whether the rapid acceptance of Western technological innovation in Samoa flows through the same channels described by the authors. Surely, messages influencing the decision to move to the city (and thus out of the orbit of traditional regulation) originate elsewhere.

Nevertheless, the study is very welcome for the rich detail it provides on this particular case and for its suggestive hypotheses. It makes a substantial contribution to the comparative analysis of communications systems—a subject Margaret Mead began to outline two decades ago. Equally important is the contribution it makes to the design of further research on the ways in which traditional word-of-mouth systems link up with influences originating outside.

Some anthropologists, particularly those working in applied fields such as colonial administration or the evaluation of technical assistance campaigns, have already contributed substantially to the development of hypotheses concerning cross-cultural communication; others (students of acculturation, for example), however, have not spelled out the communication processes in their studies. An appendix to this book briefly reviews some of the relevant anthropological contributions to the study of communication (and the rare contributions of communication specialists to anthropology). Altogether, this book gives added confidence that the common ground may indeed be fertile.

ELIHU KATZ

*University of Chicago
and
Hebrew University, Israel*

The Negro Family in British Guiana: Family Structure and Social Status in the Villages.

By RAYMOND T. SMITH. London: Routledge & Kegan Paul, 1956. Pp. 282.

In this work the author is interested in discovering and establishing structural regularities which are independent of historical and cultural variation. After an intensive analysis of the mother-centered (matrifocal) family, he investigates its relationships to the ecology, economy, occupational structure, color and status, and the class system of the country. Less formally stated but also of concern are such questions as these: Can we identify the minimal necessary social unit which insures physical and social reproduction and continuity in a society? Can we use typological categories independent of temporal considerations when analyzing the structure and functions of the family? Lastly, the author undertakes to compare the structure of the British Guiana Negro family system with systems in the Caribbean, the southern United States, and villages in Guatemala, Peru, and Scotland.

Periods of eleven, three, and three months, respectively, were spent in the three villages studied, which were not selected as statistically representative. Distributional data were obtained on house types and ownership, land use and control, occupations and income, kinds of conjugal relationships, composition and authority within the household, and general patterns of interaction within the family. Data used for units larger than the three villages were primarily non-numerical and non-statistical. The studies of other investigators were used for the broader comparisons.

Smith finds that among the British Guiana Negroes the mother-child relationship is the minimal family structure, and this strongest of family bonds persists as strongly reciprocal. The matrifocal unit is stable, in contrast to the conjugal unit. An excellent demonstration is given of the necessity for using a temporarily oriented typology of families, since the household unit varies through time in (1) its domestic context and its relation to the village and (2) in its relationship to the total social system of British Guiana. Male authority and responsibility, for example, decline with time and with the increasing age of the children. The household has practically no corporate functions and is primarily a child-rearing unit. This family structure is shown to be understandable only

in the context of the total British Guiana social system. The class-caste position and the seasonal occupation of Negro males combine to produce the family structure.

Parallels in family and class/caste structures are shown to exist among Negroes elsewhere in the New World and among villages in Peru, Guatemala, and Scotland, and these parallels argue strongly against "explanation" by culture-historical theories (e.g., Herskovits): "If we look at all the societies we have mentioned . . . , we find a correlation between low social status in a stratified society and a type of family system in which men seem to lack importance as authoritarian figures in domestic relations. These are facts of social structure, and the arrangement of these structural elements is basically similar despite marked variations in their corresponding cultural complexes in different societies."

This is altogether an impressive book, one which certainly points to a salutary rapprochement between sociology and social anthropology and which should be read by students of comparative family and social systems. The author argues his case modestly but persuasively. His methodological precautions are greatest when discussing the Negroes in the three villages. When focusing on the villages as units, he gives inadequate attention to the Negroes' relationships with the Portuguese, East Indians, and "mixed" ethnic groups. His criteria for defining these groups are also inconsistent. He uncritically adopts a three-class analysis of Trinidad as representative of British Guiana, but its empirical application is unconvincingly demonstrated.

Many will find his arguments against the theory of cultural survival to be inconclusive. His presentation would have been enhanced had he attempted to reconstruct or discuss the West African social and family systems prior to (or after) emigration to the New World (e.g., hierarchical authority structures and bondage and "slavery").

Despite these reservations, this is a stimulating, interesting, and valuable contribution toward establishing cross-cultural theories of family and social structures. The author argues effectively for the desirability of erecting paradigms which recognize the incomplete equation of culture with society. His provocative statement that culture persists longer than society calls for much further research, as does his hy-

pothesis that family solidarity correlates positively with higher status and increasing political responsibility.

C. FRANTZ

Portland (Oregon) State College

Village and Plantation Life in Northeastern Brazil. By HARRY WILLIAM HUTCHINSON. Seattle: University of Washington Press, 1957. Pp. ix+199. \$3.50.

This is a study of the relatively isolated town of Recôncavo, a "has-been" community which can look back from its present atrophy upon a glorious and prosperous past based upon sugarcane production and a slave economy. The processes of mechanization and urbanization, which are forcing a regrouping of population about modernized sugar mills, have not as yet had complete influence or proceeded as far here as in other areas. The five large private plantations retain the paternalism which grew out of the earlier epochs, and the new larger plantations organized as corporations and focused on the sugar factory are owned and controlled by extended upper-class families and still retain much of the old paternalism.

The pattern of settlement on the private plantations was the village form, and the newer regrouping about the factory system also follows this form. The county-seat towns retain their former bureaucratic functions, but town and country are quite separate, since most rural life goes on within the plantation.

Systemic descriptions of a private and a factory plantation are included with a fascinating discussion of the status role of manager, a more and more powerful role as the private plantation owners absent themselves during the winter and as the factory corporations rely more and more on professional executives. The reviewer has found what the author considers a recent development—namely, the patron and owner performing an expressive and protective status role in relation to the workers as contrasted with the administrator's more task-oriented status role—to be common and of long standing elsewhere in Latin America. The family systems of three towns and four rural classes are discussed in detail, and the importance of extended patriarchal family groupings for all except the lowest classes is stressed.

A workman-like analysis of religion is pre-

sented, a description of the secularization of members of the Catholic church and the African cults. Protestantism is completely absent, but the growth of spiritualism, partly as a function of faith-healing, is described.

This well-written monograph is a pleasure to read. Perhaps its chief shortcomings are the lack of demographic data and a poor statement of sampling procedures used for interviews.

CHARLES P. LOOMIS

Michigan State University

Spanisches Erbe und Revolution: Die Staats- und Gesellschaftslehre der spanischen Traditionalisten im neunzehnten Jahrhundert ("Spanish Heritage and Revolution: Political and Social Views of the Nineteenth-Century Spanish Traditionalists"). By JOACHIM FERNÁNDEZ. Münster: Aschendorfsche Verlagsbuchhandlung, 1957. Pp. 127.

The reign of the Bourbons in Spain since 1701 mark a period of bureaucratic centralization and economic and social reforms. The spirit of the French Enlightenment and of liberalism seem to have made their entry into feudal Spain. But Napoleon's invasion and the resistance movement reversed the trend. With the withdrawal of the French troops in 1816 began a period of repressive policies and a series of antiliberal formulations in the political literature of nineteenth-century Spain. It was the era of the traditionalists. Four spokesmen of this reversal—Aparisi y Guijarro, Jaime Balmes, Donoso Cortés, and Vazquez de Mella y Fanjul—form the subject of Fernández's sympathetic report.

While Aparisi and Balmes concerned themselves largely with a critical appraisal of daily events and did not enter the field of political theology, Donoso Cortés and Mella formulated a doctrine which became a rallying point of Catholic traditionalists in Europe. Briefly, the political theology of the traditionalists rests on the thesis that man is born in sin and, therefore, in need of firm guidance and a stern secular authority. The false optimism of the liberals and their faith in the perfectibility of man opened the gate for the spirit of rebellion. To guard against anarchism and socialism, Spain must return to her traditions: the supremacy of the Catholic church over the state, regional decentralization, the economic autonomy of the

estates, and an authoritarian monarchy comparable to that introduced into Spain by the Hapsburg dynasty before the Bourbons. The traditionalists oppose the separation of powers, parliamentary government, the political principle of equality, religious tolerance, freedom of opinion and conscience, and the secularization of church property. The solution of the social conflict between capital and labor must come from the revival of Catholic faith and charitable sentiments. The social order must be re-established on the foundations of natural law.

A helpful bibliography on the four political philosophers rounds out the booklet, whose German translation was published by the Institute for the Christian Social Sciences at the University of Münster.

ERNEST MANHEIM

University of Kansas City

The Iteso: Fifty Years of Change in a Nilo-Hamitic Tribe of Uganda. By J. C. D. LAWRENCE. New York: Oxford University Press, 1957. Pp. xx+280. \$5.80.

Material for this book was gathered during the author's five years' service as district commissioner among the Iteso, a Nilo-Hamitic-speaking tribe in the Eastern Province of Uganda, East Africa. The volume contains three main sections: a history of the Iteso, both prior to and during British administration; a general ethnography; and an account of Teso customary law based upon the analysis of a large number of cases from the local government courts. It is not, and does not pretend to be, a professional social scientist's study of social change; it is, however, a well-written account of an African people and their recent experience by an able and experienced observer.

The story of the Iteso's contact with Western influences is a rather unusual one, both in the circumstances under which it occurred and in the rapidity of the change which it produced. Their nearest linguistic and cultural kinsmen are the tribes of Karamoja District to the northeast, a group of seminomadic pastoral peoples upon whom four decades of administrative and missionary effort have had remarkably little impact. Indeed, the Nilo-Hamitic group as a whole, among whom the Masai of Kenya and Tanganyika are the best known,

have been the most conservative of East African peoples. The Iteso, however, have become relatively prosperous, settled cotton farmers, have adopted Christianity in very large numbers, and have in general acquired a reputation for "progressivism." Perhaps significantly, they were first brought under administration, not by British officers, but by Simezi Kakunguru, a powerful chief and military leader from the nearby kingdom of Buganda. The sophisticated and highly centralized Ganda state had during the late nineteenth century become the political center of gravity for the whole region. At the turn of the century it became the core of the British Protectorate of Uganda, and henceforth Ganda chiefs, converted to Christianity and experienced in a type of administration which combined indigenous absolutism with British notions of taxation and law and order, were sent out to subdue and incorporate nearby tribes. Within five years, Kakunguru and his rifle-aimed followers eradicated the age-set system which was the basis of Teso military organization, organized the country into a hierarchy of chiefdoms on the Buganda model, and built a network of roads. His harsh methods eventually deprived him of his command, but the British administrators who followed him inherited a political framework which has remained the basis of local government down to the present day.

Lawrence's book is an extremely useful contribution to the literature on the peoples of East Africa. It also exemplifies the commendable policy of British colonial governments of encouraging scholarly interests on the part of their servants (this volume carries an introduction by the then governor of Uganda, Sir Andrew Cohen).

L. A. FALLERS

University of California

Tokugawa Religion. By ROBERT N. BELLAH. Glencoe, Ill.: Free Press, 1957. Pp. 249. \$5.00.

Books on Japanese religions are many and varied. Significantly, the study of the religions of the Tokugawa Period (1600-1868) has been conspicuously neglected. Dr. Bellah's book is an important contribution to fill this gap. Based on the author's doctoral dissertation presented to the Departments of Far Eastern Languages and Sociology at Harvard, the subtitle—"The Values of Pre-industrial Japan"—well indicates

Bellah's concern and perspective. While a shift in basic value patterns often accompanies the process of development from a non-industrial to an industrial society, the author holds that an industrial society may develop under certain circumstances "without a shift in basic values." For instance, in Japan, where the primacy of political values was taken for granted, economic values have assumed a high importance, but they have remained subordinate to political values. Within this framework Bellah attempts to analyze "the way in which the rationalizing tendencies in Japanese religion contributed to economic and political rationalization of Japan" (p. 8). He examines how the "two types of religious action, one derived primarily from Buddhism and the other from Confucianism, have come to reinforce the central values of achievement and particularism" (p. 82).

His conclusions are that "a strong polity and dominant political values in Japan were distinctly favorable to the rise of industrial society" (p. 192) and that "religion played an important role in the process of political and economic rationalization in Japan" (p. 194). Bellah gives special attention to the Sinkingaku Movement, a merchant-class religio-ethical movement of the eighteenth and nineteenth centuries, as the embodiment of the ethos of Tokugawa religion.

Despite some obvious oversimplifications, this is an able analysis of Tokugawa religion. For instance, while there is much truth in Bellah's characterization of Buddhism in the Tokugawa period as lethargic and uncreative (p. 51), one cannot overlook altogether the vitality of the Pure-Land beliefs among the masses. Also, this reviewer agrees, in part, with Bellah when he states that "the Meiji Period (1868-1911) was a culmination of the central values rather than a rejection of them" (p. 19), even though Bellah tends to overstate the case (cf. p. 25). However, these minor comments are not meant to discredit this careful and sensitive treatise on Tokugawa religion.

JOSEPH M. KITAGAWA

University of Chicago

Family and Kin in Indo-European Culture. By G. S. GHURYE. ("University of Bombay Publications: Sociology Series," No. 4.) London: Oxford University Press, Indian Branch, 1955. Pp. 254. \$3.15.

With all their research on kinship in different parts of the world, anthropologists have neglected the study of the kinship systems of the Indo-European family of cultures, although these are the ancestors of our modern Western kinship systems, and we possess for them, as we do not for most primitive systems, some information on their past changes. We can study them in depth.

The reason for the neglect is not hard to find. The effective study of Indo-European kinship requires a thorough command not only of the linguistics and history of a variety of peoples but also of comparative kinship in non-Indo-European cultures. Few anthropologists have had the former, and they have tended to leave the subject to linguists and historians, whose interpretations have been inadequate because they have not had the latter.

Professor Ghurye's is a heroic attempt to resolve the dilemma. He examines the earliest evidence on the kinship of the Aryans in India and the Greeks and the Romans in Europe. His treatment of other groups, such as the Celts, Germans, and Slavs, is cursory. Moreover, the earliest records are the most difficult to interpret. If I were doing the job, I should start with a period in each culture when we have a pretty solid knowledge of the kinship arrangements and work backward in time. But I can hardly blame Ghurye for not doing the job my way, when I am not ready to undertake it myself!

The trouble with Ghurye's book is that, though he knows more anthropology than the historians, and more history than the anthropologists, he still does not know enough of either. On the latter side, he asserts, for instance, that "whereas Old High German had a distinct term [for father's brother], Anglo-Saxon had one which was based on the term for father." The fact is that the two languages used forms of the same word—*vetter* and *faedera*—and, if the latter is based on the word for father, the former is too.

On the anthropology side, he seems to me to nurse a continuing confusion: that the existence of bilateral kinship groupings is somehow incompatible with the existence of unilineal kinship. But the Anglo-Saxons, for instance, had *both* kinds of grouping: a patrilineal joint family that governed landholding and a bilateral grouping, the *maegth*, reckoned from ego on both the father's and the mother's side, that governed the payment and receipt of wergilds.

It is this confusion, I think, that leads Ghurye to believe that "the primitive Indo-European people had not developed a unilateral kin-organization of the *gens*, clan, or sept type."

But I am asking too much if I ask for perfection where perfection is so difficult of attainment. Ghurye's is a better attempt than has been made before. His judgments are often wise. And a more complete anthropologist than he, who is willing to be a little skeptical about his historical statements, especially about northern Europe, will get from this book a great deal of useful information.

GEORGE C. HOMANS

Harvard University

Soviet Youth: Some Achievements and Problems. Edited and translated by DOROTHEA L. MEEK. ("International Library of Sociology and Social Reconstruction.") London: Routledge & Kegan Paul, 1957. Pp. vi+251. \$5.50.

Soviet Youth is a study which will be useful primarily to the general reader who is interested in problems of education in the U.S.S.R. but who does not read Russian and is unfamiliar with more specialized work on the subject. The book is divided into four parts, each dealing with a different age group, beginning with the preschool child and concluding with the young employed and married adult. Each part consists of an introductory section by the author, which presents some historical background material and a brief analytical discussion, and a series of translated articles or excerpts from articles which have appeared in the last ten years in newspapers or popular magazines, such as *Izvestia*, or *Komsomolskaya Pravda* (a daily paper published by the Central Committee of the Young Communist League), or *Ogonyok* (a weekly magazine). Each article illustrates current thinking about some problem, for example, the difficulties of organizing a kindergarten on a collective farm, or the difficulties of acculturation among some of the less Westernized peoples of the U.S.S.R.

The content of the book—which draws on popular rather than scientific sources—will be familiar to all those who have worked on contemporary Russian problems. The value of *Soviet Youth* is less in the data presented than in its demonstration of a point of view and a style of educational writing. As might be expected, considering the sources, the articles present strictly an official picture, and it is clear

that all are intended as guides to the Soviet reader in forming correct opinions and attitudes toward a variety of subjects. As is usual in Soviet didactic writing, this is furthered almost equally through praise for accomplishment (with the implication that the reader should go and do likewise) and criticism for failure or ridicule of error (with the implication that this is what the reader should avoid). Despite differences in subject matter, the American reader who is familiar with the proselyting literature of many missionary publications will find himself on familiar ground here also, for, irrespective of the specific subject, the prevailing tone is one of moral exhortation, and good and evil are continually embodied in the heroes and villains presented in thumbnail sketches. So we see the girls and boys who fail to appreciate good honest work, the parents who evolve a do-it-yourself plan to take their children off the streets, and the *komsomol* secretary in Tashkent who turns out to be a chief offender in depriving women of their modern rights. Throughout the articles there recurs the theme of concern for the valuation of manual labor in a society which also highly values intellectual achievement.

The author has attempted to be objective and to present diversified materials. It is the more interesting, therefore, that, despite shifts and changes of policy, there is little alteration in the underlying attitudes from those most vividly presented almost thirty years ago in the film depicting the reform of the *besprizornye*, the vagrant children of the Revolution, known in English as *The Road to Life*. There is one difference, however, which is perhaps inadvertently underlined in this book: whereas in the film new ideas were presented with verve and gusto, in these more recent writings they have stiffened into the platitudes of soap operas. And one is left wondering if they are more or less effective than these American comments on the contemporary scene.

RHODA MÉTRAUX

Institute for Intercultural Studies
New York City

From Witchcraft to World Health. By S. LEFF, M.D., and VERA LEFF. New York: Macmillan Co., 1957. Pp. 235.

The Leffs have attempted a popular history of medicine and have achieved no great success because of a tendency toward oversimplification

and superficiality. The authors will succeed in antagonizing most professional historians and informing few who may turn to the book because of its specious title.

The book is divided into conventional historical epochs. Each historical period is then briefly described, and the practice of medicine, the discoveries, and the scientific advancements are dealt with in a rather unrelated fashion.

To their credit, it should be mentioned that the authors have a rather healthy understanding of the importance of public health measures in medical practice and give proper emphasis to immunization, water filtration, and sewage. However, to give due consideration to preventive medicine is one thing. To discuss this in partisan political terms is another. An indignant sense of social injustice at times plays havoc with their objective historical perspective. To write that "a spectacular beginning [in preventive medicine] has been made in the Soviet Union where the creation of a new social order has also meant the development of a new, positive health service, concerned above all with the people at work, at recreation and at home" (p. 225) is to write more than history.

The book has more than the usual share of errors allocated to a work of this type. To speak of the "brain as an organ in which every action [?] has its corresponding electrical reaction" (p. 195) is misleading. A reference to Oliver Wendell Holmes as an obstetrician (p. 181) would have moved the author of *The Autocrat of the Breakfast Table* to an incisive retort. To deal with Freud in terms of mental catharsis and suggestion is to give a distorted impression of his contribution and neglect his major achievements.

If popular writing has any function aside from entertainment, it must surely be to stimulate and to encourage interest and curiosity to delve further into a subject. This book does nothing of the sort.

HARRY TROSMAN, M.D.

University of Chicago

Change and Dilemma in the Nursing Profession. Edited by LEONARD REISSMAN and JOHN H. ROHRER. New York: G. P. Putnam's Sons, 1957. Pp. xii + 450.

This is a study conducted by the Urban Life Research Institute at Tulane University of the activities of nurses at work, what they think

about their work, and their relationships with patients and other staff members. One of a number of sociological research projects financed by the American Nurses' Association, it is an indication of nursing's interest in collaborating with sociology. The setting is New Orleans' Charity Hospital, one of the country's largest, financed by Louisiana for its poorer citizens, and a teaching center for medical and nursing education. Data were gathered by observation and by several kinds of interviewing.

In many occupations there is a discrepancy between what people say they value the most and what they actually spend their time doing. In nursing this discrepancy is marked and is a considerable source of concern and discomfort to nurses themselves. Nurses say that the best and most rewarding work is at the bedside, but nursing behavior in hospitals is much more a matter of supplying medications, injections, and other technical procedures to a multiplicity of patients, of keeping records, and of overseeing subordinates. This contradiction is described by interviews and observation of nurses chiefly from medical and surgical wards. The authors present data indicating that the behavior is a product of the scarcity of nurses and preference for these activities. They refer to the verbalized values as existing because of tradition. I hope that someone will study the latent function of these values, especially in the socialization of nursing students.

A second problem with which this book deals is that of the hospital's formal organization. Fluctuations in staff available from day to day and from shift to shift mean that the work boundaries between head nurse, staff nurse, licensed practical nurse, nurse's aide, and ward aide often are not preserved. When interviewed, nurses expressed the wish to avoid any explicit sanction of the actual division of labor, yet interview data throughout this study contain evidence of their concern over status; the authors suggest that this concern also promotes preservation of the rules.

Among other interesting data in this book are the account of how lower-level people in the infants' nursery blocked certain changes in nursing care proposed from above; a typology of nurses and aides (dedicated, converted, disenchanted, migrant); information on attitudes and behavior of nurse's aides and ward aides; the description of nursing care given patients by visitors and by other patients; and the summary of the projective interviews based on

"role cards" portraying various scenes in the hospital. A point which I wish had been further explored is the contrast between nursing care in the nursery and that in other parts of the hospital. Nurses depersonalize care in the latter yet dote in the former. Why do infants make such gratifying patients?

Validation for some of the findings was obtained by comparison with data from projective interviews. The verbalized values about patient care were elicited by a sentence-completion test, while responses to the role cards and to part of the sentence-completion test projected a picture of nursing behavior as technical procedures and the supervision of subordinates. A job check list is also cited as validating, but this seems to me doubtful, since I cannot tell how the authors distinguish items involving technical procedures from other items involving patient care. Interviews using role cards with the nursery staff also produced similarities with findings regarding the actual division of labor in the nursery and the wishes of the staff to preserve the formal division of labor.

The quality of this book is uneven. Well-organized analyses of data alternate with stretches of tabulations of answers to single questions set off by interpretive comments of no particular significance. The data, or the concepts which subsume them, are occasionally unclearly described (see, for example, the analyses of the job check list and the role-card interviews for the medical and surgical wards). A sixfold typology of "status-role" conflicts is proposed and then dropped. The ideal type of bureaucratic organization is used in analyzing some of the data but never to more purpose than to say that the hospital does not fit the type in some respects.

Nonetheless, I recommend this book highly to those working in medical sociology, the sociology of occupations, and the study of organizations.

DORRIAN APPLE

Boston University School of Nursing

Remotivating the Mental Patient. By OTTO VON MERING and STANLEY H. KING. New York: Russell Sage Foundation, 1957. Pp. 216. \$3.00.

In 1951 the Russell Sage Foundation commissioned a two-part study of patient care in mental hospitals. The first part, an experiment in improving patient care has been published as *From Custodial to Therapeutic Care in Mental Hospitals* by Milton Greenblatt, Richard H. York, and Esther Lucile Brown. The second, "a nationwide survey of encouraging trends in ward patient care," is described here.

During 1953 the senior author visited thirty mental hospitals selected by authorities in the field of mental health as "ones which had been able to devise in some areas of treatment new and promising methods of ward care despite generally adverse conditions." The report consists of selected anecdotal, inspirational case histories of the wards.

While anyone familiar with the drab horror of "back wards" can have only the deepest admiration for the devoted attendants and nurses whose efforts are described here, the authors' abdication of research evaluation as a part of their task makes the volume an exhortatory rather than a scientific contribution. Nowhere is data given to measure the success of the programs except for statements such as "those inside and outside the hospital felt that the men were much improved and happier." One of the wards is described as "The House of Miracles." Although the term is rhetorical, if even approximations to miracles are occurring in the back wards of the nation's mental hospitals, patients, their families, hospital personnel, and social scientists deserve a careful and controlled assessment of the circumstances which account for them.

JAMES A. DAVIS

University of Chicago

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THE UNIVERSITY OF CHICAGO PRESS

The American Journal of Sociology



Vol. LXIII

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THE AMERICAN JOURNAL OF SOCIOLOGY

Volume LXIII

MAY 1958

Number 6

AN EDITORIAL FOREWORD

ÉMILE DURKHEIM-GEORG SIMMEL

1858-1958

In the year 1858 both Émile Durkheim and Georg Simmel were born. Each in his own way has contributed so much to the development of social science that it would be ungrateful not to mark the occasion of the one-hundredth anniversary of their births without some special tribute. Consequently, the *Journal* dedicates this collection of essays to their memory.

The greatness of Simmel and Durkheim lies in two crucial contributions: first, each played an important part in his own time in giving life and body to our discipline in its earliest days; second, their works today continue to stand as inspiring models of scientific endeavor, providing sharp insights into scientific problems still under consideration.

There are many styles of commemoration. Rather than impose a somewhat artificial unity on the essays, we have permitted each scholar to find his own path and to honor Simmel and Durkheim in his own way. Some of our contributors show how their own present work is directly in the Simmel or Durkheim tradition. Thus Homans continues in the path of Simmel, concerning himself with the processes of interaction; Gold ties contemporary knowledge of socialization to the study of suicide, following through on the work for which Durkheim achieved such fame.

Other contributors have chosen to explicate the work of the men whom we honor here: Schnore presents a reinterpretation of Durkheim's *Division of Labor*; Selvin looks at *Suicide* from the viewpoint of the survey research methodologist. Alpert reminds us of the important methodological postulate that Durkheim felt to be the special mark of sociology as a science. Mills draws on Simmel's brilliant insights to point out crucial problems for the new field of small group research. Naegle and Coser concern themselves more with form than with content. Coser interprets the style of Simmel's work in terms of his marginal position in the German society of his day, and Naegle contrasts the different models of sociological activity presented by Durkheim and Simmel. And, finally, Wolff has commented on both Durkheim and Simmel as philosophers and as men.

The contributions to this issue collectively testify to the continuing vigor of the heritage left us by these two men. However excellent these essays, they can only present a partial assessment of the high places of Émile Durkheim and Georg Simmel in the history of social science, places not likely to be taken by the figures of our own or future generations.

PETER H. ROSSI
Editor

ATTACHMENT AND ALIENATION: COMPLEMENTARY ASPECTS OF THE WORK OF DURKHEIM AND SIMMEL¹

KASPAR D. NAEGELE

ABSTRACT

Simmel and Durkheim represent two sides of the study of social relations: the latter moves in almost a single line of specific and continuous inquiries; Simmel applies a constant concern with the mutual dependence of contrary elements to a huge range of phenomena. His many analyses are carried forward by a tragic theory of culture. Durkheim, by contrast, attends to the solemn forms of the serious life. Neither sustains a persistent concern with political phenomena. From their work emerges a view of social arrangements as involving coherence, differentiation, involvement, and alienation. Durkheim can serve as direct model for further work. Simmel cannot be directly continued, but he reminds us of what there is to see.

I

To recognize and then celebrate greatness in men who are committed to science is a fruitful contradiction. As an endless accomplishment, sociology, a sample of science, consists of work done and to be done; it is constituted by the published proposals, substantiated or speculative, that claim a place in a cumulative effort to state and explain the character and transitions of social arrangements. Once work is done, its author can be properly ignored. Science, after all, is a truly communistic matter. It admits of no private property. It is impersonal, though not inhuman. It is owned by those who can understand and increase it. If we were consistent—if we were only scientists or only sociologists—we would publish anonymously. Yet sociology must be created. It does not grow by itself, thus the conditions of its

growth are of consequence; they merit attention. In what sense, for instance, have Simmel or Park, Durkheim or Radcliffe-Brown contributed qualitatively *more* to the study of society than Schaeffle or Gidding, Gumplowitz or Ward? Surely, the growth of science forces on us the fashioning of a periodically revised sense of importance, even of a consensus of importance. Or how, in their temporal succession, can the contributions of Adam Smith, Spencer, and Durkheim, for instance, be seen as a balance of continuity and discontinuity? How can they be appropriated by a present generation?

It is a special question to see in what ways continuity and discontinuity among the efforts of scientific men make their particular contributions—"positive and negative." It may be wasteful to study work without reading what Schiller the poet said about play.

¹ I am well aware that in some respects this paper is both premature and fragmentary. It states a program of studies of Simmel yet to be carried out rather than conclusions. More often than not, it does not provide the evidence already in hand. Still, an anniversary must be celebrated when it comes. In daring to embark, as time permits, on a study of Simmel—the parts of which might one day come together as a kind of intellectual biography of him—I was encouraged by various influences and people, among them Dr. Michael Landmann and Margarete Susman, both of Sankt Gallen, Switzerland, and helped by an unsolicited grant-in-aid of the Ford Foundation. I must also acknowledge gratefully most helpful correspondence with Dr. Else Simmel.

Perhaps we should turn our backs to the past, start all over again, by writing down what we see, think, and hear, and let others order it to the impersonal sequence of revised proposals. The conditions of creativity in any one of us may well run counter to the demands for intellectual social reciprocity that are logically part of the idea, if not the history, of science. This is, of course, not meant as an argument against "continuity" in particular or against theory in general. Quite the reverse. The past is a

most necessary resource. But it can be useful only when we can select from it; just as we can ask scientifically relevant questions only by addressing the world through the queries implied by explicit distinctions. Theory yields such distinctions and such selection. Nevertheless, patterns of conceptual distinctions might have a better chance of productive survival if they were also allowed to grow some personal roots, to stand for translated experience that in turn could be corrected.

Ultimately, on this plane, all such questions will have to come back to the tough, twofold effort of seeing the enormous profusion and "severalness" of social arrangements in their vivid or dull, up and down "thereness," and of seeking to form just a few proposals that yield a description of the properties of social relations, the variant modes in which they are or could be organized, and the transitions to which they are subject. Society, as we can no longer forget, reveals its order only when its constituents—the individuals within it—are seen as necessary but insufficient. Their individuality is in turn necessarily dependent on a web of circles and arrangements. These never fully contain what they have helped to engender. But both society and science are products and demand producers. The form of the product, to be intelligible—be it a gesture or a monograph, drafted legislation, or an entreaty to a woman—has at least a double history: its author's and the meaning and consequences it receives from others. It has an inner and a shared history. Scientific accomplishments of the past are then constituents of a truly social and ideally open, yet professional, enterprise. As such they are the necessary elements of a growing configuration of public knowledge. But when gathered by their individual origins, they also constitute parts of the substance of ended lives. An author's work can be considered as the whole that the succession of his efforts yields. The properties of this whole are not only of biographical relevance: they can be useful facts in the detailed concern with the several and related meanings

of the ideal term "continuity" on the divergent planes of science, society, and one's own life and work.

This paper is a fragmentary attempt to see the configuration of Simmel's work by contrasting it with that of Durkheim, to see more particularly how they differed in their perceptions of the properties of social relations, to allude in passing to their brand of seriousness, and to draw from all this some implications for the opposing ways in which we can be their debtors.

II

At this distance Durkheim's work seems well contained by a few themes. Its distinct foci and boundaries give it a completeness. They also invite us, quite appropriately, to "take off" from there, to let him serve us as a model. The logic that arranged and interpreted the facts of suicide will do equally well with the puzzles of alcoholism or cross-cousin marriage. In both cases we need a distinction between rates and individual histories; a search for types among a countable, and hence apparently uniform, category of events; a commitment to explanations that seek to exploit the constituent elements of social relations and their organization as causes for the unequal distribution of phenomena, a distribution that is clearly not solely a matter of meaningless nature.

The themes combine rejection and acceptance. Part of the assertion that the social is something *sui generis* consists of a withdrawal from the image of a single, ultimate order—individual or physical—which alone is real and thus, the only context of all explanations. Instead, Durkheim dignifies society as a moral phenomenon that stands stubbornly beside nature, must be understood in its impersonal "thereness," much as though it were a collection of related things, but which must, in addition, be perceived for what it is and what it is not: a coherence of representations that coerce individuals by virtue of being able to fuse what is obligatory with what is desirable—a fusion analogous to the double face of the sacred. The sacred implies distance and the

forbidden. Yet it stands for qualities sought after, for an appropriation that traverses distance. Durkheim stays with his concern with society. He moves through a progression of empirical concerns: the coherence and differentiation of society; the support and burdens that different kinds of social orders provide their members; the ways in which ultimate commitments and modes of thought are constituted in the enduring demands, mediated and expressed through rituals which outlast individuals while, in principle, requiring them. The progression from *The Division of Labor*, via *Suicide*, to *The Elementary Forms of the Religious Life* will probably long remain a double model: as a fruitful trinity of researches into phenomena that one must apprehend as part of the very core of any and all social phenomena and as an inspired continuity that is cumulative.

But there is, of course, far more than that. There are the recurrent methodological examinations and revisions, driven on in the first instance by the wish to keep sociology a positive science, the recognition that social facts are moral, and the ambiguous insistence that they be treated as things. In one of these, a discussion of pragmatism,² we can see the persistent concern with the relative difference and connection between ideas and acts set out, as if under a microscope, in a condensed but decisive dissection of the forms and conditions of certainty.

Further, there are the discussions of socialism, individualism, and the position of intellectuals; of the place of elites in a democracy; of the character of the German mentality and the significance of World War I: pre-political diagnoses that do not keep Durkheim from a continuous reconsideration of the "spirit of discipline," which is "the essential condition of all common life."³

No wonder that the related structures of

morality and education become at once the labels of his academic chair and, in various forms, the titles of several of his collected series of lectures. In one of these collections the diverse and complementary bonds that divide and bind the several spheres of economic activity, political coherence, professional service, and familial life are characterized as much for their own sake as by way of demonstrating that the social is indeed the moral, while society cannot do without crime.⁴ Crime and punishment, the normal and the pathological—these are never far from Durkheim's attention. They provide a circle of facts, in quantitative or statutory form, that he likes to use or explain.

Yet he also turns from homicides and suicides, from the formative categories of thought, or from the dominant modes of large-scale social coherence and their historic succession, to the more immediate sphere of the family, the origins of marriage, and the prohibition of incest. He writes about moral education and education and sociology. In a very deliberate way he writes accounts of a very few predecessors. He finds in Montesquieu two ideas necessary to social science: the lawfulness of society and the existence of discoverable types of society. To formulate such types is to look for generic characteristics of social structure and their alternative combinations. Saint-Simon, Comte, Rousseau, and Schaeffle also explicitly occupy him. But, in the end, it is to the twin issues of anomie and education that he returns again and again. "What," one might paraphrase his insistent query, "are the conditions under which a complex contemporary society can avoid loss of direction or meaning? How adequate, given our growing knowledge of society, are such solutions as the variant forms of socialism? Is it not true that the coherence necessary for a large society requires a deliberate and specialized division of labor in which people balance a sense of the collectivity with a concentra-

² *Pragmatisme et sociologie* (Paris: Librairie Philosophique, J. Vrin, 1955), pp. 199–202.

³ Durkheim, *The Rules of Sociological Method* (Chicago: University of Chicago Press, 1938), p. 124.

⁴ *Leçons de sociologie: physique des mœurs et du droit* (Paris: Presses Universitaires de France, 1950).

tion on their own efforts?" In education Durkheim sees the agency by which society re-creates itself, an agency that is at once diverse and uniform. This agency, by the way, has received astonishingly little attention by sociologists in North America this last decade or more, while these very days it is singled out as the cause of Western society's failure to remain unequivocally pre-eminent in the realm of physical science and technology.

Neither man completed his work. Yet each left a distinct "whole" as well as a distinct style of question and answer.

III

Simmel seems almost the opposite of Durkheim, but then, as the French say, extremes meet. His writings are profuse. Happily, to return him to attention in Germany during this anniversary year, a selection of his writings, especially of earlier or less accessible pieces, has just been published under the excellently appropriate title "Bridge and Door."⁵

One finds in this collection an at least partially representative sample of the range of specific topics that Simmel discussed: fate, life, and death as the co-ordinates of "experience"; the character of history and of culture; religion as a mode of coherence of the self and not primarily as a matter of specific beliefs; landscapes and faces as types of unity confronting a beholder; the moral alternatives proposed by Kant and Nietzsche; the different ways in which a man's work and his life can be connected, as exemplified by Goethe and Rodin. The collection also contains some lesser-known sociological pieces on individualism, on meals (their regularity, sequence, and style, their double meaning), on the role of certain aesthetic categories in social arrangements and in ideologies. With extraordinary ease, Simmel begins with any one of a multitude of obvious events or ideas and ends by using them as appropriate illustrations of his basic

theme. Simmel's work is so diverse that one could easily be tempted to make a list of at least the major issues to which he pays attention. Yet lists give the impression of being linear. Simmel's writings do not form a single line of analyses successively growing out of each other. Rather, his work is connected but uncumulative. In the best sense, it is the work of an artist who seeks to work the way he saw his model, Goethe, live: re-creating, through the paradoxical use of terms, the experienced relations and activities of our selves and of the varieties of confrontations through which we live. Simmel's work includes several spheres, but it does not fall into disciplines.

It is vain to claim him primarily as a philosopher or as a sociologist. It is, necessary to see his sociological analyses as an accompaniment to *all* his work: they are not confined to what he himself might have labeled sociology, any more than his work itself is confined to any one discipline. The very range of his reflection is an instructive accomplishment. He wrote about the character of knowledge, but he discussed equally the nature of understanding of others and of historic events, as well as the processes which constitute our apprehension of the other when we see him "first." This last question, however, is pursued in a study of Rembrandt. Simmel focused his thought on the work of a series of men—Dante, Goethe, Michelangelo, Moltke, Nietzsche, Schopenhauer, Kant, George. He takes up each man not as a person but as the creator or exemplar of one or another alternative solution to a problem of life.

There are the recurrent essays on aesthetic matters: on ruins, mountains, landscapes, bridges, doors, vessels and their handles. These become, in fact, the occasion for further juxtapositions of opposites: ruins allow one to experience as the work of nature what originally must have been the work of men; handles link to an environment what is nevertheless a self-contained object placed and seen within it. One can turn from this with mild amuse-

⁵ Georg Simmel, *Brücke und Tür*, ed. Michael Landmann, in association with Margarete Susman (Stuttgart: K. F. Koehler, 1957).

ment or annoyed indifference, feel tempted to dismiss it as a play on words, as utterly irrelevant to the serious study of social matters. Still, in Simmel's view, "the play's the thing": the serious is often best caught in its translated forms—in caricature, in small talk, in the unique event. As a matter of fact, his essay on adventures holds up a whole parade of complementary notions concerning routine and disruption, accident and fate, periphery and center, repetition and irrevocability, beginning and end—all of which help constitute social situations. Simmel's explicit sociological scope is now known: like Durkheim, he sees in numbers a strategic starting point for accounting for certain differences between social arrangements, and for the direction that change can take.

While Durkheim fruitfully, yet questionably, proposes links among the complexity of social arrangements, moral density, and an increase in the numbers whose affairs must be co-ordinated, Simmel remains, in the main, among knowable groups of different size. True to his wish to explore an attitude; to begin anywhere, yet always from the same place; to be concerned far more with the *terminus a quo* than with the *terminus ad quem*, Simmel took up other themes: poverty, intersecting group memberships, conflict, secrecy, strangers and nobility, gratitude, competition and exclusion, the social functions of eyes and ears.

Cutting across these studies are several contributions to a sociology of women, beginning with an essay on female psychology (written in 1890) and including considerations of the feminist movement, the relations of women to militarism, and three long pieces on coquetry, "feminine culture," and the reciprocities of male and female. Simmel again seeks to formulate facts through stating contradictions: male and female are merely a sample of what we might now call the reciprocal differentiation of roles. As such, they imply one another. As categories, they also stand "by themselves"; the male represents our image of some coherent, impersonal, or demand-

ing reality; the female represents aliveness that has not yet been complicated by an elaborated self-consciousness. Such formulations are no longer likely to attract attention and research. But we can convert them into an approach to the study of types of moral commitment. It would seem that we balance a sense of the *reciprocity* of our contributions or standards with a belief in their ultimate "*so-ness*." Perhaps as adults we combine two moralities. In different ways Durkheim and Simmel formulated the respective character of these moralities, while Piaget creates experiments to present them to us as stages in the life-cycle.⁶

Par excellence, Simmel is a sociologist of intimacy. Closeness and anonymity, faithfulness and the faithlessness of fashion, private correspondence and adornment—these concern him in the first and the last instance. Wider society, as the more impersonal middle between specific selves and general ideas, is included in his dissection more as background than as foreground. General ideas, however, especially in the form of governing webs of attitudes, also attract him, especially the normative constellations, such as optimism and pessimism and love. Ultimately, he is moved and caught by the spectacle of a cumulative conflict between ourselves and the products of our selves. To be humanly alive is to create objects as part of a labor that divides the self into subject and object. Objects can and do grow in number. They outgrow us. To live, we must create the things that destroy us. They destroy us by the oppressive embarrassment of alien riches. Durkheim's anomie here finds a complementary notion: *omnia habentes, nihil possidentes*. It should be possible to translate this—into research—and to ask how people see the discrepancies between what they know and what they do not know, what they have and what they do not have. When are their relative deprivations less important than their sense of not

⁶ Jean Piaget, *The Moral Judgement of the Child* (London: Kegan Paul, Trench, Trubner, 1932).

having enough inner room for all that is in their grasp?

Simmel collected neither facts nor figures. He sought to represent through ideas what he defined as going beyond them. This forced him into the device of paradox. He may have been hypnotized by his own penchant.

Constantly he represents life as a stream of events. These come to confront their authors. Together events and authors are part of something else. This "something else" and something further is life. Life encompasses events and phenomena, persons and groupings. All these involve the facts of distance and exclusion. Life, like individuals and social arrangements, always goes further (while it lasts) but equally involves limits. These limits can be crossed. It is precisely the acts of crossing, of going further, of leaving something behind, that constitute the very character of aliveness.

By the same logic, boundaries—the divisions between inside and outside, here and there, now and then, I and You, We and They—constitute part of the recurrent structure that a sociologist has appointed himself to explain. Be it money or religion, art styles or the nature of conflict, ultimately the discrepancy between what one can say and what one experiences is the poignant puzzle for Simmel. He holds fast to a solution which would make the discrepancy itself both a gap and a bridge between "life" and the forms or the products of it. One book alone contains the theme on which his many books are variations: *Lebensanschauung: vier metaphysische Kapitel* (1918). We can go back to this source and rework it into a tentative scheme for seeing how people actually see themselves related to others: what do they mean by being "part of something," or "surrounded," or "on the edge"? We can ignore this and begin in the middle. For instance, we can concentrate on any one of a series of fragmentary observations on the properties of human groups or on the shifts when one social constellation gives way to another. Or we can take further some of the

inquiries he began on such topics as consolation or gratitude, secrecy or multiple group membership. In any case, we are bound to fail if we take him seriously by taking him literally. Formal sociology is dead. It confounds description and explanation. Simmel used many reciprocal distinctions to bring to mind and heal our immediate and receding surroundings so that, eventually, their structure can be explained. Formal sociology simply distributes such distinctions over haphazardly apprehended facts. I shall return to this point at the end. Three lessons, however, seem to lie between the attempt to reformulate his philosophy of life and the immediate use of selected collections of his particular proposals.⁷

He urges us, I think, to look for the "duplicity" in social arrangements, to determine the boundaries which make relations social, and to distinguish varieties of distance through which human acts can be sorted out.⁸

Admittedly, in the end, Simmel wishes to stay with the self. At least he never wants to leave it for long. But his intentions need not imprison his contributions. A meal, the settlement of a quarrel, a conversation—these samples of social arrangements exhibit in Simmel's perspective a "severalness" of divergent characteristics which nevertheless cohere. Forgiveness after a quarrel, for instance, can reconstitute a former solidarity on a higher level while exhibiting the reciprocities of acknowledgment and "forgetting" of injuries, of deliberate self-humiliation that runs the risk of rejection while seeking the reward of acceptance, and of a consensus on what

⁷ Besides the work of Theodore M. Mills, see, in particular, Robert K. Merton's provisional list of group-properties which is, *inter alia*, explicitly indebted to Simmel: "Continuities in the Theory of Reference Groups and Social Structure," in *Social Theory and Social Structure* (Glencoe: Free Press, 1957), pp. 310-26.

⁸ For a parallel formulation of Simmel's basic procedure see the circumspect doctoral dissertation by D. Levine on a comparison of Parsons and Simmel (University of Chicago, 1957, unpublished).

is appropriate behavior after a violation of expectations. Simmel bids us move social reality into focus with the help of a series of co-ordinates that constitute a set of mutually contrary ideas. More than that, he suggests that all social arrangements are in the first instance sustained, remembered, or rehearsed by selves, who thereby form social relations that can never fully include their parties. Even in excluding us, society has a place for us all. Yet social relations have boundaries; they are as much defined by what they are as by what they are not. Distance and proximity are, at the same time, names for distinguishing between others—related or not—and simultaneous attributes of all social relations. In addition, distance, being variable, stands for alternative possibilities of seeing the same phenomenon close at hand or far away. Sociology itself is a form of distance vis-à-vis the stream of recurrent confrontations of individuals in varying combinations of numbers.

IV

At best, these are reminders of the range and direction of the accomplishments of Simmel and Durkheim. A complementary and equally fragmentary comparison might still give the previous sections some coherence and lead on to the last questions: What properties of social relations does each man bring to our attention; how differently must the single word "continuity" be applied to this double stream of proposals; and how incomplete would our view of society be if we used only Durkheim and Simmel as our guides?

For the sake of clarity and brevity, but at the cost of accuracy, one could see Durkheim and Simmel as each exhibiting what the other is without. Their like features can then later become plain. Durkheim confronts gathered facts, including numerical regularities. He is concerned with method. He has an end in view and wishes to reach it through the answers to successive questions. He moves from answer to question, from the signs of unhappiness to the dis-

tribution of it, from the apparent sameness of a phenomenon to its typical variant forms, from the apparent associations of specific ideas to some "eternal" attitudes. Often he seeks to establish his own position by the successive elimination of rival alternatives. His is a succession of analyses of the generic features of a few strategic elements of society: work, attachment to one's self, and to others, religion, education, patterns of injunctions. He pays explicit attention to the succession of shifts within these. He also attends to the fact that society moves into the future by educating all its young but dispersing them to different places. He keeps in mind always that society is in transition, although wider phases—like his own times—he recognizes as "in-between times," jointly characterized by "moral mediocrity" and the possibilities of the dispersal of coherent images that can be shared. Yet he is not prepared to see the process as a whole as tragic. Discipline, a sense of the whole, occupational groupings, and meaningful occasions for remembering and so continuing the collective agreements that can distribute solemnity and direction seem almost sufficient resources for continuity.

In a series of cumulative monographs Durkheim incloses economically a sequence of questions which lead to the progressive differentiation of the original notion that social relations constitute phenomena in their own right. He, too, sees the "severalness" of person and social pattern, and he speaks about the non-contractual elements *in* contract, the impersonal elements *in* personality, the duality *of* the individual. Yet the methodological device of considering the social as a "thing" facilitates or expresses the substantive view of the social as in serious respects impersonal, as that which (eventually) confronts (and contains) the immediate self. This view leads Durkheim to write about solemnity. The "same" confrontation in Simmel's hands, writing on the inclined plane of tragedy, becomes a matter of alienation, of the strange, of proximity and distance—a theme which

Durkheim, however, also takes up to describe our encounters with taboos, with the sacred. Simmel, in heuristic contrast, *discloses* a very full world of transitory occasions representing the chronic duality of the self, which thus becomes a pre-eminent social condition and accomplishment. Potentially dramatic or apparently ordinary occasions and a huge panoply of confrontations with the specific accomplishments or completed lives of others, with the contrary features of lifeless objects in space and the meaningful, but puzzling, coherence of faces and landscapes, styles and money—all are taken up in unceasing succession to be analyzed alike. Subsequent analyses do not grow out of previous ones. But all of them seek to catch the flow of exchange in time between people. Simmel tells us what he sees. While he represents it, it is alive. He stays within the structure of vivid experience and enlarges it at the same time. Simmel states his claims via *contrary* notions that claim *equal* relevance to the *one* phenomenon under discussion. In this way he manages to isolate and attract facts with one idea and then attract them away again to another idea. This is more than a literary accomplishment. It proceeds expressly with the help of clear abstractions. Experience is translated into many proposals, all open to examination and revision. Yet this involves style rather than method. It cannot be directly continued. Ironically, for all its aliveness, it utterly bypasses the strategic fact that society also includes children. Otherwise, Simmel, perhaps as no one else, forces the wealth of the surrounding world, the strangeness of the familiar, upon us. Now *we* must choose, but now we also *can* choose.

Still, they diverge in their roles as forefathers. Durkheim one can follow as such: rates, index formation, types of social acts and of social relations, the search for the functions of social patterns—all persist as direct puzzles or resources. Simmel wants no immediate descendants. He expects “only” to provide resources that others can use for their own purposes. He does not ex-

pect his successors and debtors to remember whence came the notions that, in fact, they learned from him.⁹ One can confront certain of his specific propositions with further or actual facts, as Mills and Hawthorn have done,¹⁰ with promising results, or one can, like Merton, collect Simmel’s suggestions as part of the continuing effort to set forth a coherent identification of the generic features of social groups. Similarly, Simmel’s ideas could be much further exploited than they have been in the direction of a sociology of work. After all, the serious gap between producer and product and the playful coherence between host and guest are characteristic themes of Simmel. As no one else’s, his thoughts lead to the question: What is the significance of the difference between what we study and what we do not study within the boundaries of social phenomena? He had the courage to start with the visible, and the imagination to end nowhere. If we could, step by step, walk to the limits of his work, we would, I think, wrest from it further resources than its “content” can yield alone. But this takes time.

V

Simmel and Durkheim are not only complementary within the gathering enterprise of sociology; they also converge. Both see social relations as involving a confrontation of several dimensions. To Durkheim the emphasis is on the character of commitment; to Simmel, on involvement. The first, as we know, speaks of constraint as the combination of duty and desire. The second sees social relations as typical balances of distance and proximity.

⁹ See both the Introduction to *Philosophische Kultur* (Potsdam: Gustav Kirpenheuer Verlag, 1923), and the introductory quotation in *Fragmente und Aufsätze* (Munich: Drei Masken Verlag, 1923).

¹⁰ For one example of Mills’s work, see his essay elsewhere in this issue. H. B. Hawthorn has examined some aspects of the proposals on secrecy in “A Test of Simmel on the Secret Society: The Doukhobors of British Columbia,” *American Journal of Sociology*, LXII (1956), 1-7.

Durkheim analyzes sanctions which define and greet offenders, who, through their offense, create gaps between the actual and the ideal. Simmel traces out the functions of gratitude and faithfulness, since he sees in these two enjoined dispositions resources for the continued coherence among persons. Both, then, are concerned with the conditions that must obtain for people to act attached. Both distinguish varieties of attachment. Both are prepared to admit some generic features of attachment.

Still, Simmel and Durkheim seem to come back to four reciprocal themes: coherence, differentiation, alienation, and involvement. These in turn are perhaps two pairs of opposites. Social relations are constituted in some mutual regulation that can be experienced at the same time as an impersonal but relevant set of demands. They involve an agreement. Alike, they involve a temporal succession of exchanges. They involve the taking and giving of turns, the creation of parties to the agreement. As melody-like structures, they raise the matter of commitment to the future and to the memory of the past. Even where "nothing" is produced—as in friendship—the previous coherence and differentiation come to constitute something left behind, something which raises the matter of appropriation and alienation. Given the impersonal component, finally, to all personal matters, any one person in principle can select "how much" or how seriously he "puts himself" into the very succession of events constituting his life, while always remaining less than all of it. The full and adequate elaboration of these claims demands another paper.

Both Simmel and Durkheim direct their discoveries toward the serious life and the serious in life. Simmel, unlike Durkheim, considers in some detail the realm between the ordinary and the solemn, or between the actual and the ideal. He analyzes coquetry, sociability, and acting. These can hold up mirrors to the "real" or the "serious." They enact the distinctions and discrepancies between such contrasts as trivial

and important, actual and possible. As "play-forms" of society, they help lay bare the nature of the dumb and considered calculations, private or shared, that in turn help constitute social arrangements. The latter, of necessity, both involve and deflect images of relative importance. In that way they yield economies of exchange in emotions, goods, or ideas. The large-scale coherence of such economies did not explicitly concern Simmel. Instead, he writes often about our chances of ordering events to a notion of periphery or core. Questioningly, he comes to the conclusion that "deeper" people can survive only through maintaining a measure of superficiality. Ultimately, whatever their logical compatibility, the several lines of moral assessment by which we constitute ourselves in society must clash insanely in any member of society who really thinks these matters through. Here Simmel meets Weber, who supposedly answered the question why he studied sociology with the retort: "To see how much I can stand!" By contrast, Durkheim is concerned with the aliveness of the resources to which collectivities can remain committed. Where Simmel sees conflict and alienation, Durkheim sees emptiness and the uncommitted pursuit of rituals as the inclusive, disruptive forces given with the very constitution of society itself.

Yet neither man, for any length of time, keeps in focus national societies and the representative agencies of such societies—government and parties. Both write about Germany and about socialism and about war. Yet both seem apolitical sociologists. Neither appears to be centrally concerned, as Weber was, with the polity. In Simmel's case one suspects that, for all his sophisticated insight into the subtleties of daily life, he was blind (and not just indifferent) to the shifting balance between democratic and totalitarian tendencies in the Germany in which he lived. Neither Simmel nor Durkheim, then, is adequate, by himself or with the other, as a guide to an attempt to single out the major features of societies. We do not get from them a vivid sense of

the large and rivalrous political units that claim men's lives and loyalties. Neither Durkheim nor Simmel asks about the ways in which national boundaries create political and other differences, while such boundaries are at the same time cut across by strategic similarities, especially in the sphere of occupational ranking and the judgments of importance that such ranking can imply. Admittedly, their relative political indifference does not make their work irrelevant to those who wish to examine those commitments and structures which involve the co-ordination and representation of men in their capacity as members of civil society or as voters, legislators, and government officials. Besides, it is primarily Simmel, of the two, who is the apolitical thinker. Durkheim, after all, concerned himself with elites and with socialism.

As two major creators of sociology, Simmel and Durkheim, then, complement each other. One lays before us a range of phenomena with which we can now compare the actual range of issues that we have come to include in our efforts. His distinctions remain to be translated into the lan-

guage of research. But his scope, where it exceeds ours, can well be used at once for an assessment of our intellectual patterns of inclusion and exclusion. Admittedly, his intent to state on the plane of ideas the great flux of inner and outer occurrences may have to be subordinated by us to a wish to explain only some of them. Yet how should we best choose? The other, by contrast, presents an economy of continuous questions rather than a discontinuity of phenomena shown to have a constant structure. He produced a monograph that is a model of procedure. Simmel, for all his wish to write about life, cannot show us how to proceed—perhaps precisely because he himself succeeded so persistently. But he can tell us clearly how much there is to ask about. Between them, Durkheim and Simmel represent the two sides of the study of social relations: the effort to bring social reality into a perspective that can be shared and the effort to select from it all a sufficiently clear question, whose answer will explain some strategic regularity.

UNIVERSITY OF BRITISH COLUMBIA

THE CHALLENGE OF DURKHEIM AND SIMMEL

KURT H. WOLFF

ABSTRACT

Two newly published works—a Durkheim translation and a collection of Simmel writings in German—provide the starting point for an assessment and a comparison of them as men and philosophers. Consideration is given to their philosophical aims, as well as to their proximity to, and distance from, contemporary sociologists. Simmel is held to be far less time-bound than Durkheim. His aim is preponderantly theoretical and ahistorical; Durkheim's, predominantly practical and historical.

The recent publication of two books—a new Durkheim translation and a German collection of writings by Simmel¹—is not by way of commemorating the one-hundredth anniversary of the authors' births, but we can hardly be prevented from trying to relate these works to such a commemoration. On this occasion may we not ask, anew, who were these men, what did they attempt to do, what did they accomplish, and, also, who, in their eyes, are we and what are *we* trying to achieve?

We must remember about both Durkheim and Simmel, it seems to me, a whole avalanche of things: possibly that they are our fathers and our tyrants and, aside from being sociologists, that they are men and philosophers. I begin with the last element in this complex because they may just manage to lord it over us if we ignore them as men and philosophers and tend to take them at their face value as sociologists.

In the first place, they are philosophers by training and (though Simmel more clearly than Durkheim) by profession; in their writings it is stated publicly. But this is only the most "sociological" sense of the

predication. More relevant, their work in sociology is based on philosophical assumptions, raises philosophical questions, and has philosophical aims. The first two of these characteristics, of course, apply to all scientific, hence also to all sociological, work; but the last, a matter of intent, does not. Thus it may be well to remember all three. As to "philosophical assumptions," it is enough to recall that Kant was a major influence on both Durkheim and Simmel, even though he did not furnish them with quite the same viatica. "Philosophical questions" refer to characteristically Kantian queries—ontological, moral, epistemological queries—concerning the nature of reality and of those parts of it (such as society, religion, the individual, history) that our authors singled out, and our relation to reality, particularly the relation of knowledge. "Philosophical aims" include the establishment of sociology as a science, demanded by the time—the same time—in which Durkheim and Simmel lived, even though they differed in their conceptions of, as well as in their involvement in, both the sociology to be founded and the time which argued such a goal. The goal, of course, they share with most of the earlier practitioners of our discipline, but it is no less philosophical or less worthy of rein-spection for that.

I

Durkheim's *Professional Ethics and Civic Morals* is the translation of *Leçons de sociologie: Physique des mœurs et du droit* (1950), "a course of lectures given by

¹ *Professional Ethics and Civic Morals*. By Émile Durkheim. Translated by Cornelia Brookfield. Preface by H. N. Kubali. Introduction by Georges Davy. ("International Library of Sociology and Social Reconstruction.") London: Routledge & Kegan Paul, 1957.

Brücke und Tür: Essays des Philosophen zur Geschichte, Religion, Kunst und Gesellschaft. By Georg Simmel. In collaboration with Margarete Susman. Edited by Michael Landmann. Introduction by Michael Landmann. (With a bibliographical appendix.) Stuttgart: K. F. Koehler Verlag, 1957.

Durkheim between the years 1890 and 1900 at Bordeaux and repeated at the Sorbonne, first in 1904, and then in 1912, and revised some years before his death" (Preface, p. ix). While this information makes it fairly certain that this material precedes Durkheim's last major work, the *Elementary Forms*, it is not specific enough to locate it within the chronology of his three earlier important books, all of which appeared in the 1890's, although part of the manuscript (p. x) stems from 1898-1900, that is, it is later than the *Division of Labor*, *Rules*, and *Suicide*. Still, most of what this book tells us about the development of Durkheim's thought requires the assembling and sifting of internal evidence, a task I shall not broach.

The book consists of eighteen chapters, three on professional ethics, six on civic morals, one on murder, one on theft, three on property, and four on contract. Its concern, characteristic of Durkheim, is mixed: orderly exposition and plea—among other things, for professional groups and for accepting and acting on an evolutionary view of history or social change. We have problems, we are in trouble, Durkheim says; what are these problems; what must we do? Where do we come from, and might this tell us how we should move? "One of the gravest conflicts of our day" is between the "national ideal" and the "human ideal," between "patriotism and world patriotism" (p. 72). It could be solved if "civic duties" were to become "only a particular form of the general obligations of humanity," which would be in line with evolution (p. 74): "it is human aims that are destined to be supreme" (p. 73); societies could "have their pride, not in being the greatest or the wealthiest, but in being the most just, the best organized and in possessing the best moral constitution" (p. 75). This, to Durkheim, appears more feasible than does a world society, a solution of the problem only "in theory"; and a "confederation of European States" is dismissed as just another "individual State," not "humanity" (p. 74).

This series of arguments shows how Durkheim's academicism handicaps his practical concern: the consideration of a European union leading to world society is dropped because Europe is not the world. But his practical concern does not leave him alone: as long as there is no world society, there can be no world morality, since "man is a moral being only because he lives within established societies" (p. 73), "civilized," quite generally, meaning "socialized" (p. 25). This blunt equation reflects Durkheim's "sociologism," a limitation often criticized—and once more in this book, in Georges Davy's interesting Introduction (esp. p. xliii). Durkheim himself, however, has his doubts about the exalted place he has given society. He does not feel at ease with its large modern variety (cf. pp. 15-16, 60-61), and his doubts go back to his first book. Even there, he tries to find grounds on which to applaud the development from "mechanical" to "organic" and to argue the moral nature of the division of labor but finds his optimism dampened by the realization of anomie and in need of a fresh impetus, provided by the vision of a society reorganized on the basis of professional groups.

The argument in favor of professional groups is restated in the first three chapters of this new book, but it appears in many other places in the book as well. It culminates in some practical proposals (pp. 37 ff.; see also 94-97) which are in keeping with Durkheim's evolutionary conception of social change. Social change itself—which must always be preceded by careful reflection (p. 90)—has brought us to the age of democracy, which he characterizes in this way: the democratic state is distinguished by "(1) a greater range of the government consciousness, and (2) closer communications between this consciousness and the mass of individual consciousnesses" (p. 88); democracy is

the political system by which the society can achieve a consciousness of itself in its purest form. The more that deliberation and reflection and a critical spirit play a considerable part in

the course of public affairs, the more democratic the nation. . . . [Democracy] is the form that societies are assuming to an increasing degree [p. 89].

Reflection, furthermore, increases readiness to change, while "sacredness," to use the term employed by Howard Becker in a similar proposition, makes change difficult (pp. 84, 87). Finally, democracy "is the political system that conforms best to our present-day notion of the individual" (p. 90), which is that of the "autonomous" individual who understands "the necessities he has to bow to and accept[s] them with full knowledge of the facts" (p. 91).

Durkheim believes that history tends toward such autonomy (pp. 56, 68, 112). His optimism is supported by his conception of the state. The state is the "social brain" (p. 30) which "decides for" the society (p. 49) but "does not execute anything"; its "principal function" is "to think" (p. 51). It increases its functions and consciousness along with the development of individualism (p. 57). This "in its essence individualist" (p. 69) view is to overcome the "mystic" conception, according to which the state has aims higher than the individuals' on whom at best it may shed some rays of its glory (pp. 54, 64). Yet, if the state "is to be the liberator of the individual, it has itself need of some counterbalance; it must be restrained by other collective forces" (p. 63), that is, by professional groups.

This analysis, these assessments, Durkheim insists, are superior not only to the Hegelian *mystique* (p. 64) but also to the views of classical economists and Socialists alike (pp. 10, 15, 29, 122, 216). Neither economists nor Socialists, for instance, see the state for what it is, "the organ of moral discipline" (p. 72); nor would a shift from private to collective ownership of the means of production solve our basic problem, which is the infusion of moral rule into economic life (p. 30). This is an instance of Durkheim's conviction that the examination of our past shows us that things are as he sees them. Nevertheless,

the right to say that this or that form . . . must disappear, requires more from us than merely showing that these forms conflict with an earlier principle. There still remains to demonstrate how they were able to establish themselves and under the influence of what causes, and to prove that these same causes are no longer actually present and active. We cannot demand that existing practices be put down on the score of an *a priori* axiom [pp. 124-25].

Here we have in a nutshell Durkheim's conception of social change, including its far-reaching confusion. It commissions causal explanation to decide on how things are and hang together but smuggles the "Ought" back into the "facts" by giving causal explanation a second task, that of serving as a recipe for practice. The classical case of this confusion is *Suicide*, which opens with rigorous causal analysis; then, in effect, abandons the law of one-cause-one-effect laid down in the *Rules*; and ends up in a vision, plausible and troubled, of the relations among types of social cohesion and types of suicide and in practical questions of policy. Durkheim confuses or mixes history with social change, interpretation with explanation (on this cf. Davy, toward the end of his Introduction), practice with theory, ontology with methodology, plea with exposition. Once these dichotomies are espoused, once the two elements in each of them are separately torn out of reality, as they are by Durkheim's positivism and in his strenuous effort to establish a scientific sociology, their synthesis is impossible, its only deceiving hope being verbal legerdemain.²

Durkheim was moved by two forever muddled inspirations—the improvement of our society and the development of an in-

² Another element in Durkheim's makeup as a sociologist is his conservatism, shown in the present work in some of the conceptions mentioned, as well as in his poor grasp of power and stratification and his respect for the status quo (cf. Lewis A. Coser, "Durkheim's Conservatism and Its Implications for His Sociological Theory," to be published in a volume commemorating the hundredth anniversary of Durkheim's birth [Ohio State University Press, 1958]). I must also abstain from talking about what are, from a theoretical and sociological standpoint,

strument carefully and passionately forged to be inadequate to the task. He was a man who either eats his cake or his bread and is poor: reformer, historian, philosopher; or keeps it and starves: theorist, scientist, sociologist. In his search for what he could believe in as real, he found himself incapable of accepting anything but Society. He sanctified it as the source of all that he loved—religion, morality, knowledge; he sanctified it as the Reality behind all these realities—and mustered all the more fervor dissecting it in his “piacular” rites of the scientist who longs for a lost sacred world.

Something of this sort may suggest Durkheim’s philosophical aims. In less emphatic metaphors, he wanted to design a society and wished his fellow men to accept it—a secular society, to be embraced with sacred passion. This is one of the numerous ties that connects him with his predecessor, Comte, and distinguishes him from his contemporary, Weber, who affirmed secularization as definitively as Durkheim did but who suffered from his clearer knowledge that one meaning of such affirmation is to accept “the fundamental fact” of being “destined to live in a godless and prophetless time.”

Do we not recognize ourselves? Do we not have aims similar to Durkheim’s and perhaps similarly confused? Is not Durkheim thus one of our fathers, and must we not emancipate ourselves from him? And is this not painful because we love him and, in outgrowing him, must go through the pain that was his own? Is he, then, not one of us, a little older, to be sure, since he helped to push us to where we are?

II

With Simmel matters are quite different. We hardly know him yet. We are just discovering, thanks to new translations, that

the most challenging chapters of the present book, those on property and, above all, on contract, the latter fully justifying Parsons’ praise of Durkheim’s treatment of the subject (cf. *The Structure of Social Action* [Glencoe, Ill.: Free Press, 1949]).

there is more to him than the invention of formal sociology, which the critical and enthusiastic Albion W. Small presented to the readers of his *American Journal of Sociology* in the 1890’s and which has long since demonstrated its academic respectability in one history of sociological thought after another. But, as yet, there are only a few of us for whom he is among the fathers. Emancipation is still far off because we have not even gone through domination and love. Perhaps we can take another road, learning from the history of our relations with Durkheim.

Simmel reminds us of things we tend to forget but would gain from remembering. The central concern of Comte, Durkheim, Weber, and many other nineteenth- and twentieth-century sociologists and philosophers was the time in which they lived, particularly as the promises of liberalism and the Enlightenment were being broken, ever more incontrovertibly, before their eyes. Unlike theirs, Simmel’s concerns only included that over his historical period. It was not primary. A remarkable sociologist, he was, much more clearly and purely than Comte or Weber or any of those referred to (or than many professional philosophers), a philosopher, a man who “wonders,” and who wonders even about received notions of which, since he was an unusually alert, perceptive, sensitive person, he had a wealth at his disposal.

He was far less deeply time-bound than Durkheim, although, of course, his time influenced him. Thus, for instance, he thought that people who were on their way to self-realization and abhorred the detours which led them outside themselves hated “culture” (p. 90): here a nineteenth-century bourgeois notion of “culture” is slipping in. He thought that it is the nature of the inner life to seek artistic expression in self-contained forms which contrast with its dynamic and perhaps torn character (p. 99): this is directed against expressionism—he thought that it is the essence of art to present the human body in a way which shows it controlled and

unified by mind or soul (p. 132); this would disqualify much twentieth-century art. Or he thought that it is inherent in the sociology of the meal for the dinner table not to exhibit "the broken, differentiated, modern colors but the broad, glossy ones which relate to wholly primary sensitivities: white and silver" (p. 248). Simmel occasionally mistook matters of his time as timeless, but such a confusion is not constitutive of his achievement as Durkheim's very different one is of his. It rather reminds one of a distracted professor's pet notions or absent-mindedness.

To go beyond characterizing Simmel as a wonderer whose locale, rather infrequently, intrudes, it is convenient to indicate the content of the twenty-eight pieces brought together in *Brücke und Tür*. In time, they range from 1896 to 1918, the year of his death. In content, they are placed under six headings: "Life and Philosophy," "History and Culture," "Religion," "Aesthetics and Art," "Historical Figures," and "Society." Only three of them, or less than one-sixth—"The Field of Sociology," "The Metropolis and Mental Life," and "The Individual and Freedom," all under "Society"—are available in English.³ Of the title essay, Michael Landmann, the editor, writes:

As one of the most beautiful examples of Simmel's way of letting himself be inspired by the nearest things [around him] and wresting their ultimate meaning from them, . . . ["Bridge and Door"] is placed at the beginning. The human capacity of "connecting" which it isolates and elucidates is so characteristic of Simmel himself that . . . its title . . . seemed suitable as that of the whole collection [p. 271].

Man, in the title essay, is presented as connecting and separating, building bridges and making doors, which can be opened and closed. Does the doorlessness of con-

temporary American living rooms symbolize or promote the disappearance of both privacy (separation) and—since doors can also be opened, while no-doors cannot—freedom? It is a Simmelian kind of question, although American homes were not among his "nearest things." Yet many items might and did become the occasion for his thought, which skipped time and place and could descend on any of them:

While the world surely determines what the content of our cognition shall be, but only because cognition determined beforehand what can be world to us, so fate surely determines the life of the individual, but only because the individual chose, by a certain affinity with them, those events on which it can bestow the meaning whereby they become his "destiny" ["The Problem of Destiny" (1913), p. 13].

For human life has a double aspect, causality and meaning, and, without both of them, "destiny" cannot emerge. Animals follow only causality; gods, only meaning; man alone must live with both. It is this same distinction between causality and meaning (also cf. pp. 51, 76, 86) which, in a sociological frame of reference, we know from Max Weber's very definition of sociology (causal explanation and understanding), and whose inadequate appreciation I urged as a characteristic of Durkheim's.

It plays a central role in Simmel's mature view of history. His paper "On the Nature of Historical Understanding" (1918), relevant to an illumination of theories of understanding, including historicism, of the nature of the Thou, the mind-body problem, and the concept of secularization, in effect distinguishes between intrinsic and extrinsic understanding⁴ ("We should never understand the What of things from their historical devel-

³ See *The Sociology of Georg Simmel*, translated, edited, and with an introduction by Kurt H. Wolff (Glencoe, Ill.: Free Press, 1950), pp. 3-25, 409-24, and 58-84; the last of these is a later and much longer version than the essay reprinted in *Brücke und Tür*.

⁴ Except for terminology, this is very similar to Karl Mannheim, "The Ideological and the Sociological Interpretation of Intellectual Phenomena" (1926) (manuscript translated by Kurt H. Wolff), although to my knowledge Mannheim nowhere refers to Simmel in the context of his discussions of problems of interpretation or understanding.

opment if we did not somehow understand this What itself" [p. 77]). This dichotomy is related to the one just referred to (causality versus meaning), as well as to its variant, the objective versus the historical (psychological, meaningful):

Psychological development is controlled and made understandable by objective development, and objective by psychological. This signifies that both of these are only sides, methodologically made independent, of a unity, of the historically understood event. . . . For life can be understood only by life. To this purpose, life lays itself apart in strata, each of which mediates the understanding of the others, and which, in their mutual interdependence, announce its unity [p. 83].

This passage, written in Simmel's last period, that of the philosophy of life, contains, in addition to the theme of the supremacy of life, two others: what may be called the "autonomization of parts of a whole" and the "understandability of a whole only through its parts." These three themes are connected. For instance:

The Marxian scheme of economic development, according to which in every historical epoch the economic forces engender a form of production which is adequate to them, in which, however, they grow to magnitudes no longer manageable within that form but bursting it and creating a new one for themselves—this scheme is valid far beyond the economic sphere. . . . Creative life continuously engenders something which itself no longer is life, something life somehow runs up against, something which opposes it with a claim of its own, and which cannot express itself except in forms which are and mean something in their own right and independently of it ["Transformations of Cultural Forms" (1916), pp. 98–99].

That the part of a whole becomes an independent whole, growing out of and claiming its own right over against it; this, perhaps, is altogether the most basic tragedy of the spirit—a tragedy which in the modern era has reached its fullest development and has taken over the direction of the cultural process ["Philosophy of the Landscape" (1913), p. 143].

It is noteworthy and characteristic of Simmel that, despite its strong tie to Hegel-

Feuerbach-Marx's "alienation," Simmel does not apply his idea of the autonomization of a part to an interpretation of his time, as Durkheim did with the concept of anomie, which is no more closely related to "alienation." Rather, less time-bound philosopher that he was, he chooses that conceptual aspect of "alienation" which he could best put to epistemological purposes, the illumination of the process of understanding. Hence the connection with the second theme, the understandability of a whole only through its parts: "Perhaps the nature of our activity to us is a mysterious unity which, like so many other unities, we can grasp only by splitting it up" ("On the Metaphysics of Death" [1910], p. 32). Or in respect to the problem of understanding a philosopher and his work:

When we infer an innermost personality from . . . [its] achievement, and again understand the achievement through the personality, this [procedure] may well be circular. But it is one of those circles which are unavoidable for our thinking; it . . . merely [corresponds to] the complete unity of the phenomenon which expresses itself in the fact that each of the elements into which we split it up becomes understandable only through the other ["On the History of Philosophy" (1904), p. 41].⁵

This suggests the last of the themes that I wish to call attention to in this wholly fragmentary sample, designed only to lead the reader to Simmel's work itself. It is that of relatively autonomous and irreducible attitudes toward the world, or the theme of "worlds," connecting Simmel with Husserl and later developments in phenomenology, especially in the work of Alfred Schutz.⁶

The great categories of our inner life—Is and Ought, possibility and necessity, wish and fear

⁵ Cf. also pp. 102–3 and 149 and cases collected in Donald N. Levine, "The Structure of Simmel's Social Thought," and Rudolph H. Weingartner, "Form and Content in Simmel's Philosophy of Life," papers to be published in a volume commemorating the hundredth anniversary of Simmel's birth (Ohio State University Press, 1958).

⁶ Cf. Weingartner, *op. cit.*

—form a series through which pass the objective contents of consciousness, the logically fixable conceptual meanings of things. These categories may be compared to the various states which one and the same chemical substance can adopt, or to the multiplicity of musical instruments on which one and the same melody, but every time in a particular tone color, can be played. Perhaps it is only different accompanying feelings which show us the same objective content now as being, now as non-being, now as an Ought, now as hoped for—or more correctly: these feelings *mean* that that content now is one and now the other. According to its over-all posture, our soul responds to the same content or perception with completely different attitudes, thus giving us completely different meanings of it [“Contributions to the Epistemology of Religion” (1902), p. 106].

Or:

In and of themselves, religion and art have nothing to do with one another . . . because each of them by itself alone expresses, in its particular language, the whole of existence. One can conceive of the world religiously or artistically, practically or scientifically: it is the same contents which each time, under a different category, form a cosmos of a consistent and incommensurable character. Our soul, however, with its short-lived impulses and its limited ability, is incapable of developing any of these worlds as wholly as it ideally demands. Each of them remains dependent on the haphazard stimuli which permit now this, now that, portion of it to grow up in us. But it is precisely the fact that these world images lack the self-sufficient rounding out called for by their objective content which creates the deepest vitalities and psychic patterns, because it urges each of these images to take from the others impulses, contents, and challenges which, were it completely developed, it would find in its own inner structure [“Christianity and Religion” (1907), p. 140].

III

If Durkheim’s philosophical aim is to infuse morality into the society of his time, thus being predominantly practical and historical, Simmel’s is to understand the world and is, above all, theoretical and

ahistorical. If it is true that we feel closer to Durkheim because our aims resemble his more than they do Simmel’s, it is also true, I think, that in a sense we need Simmel more urgently than Durkheim. Being deeper-seeing and more self-conscious, he has, on a systematic, theoretical, ahistorical view, more for us to tackle. Our indebtedness, as sociologists, to Durkheim has been driven home to us with poignancy and conviction, notably by Talcott Parsons and Robert K. Merton, and some of Simmel’s achievements in sociology have recently begun to be appreciated, rediscovered, and made use of in various fields, especially in the study of small groups and the phenomena of secrecy and secret societies. But I have tried to comment on Durkheim and Simmel less as sociologists than as men, endeavoring to show some of their philosophical concerns as the concerns of men who lived in a given time and place and who, as men, also to an extent transcended them.

Together they challenge us to do right by them and to do better. More specifically, we may want to identify Durkheim’s confusions and avoid them, being as fully aware as we can of their seriousness and pregnancy, and we may wish to pay more attention than Simmel did to the relation between a theoretical and a historical preoccupation. Both, though in different ways, also invite us to reconsider our own and sociology’s philosophical premises. Of course, we can learn from them many more things in respect to which we want to do them justice or surpass them. But, unfortunately or fortunately, even the narrow, selective focus of the present allusive observations may serve to remind us that there is no textbook which teaches us how to approach them or go beyond them—how, if you will, to commemorate them. They are a challenge to us, too, as men, to the best in us, and on *this* ground, let us try to remember, as sociologists.

OHIO STATE UNIVERSITY

SOCIAL BEHAVIOR AS EXCHANGE

GEORGE C. HOMANS

ABSTRACT

To consider social behavior as an exchange of goods may clarify the relations among four bodies of theory: behavioral psychology, economics, propositions about the dynamics of influence, and propositions about the structure of small groups.

THE PROBLEMS OF SMALL-GROUP RESEARCH.

This essay will hope to honor the memory of Georg Simmel in two different ways. So far as it pretends to be suggestive rather than conclusive, its tone will be Simmel's; and its subject, too, will be one of his. Because Simmel, in essays such as those on sociability, games, coquetry, and conversation, was an analyst of elementary social behavior, we call him an ancestor of what is known today as small-group research. For what we are really studying in small groups is elementary social behavior: what happens when two or three persons are in a position to influence one another, the sort of thing of which those massive structures called "classes," "firms," "communities," and "societies" must ultimately be composed.

As I survey small-group research today, I feel that, apart from just keeping on with it, three sorts of things need to be done. The first is to show the relation between the results of experimental work done under laboratory conditions and the results of *quasi*-anthropological field research on what those of us who do it are pleased to call "real-life" groups in industry and elsewhere. If the experimental work has anything to do with real life—and I am persuaded that it has everything to do—its propositions cannot be inconsistent with those discovered through the field work. But the consistency has not yet been demonstrated in any systematic way.

The second job is to pull together in some set of general propositions the actual results, from the laboratory and from the field, of work on small groups—propositions that at least sum up, to an approximation,

what happens in elementary social behavior, even though we may not be able to explain why the propositions should take the form they do. A great amount of work has been done, and more appears every day, but what it all amounts to in the shape of a set of propositions from which, under specified conditions, many of the observational results might be derived, is not at all clear—and yet to state such a set is the first aim of science.

The third job is to begin to show how the propositions that empirically hold good in small groups may be derived from some set of still more general propositions. "Still more general" means only that empirical propositions other than ours may also be derived from the set. This derivation would constitute the explanatory stage in the science of elementary social behavior, for explanation is derivation.¹ (I myself suspect that the more general set will turn out to contain the propositions of behavioral psychology. I hold myself to be an "ultimate psychological reductionist," but I cannot know that I am right so long as the reduction has not been carried out.)

I have come to think that all three of these jobs would be furthered by our adopting the view that interaction between persons is an exchange of goods, material and non-material. This is one of the oldest theories of social behavior, and one that we still use every day to interpret our own behavior, as when we say, "I found so-and-so rewarding"; or "I got a great deal out of him"; or, even, "Talking with him took a great deal out of me." But, perhaps just be-

¹See R. B. Braithwaite, *Scientific Explanation* (Cambridge: Cambridge University Press, 1953).

cause it is so obvious, this view has been much neglected by social scientists. So far as I know, the only theoretical work that makes explicit use of it is Marcel Mauss's *Essai sur le don*, published in 1925, which is as ancient as social science goes.² It may be that the tradition of neglect is now changing and that, for instance, the psychologists who interpret behavior in terms of transactions may be coming back to something of the sort I have in mind.³

An incidental advantage of an exchange theory is that it might bring sociology closer to economics—that science of man most advanced, most capable of application, and, intellectually, most isolated. Economics studies exchange carried out under special circumstances and with a most useful built-in numerical measure of value. What are the laws of the general phenomenon of which economic behavior is one class?

In what follows I shall suggest some reasons for the usefulness of a theory of social behavior as exchange and suggest the nature of the propositions such a theory might contain.

AN EXCHANGE PARADIGM

I start with the link to behavioral psychology and the kind of statement it makes about the behavior of an experimental animal such as the pigeon.⁴ As a pigeon explores its cage in the laboratory, it happens to peck a target, whereupon the psychologist feeds it corn. The evidence is that it will peck the target again; it has learned the behavior, or, as my friend Skinner says, the behavior has been reinforced, and the pigeon has undergone *operant conditioning*. This kind of psychologist is not interested in how the behavior was learned: "learning theory" is a poor name for his field. Instead,

² Translated by I. Cunnison as *The Gift* (Glencoe, Ill.: Free Press, 1954).

³ In social anthropology D. L. Oliver is working along these lines, and I owe much to him. See also T. M. Newcomb, "The Prediction of Interpersonal Attraction," *American Psychologist*, XI (1956), 575-86.

⁴ B. F. Skinner, *Science and Human Behavior* (New York: Macmillan Co., 1953).

he is interested in what determines changes in the rate of emission of learned behavior, whether pecks at a target or something else.

The more hungry the pigeon, the less corn or other food it has gotten in the recent past, the more often it will peck. By the same token, if the behavior is often reinforced, if the pigeon is given much corn every time it pecks, the rate of emission will fall off as the pigeon gets *satiated*. If, on the other hand, the behavior is not reinforced at all, then, too, its rate of emission will tend to fall off, though a long time may pass before it stops altogether, before it is *extinguished*. In the emission of many kinds of behavior the pigeon incurs *aversive stimulation*, or what I shall call "cost" for short, and this, too, will lead in time to a decrease in the emission rate. Fatigue is an example of a "cost." Extinction, satiation, and cost, by decreasing the rate of emission of a particular kind of behavior, render more probable the emission of some other kind of behavior, including doing nothing. I shall only add that even a hard-boiled psychologist puts "emotional" behavior, as well as such things as pecking, among the unconditioned responses that may be reinforced in operant conditioning. As a statement of the propositions of behavioral psychology, the foregoing is, of course, inadequate for any purpose except my present one.

We may look on the pigeon as engaged in an exchange—pecks for corn—with the psychologist, but let us not dwell upon that, for the behavior of the pigeon hardly determines the behavior of the psychologist at all. Let us turn to a situation where the exchange is real, that is, where the determination is mutual. Suppose we are dealing with two men. Each is emitting behavior reinforced to some degree by the behavior of the other. How it was in the past that each learned the behavior he emits and how he learned to find the other's behavior reinforcing we are not concerned with. It is enough that each does find the other's behavior reinforcing, and I shall call the reinforcers—the equivalent of the pigeon's corn—*values*, for this, I think, is what we

mean by this term. As he emits behavior, each man may incur costs, and each man has more than one course of behavior open to him.

This seems to me the paradigm of elementary social behavior, and the problem of the elementary sociologist is to state propositions relating the variations in the values and costs of each man to his frequency distribution of behavior among alternatives, where the values (in the mathematical sense) taken by these variable for one man determine in part their values for the other.⁵

I see no reason to believe that the propositions of behavioral psychology do not apply to this situation, though the complexity of their implications in the concrete case may be great indeed. In particular, we must suppose that, with men as with pigeons, an increase in extinction, satiation, or aversive stimulation of any one kind of behavior will increase the probability of emission of some other kind. The problem is not, as it is often stated, merely, what a man's values are, what he has learned in the past to find reinforcing, but how much of any one value his behavior is getting him now. The more he gets, the less valuable any further unit of that value is to him, and the less often he will emit behavior reinforced by it.

THE INFLUENCE PROCESS

We do not, I think, possess the kind of studies of two-person interaction that would either bear out these propositions or fail to do so. But we do have studies of larger numbers of persons that suggest that they may apply, notably the studies by Festinger, Schachter, Back, and their associates on the dynamics of influence. One of the variables they work with they call *cohesiveness*, defined as anything that attracts people to take part in a group. Cohesiveness is a value variable; it refers to the degree of reinforcement people find in the ac-

tivities of the group. Festinger and his colleagues consider two kinds of reinforcing activity: the symbolic behavior we call "social approval" (sentiment) and activity valuable in other ways, such as doing something interesting.

The other variable they work with they call *communication* and others call *interaction*. This is a frequency variable; it is a measure of the frequency of emission of valuable and costly verbal behavior. We must bear in mind that, in general, the one kind of variable is a function of the other.

Festinger and his co-workers show that the more cohesive a group is, that is, the more valuable the sentiment or activity the members exchange with one another, the greater the average frequency of interaction of the members.⁶ With men, as with pigeons, the greater the reinforcement, the more often is the reinforced behavior emitted. The more cohesive a group, too, the greater the change that members can produce in the behavior of other members in the direction of rendering these activities more valuable.⁷ That is, the more valuable the activities that members get, the more valuable those that they must give. For if a person is emitting behavior of a certain kind, and other people do not find it particularly rewarding, these others will suffer their own production of sentiment and activity, in time, to fall off. But perhaps the first person has found their sentiment and activity rewarding, and, if he is to keep on getting them, he must make his own behavior more valuable to the others. In short, the propositions of behavioral psychology imply a tendency toward a certain proportionality between the value to others of the behavior a man gives them

⁶ K. W. Back, "The Exertion of Influence through Social Communication," in L. Festinger, K. Back, S. Schachter, H. H. Kelley, and J. Thibaut (eds.), *Theory and Experiment in Social Communication* (Ann Arbor: Research Center for Dynamics, University of Michigan, 1950), pp. 21-36.

⁷ S. Schachter, N. Ellertson, D. McBride, and D. Gregory, "An Experimental Study of Cohesiveness and Productivity," *Human Relations*, IV (1951), 229-38.

⁵ *Ibid.*, pp. 297-329. The discussion of "double contingency" by T. Parsons and E. A. Shils could easily lead to a similar paradigm (see *Toward a General Theory of Action* [Cambridge, Mass.: Harvard University Press, 1951], pp. 14-16).

and the value to him of the behavior they give him.⁸

Schachter also studied the behavior of members of a group toward two kinds of other members, "conformers" and "deviates."⁹ I assume that conformers are people whose activity the other members find valuable. For conformity is behavior that coincides to a degree with some group standard or norm, and the only meaning I can assign to *norm* is "a verbal description of behavior that many members find it valuable for the actual behavior of themselves and others to conform to." By the same token, a deviate is a member whose behavior is not particularly valuable. Now Schachter shows that, as the members of a group come to see another member as a deviate, their interaction with him—communication addressed to getting him to change his behavior—goes up, the faster the more cohesive the group. The members need not talk to the other conformers so much; they are relatively satiated by the conformers' behavior: they have gotten what they want out of them. But if the deviate, by failing to change his behavior, fails to reinforce the members, they start to withhold social approval from him: the deviate gets low sociometric choice at the end of the experiment. And in the most cohesive groups—those Schachter calls "high cohesive-relevant"—interaction with the deviate also falls off in the end and is lowest among those members that rejected him most strongly, as if they had given him up as a bad job. But how plonking can we get? These findings are utterly in line with everyday experience.

PRACTICAL EQUILIBRIUM

At the beginning of this paper I suggested that one of the tasks of small-group research was to show the relation between the results of experimental work done under laboratory conditions and the results of field research on real-life small groups. Now the

latter often appear to be in practical equilibrium, and by this I mean nothing fancy. I do not mean that all real-life groups are in equilibrium. I certainly do not mean that all groups must tend to equilibrium. I do not mean that groups have built-in antidotes to change: there is no homeostasis here. I do not mean that we assume equilibrium. I mean only that we sometimes *observe* it, that for the time we are with a group—and it is often short—there is no great change in the values of the variables we choose to measure. If, for instance, person A is interacting with B more than with C both at the beginning and at the end of the study, then at least by this crude measure the group is in equilibrium.

Many of the Festinger-Schachter studies are experimental, and their propositions about the process of influence seem to me to imply the kind of proposition that empirically holds good of real-life groups in practical equilibrium. For instance, Festinger *et al.* find that, the more cohesive a group is, the greater the change that members can produce in the behavior of other members. If the influence is exerted in the direction of conformity to group norms, then, when the process of influence has accomplished all the change of which it is capable, the proposition should hold good that, the more cohesive a group is, the larger the number of members that conform to its norms. And it does hold good.¹⁰

Again, Schachter found, in the experiment I summarized above, that in the most cohesive groups and at the end, when the effort to influence the deviate had failed, members interacted little with the deviate and gave him little in the way of sociometric choice. Now two of the propositions that hold good most often of real-life groups in practical equilibrium are precisely that the more closely a member's activity conforms to the norms the more interaction he receives from other members and the more liking choices he gets from them too. From

⁸ Skinner, *op. cit.*, p. 100.

⁹ S. Schachter, "Deviation, Rejection, and Communication," *Journal of Abnormal and Social Psychology*, XLVI (1951), 190-207.

¹⁰ L. Festinger, S. Schachter, and K. Back, *Social Pressures in Informal Groups* (New York: Harper & Bros., 1950), pp. 72-100.

these main propositions a number of others may be derived that also hold good.¹¹

Yet we must ever remember that the truth of the proposition linking conformity to liking may on occasion be masked by the truth of other propositions. If, for instance, the man that conforms to the norms most closely also exerts some authority over the group, this may render liking for him somewhat less than it might otherwise have been.¹²

Be that as it may, I suggest that the laboratory experiments on influence imply propositions about the behavior of members of small groups, when the process of influence has worked itself out, that are identical with propositions that hold good of real-life groups in equilibrium. This is hardly surprising if all we mean by equilibrium is that all the change of which the system is, under present conditions, capable has been effected, so that no further change occurs. Nor would this be the first time that statics has turned out to be a special case of dynamics.

PROFIT AND SOCIAL CONTROL

Though I have treated equilibrium as an observed fact, it is a fact that cries for explanation. I shall not, as structural-functional sociologists do, use an assumed equilibrium as a means of explaining, or trying to explain, why the other features of a social system should be what they are. Rather, I shall take practical equilibrium as something that is itself to be explained by the other features of the system.

If every member of a group emits at the end of, and during, a period of time much the same kinds of behavior and in much the

same frequencies as he did at the beginning, the group is for that period in equilibrium. Let us then ask why any one member's behavior should persist. Suppose he is emitting behavior of value A_1 . Why does he not let his behavior get worse (less valuable or reinforcing to the others) until it stands at $A_1 - \Delta A$? True, the sentiments expressed by others toward him are apt to decline in value (become less reinforcing to him), so that what he gets from them may be $S_1 - \Delta S$. But it is conceivable that, since most activity carries cost, a decline in the value of what he emits will mean a reduction in cost to him that more than offsets his losses in sentiment. Where, then, does he stabilize his behavior? This is the problem of social control.¹³

Mankind has always assumed that a person stabilizes his behavior, at least in the short run, at the point where he is doing the best he can for himself under the circumstances, though his best may not be a "rational" best, and what he can do may not be at all easy to specify, except that he is not apt to think like one of the theoretical antagonists in the *Theory of Games*. Before a sociologist rejects this answer out of hand for its horrid profit-seeking implications, he will do well to ask himself if he can offer any other answer to the question posed. I think he will find that he cannot. Yet experiments designed to test the truth of the answer are extraordinarily rare.

I shall review one that seems to me to provide a little support for the theory, though it was not meant to do so. The experiment is reported by H. B. Gerard, a member of the Festinger-Schachter team, under the title "The Anchorage of Opinions in Face-to-Face Groups."¹⁴ The experimenter formed artificial groups whose members met to discuss a case in industrial relations and to express their opinions about its probable outcome. The groups were of two kinds: high-attraction groups, whose members were told that they would like one another very much, and low-attraction groups, whose

¹¹ For propositions holding good of groups in practical equilibrium see G. C. Homans, *The Human Group* (New York: Harcourt, Brace & Co., 1950), and H. W. Riecken and G. C. Homans, "Psychological Aspects of Social Structure," in G. Lindzey (ed.), *Handbook of Social Psychology* (Cambridge, Mass.: Addison-Wesley Publishing Co., 1954), II, 786-832.

¹² See Homans, *op. cit.*, pp. 244-48, and R. F. Bales, "The Equilibrium Problem in Small Groups," in A. P. Hare, E. F. Borgatta, and R. F. Bales (eds.), *Small Groups* (New York: A. A. Knopf, 1953), pp. 450-56.

¹³ Homans, *op. cit.*, pp. 281-301.

¹⁴ *Human Relations*, VII (1954), 313-25.

members were told that they would not find one another particularly likable.

At a later time the experimenter called the members in separately, asked them again to express their opinions on the outcome of the case, and counted the number that had changed their opinions to bring them into accord with those of other members of their groups. At the same time, a paid participant entered into a further discussion of the case with each member, always taking, on the probable outcome of the case, a position opposed to that taken by the bulk of the other members of the group to which the person belonged. The

TABLE 1

PERCENTAGE OF SUBJECTS CHANGING TOWARD
SOMEONE IN THE GROUP

	Agree- ment	Mild Disagree- ment	Strong Disagree- ment
High attraction....	0	12	44
Low attraction....	0	15	9

TABLE 2

PERCENTAGE OF SUBJECTS CHANGING TOWARD
THE PAID PARTICIPANT

	Agree- ment	Mild Disagree- ment	Strong Disagree- ment
High attraction....	7	13	25
Low attraction....	20	38	8

experimenter counted the number of persons shifting toward the opinion of the paid participant.

The experiment had many interesting results, from which I choose only those summed up in Tables 1 and 2. The three different agreement classes are made up of people who, at the original sessions, expressed different degrees of agreement with the opinions of other members of their groups. And the figure 44, for instance, means that, of all members of high-attraction groups whose initial opinions were strongly in disagreement with those of other members, 44 per cent shifted their opinion later toward that of others.

In these results the experimenter seems

to have been interested only in the differences in the sums of the rows, which show that there is more shifting toward the group, and less shifting toward the paid participant, in the high-attraction than in the low-attraction condition. This is in line with a proposition suggested earlier. If you think that the members of a group can give you much—in this case, liking—you are apt to give them much—in this case, a change to an opinion in accordance with their views—or you will not get the liking. And, by the same token, if the group can give you little of value, you will not be ready to give it much of value. Indeed, you may change your opinion so as to depart from agreement even further, to move, that is, toward the view held by the paid participant.

So far so good, but, when I first scanned these tables, I was less struck by the difference between them than by their similarity. The same classes of people in both tables showed much the same relative propensities to change their opinions, no matter whether the change was toward the group or toward the paid participant. We see, for instance, that those who change least are the high-attraction, agreement people and the low-attraction, strong-disagreement ones. And those who change most are the high-attraction, strong-disagreement people and the low-attraction, mild-disagreement ones.

How am I to interpret these particular results? Since the experimenter did not discuss them, I am free to offer my own explanation. The behavior emitted by the subjects is opinion and changes in opinion. For this behavior they have learned to expect two possible kinds of reinforcement. Agreement with the group gets the subject favorable sentiment (acceptance) from it, and the experiment was designed to give this reinforcement a higher value in the high-attraction condition than in the low-attraction one. The second kind of possible reinforcement is what I shall call the "maintenance of one's personal integrity," which a subject gets by sticking to his own opinion in the face of disagreement with the group. The experimenter does not mention this reward, but I cannot

make sense of the results without something much like it. In different degrees for different subjects, depending on their initial positions, these rewards are in competition with one another: they are alternatives. They are not absolutely scarce goods, but some persons cannot get both at once.

Since the rewards are alternatives, let me introduce a familiar assumption from economics—that the cost of a particular course of action is the equivalent of the foregone value of an alternative¹⁵—and then add the definition: Profit = Reward — Cost.

Now consider the persons in the corresponding cells of the two tables. The behavior of the high-attraction, agreement people gets them much in the way of acceptance by the group, and for it they must give up little in the way of personal integrity, for their views are from the start in accord with those of the group. Their profit is high, and they are not prone to change their behavior. The low-attraction, strong-disagreement people are getting much in integrity, and they are not giving up for it much in valuable acceptance, for they are members of low-attraction groups. Reward less cost is high for them, too, and they change little. The high-attraction, strong-disagreement people are getting much in the way of integrity, but their costs in doing so are high, too, for they are in high-attraction groups and thus foregoing much valuable acceptance by the group. Their profit is low, and they are very apt to change, either toward the group or toward the paid participant, from whom they think, perhaps, they will get some acceptance while maintaining some integrity. The low-attraction, mild-disagreement people do not get much in the way of integrity, for they are only in mild disagreement with the group, but neither are they giving up much in acceptance, for they are members of low-attraction groups. Their rewards are low; their costs are low too, and their profit—the difference between the two—is also low. In their low profit they resemble the high-attraction, strong-disagree-

ment people, and, like them, they are prone to change their opinions, in this case, more toward the paid participant. The subjects in the other two cells, who have medium profits, display medium propensities to change.

If we define profit as reward less cost, and if cost is value foregone, I suggest that we have here some evidence for the proposition that change in behavior is greatest when perceived profit is least. This constitutes no direct demonstration that change in behavior is least when profit is greatest, but if, whenever a man's behavior brought him a balance of reward and cost, he changed his behavior away from what got him, under the circumstances, the less profit, there might well come a time when his behavior would not change further. That is, his behavior would be stabilized, at least for the time being. And, so far as this were true for every member of a group, the group would have a social organization in equilibrium.

I do not say that a member would stabilize his behavior at the point of greatest conceivable profit to himself, because his profit is partly at the mercy of the behavior of others. It is a commonplace that the short-run pursuit of profit by several persons often lands them in positions where all are worse off than they might conceivably be. I do not say that the paths of behavioral change in which a member pursues his profit under the condition that others are pursuing theirs too are easy to describe or predict; and we can readily conceive that in jockeying for position they might never arrive at any equilibrium at all.

DISTRIBUTIVE JUSTICE

Yet practical equilibrium is often observed, and thus some further condition may make its attainment, under some circumstance, more probable than would the individual pursuit of profit left to itself. I can offer evidence for this further condition only in the behavior of subgroups and not in that of individuals. Suppose that there are two subgroups, working close together in a factory, the job of one being somewhat

¹⁵ G. J. Stigler, *The Theory of Price* (rev. ed.; New York: Macmillan Co., 1952), p. 99.

different from that of the other. And suppose that the members of the first complain and say: "We are getting the same pay as they are. We ought to get just a couple of dollars a week more to show that our work is more responsible." When you ask them what they mean by "more responsible," they say that, if they do their work wrong, more damage can result, and so they are under more pressure to take care.¹⁶ Something like this is a common feature of industrial behavior. It is at the heart of disputes not over absolute wages but over wage differentials—indeed, at the heart of disputes over rewards other than wages.

In what kind of proposition may we express observations like these? We may say that wages and responsibility give status in the group, in the sense that a man who takes high responsibility and gets high wages is admired, other things equal. Then, if the members of one group score higher on responsibility than do the members of another, there is a felt need on the part of the first to score higher on pay too. There is a pressure, which shows itself in complaints, to bring the *status factors*, as I have called them, into line with one another. If they are in line, a condition of *status congruence* is said to exist. In this condition the workers may find their jobs dull or irksome, but they will not complain about the relative position of groups.

But there may be a more illuminating way of looking at the matter. In my example I have considered only responsibility and pay, but these may be enough, for they represent the two kinds of thing that come into the problem. Pay is clearly a reward; responsibility may be looked on, less clearly, as a cost. It means constraint and worry—or peace of mind foregone. Then the proposition about status congruence becomes this: If the costs of the members of one group are higher than those of another, distributive justice requires that their rewards should be higher too. But the thing works both ways: If the rewards are higher, the costs should

be higher too. This last is the theory of *noblesse oblige*, which we all subscribe to, though we all laugh at it, perhaps because the *noblesse* often fails to *oblige*. To put the matter in terms of profit: though the rewards and costs of two persons or the members of two groups may be different, yet the profits of the two—the excess of reward over cost—should tend to equality. And more than "should." The less-advantaged group will at least try to attain greater equality, as, in the example I have used, the first group tried to increase its profit by increasing its pay.

I have talked of distributive justice. Clearly, this is not the only condition determining the actual distribution of rewards and costs. At the same time, never tell me that notions of justice are not a strong influence on behavior, though we sociologists often neglect them. Distributive justice may be one of the conditions of group equilibrium.

EXCHANGE AND SOCIAL STRUCTURE

I shall end by reviewing almost the only study I am aware of that begins to show in detail how a stable and differentiated social structure in a real-life group might arise out of a process of exchange between members. This is Peter Blau's description of the behavior of sixteen agents in a federal law-enforcement agency.¹⁷

The agents had the duty of investigating firms and preparing reports on the firms' compliance with the law. Since the reports might lead to legal action against the firms, the agents had to prepare them carefully, in the proper form, and take strict account of the many regulations that might apply. The agents were often in doubt what they should do, and then they were supposed to take the question to their supervisor. This they were reluctant to do, for they naturally believed that thus confessing to him their inability to solve a problem would reflect on their competence, affect the official ratings he

¹⁶ G. C. Homans, "Status among Clerical Workers," *Human Organization*, XII (1953), 5-10.

¹⁷ Peter M. Blau, *The Dynamics of Bureaucracy* (Chicago: University of Chicago Press, 1955), 99-116.

made of their work, and so hurt their chances for promotion. So agents often asked other agents for help and advice, and, though this was nominally forbidden, the supervisor usually let it pass.

Blau ascertained the ratings the supervisor made of the agents, and he also asked the agents to rate one another. The two opinions agreed closely. Fewer agents were regarded as highly competent than were regarded as of middle or low competence; competence, or the ability to solve technical problems, was a fairly scarce good. One or two of the more competent agents would not give help and advice when asked, and so received few interactions and little liking. A man that will not exchange, that will not give you what he has when you need it, will not get from you the only thing you are, in this case, able to give him in return, your regard.

But most of the more competent agents were willing to give help, and of them Blau says:

A consultation can be considered an exchange of values: both participants gain something, and both have to pay a price. The questioning agent is enabled to perform better than he could otherwise have done, without exposing his difficulties to his supervisor. By asking for advice, he implicitly pays his respect to the superior proficiency of his colleague. This acknowledgment of inferiority is the cost of receiving assistance. The consultant gains prestige, in return for which he is willing to devote some time to the consultation and permit it to disrupt his own work. The following remark of an agent illustrates this: "I like giving advice. It's flattering, I suppose, if you feel that others come to you for advice."¹⁸

Blau goes on to say: "All agents liked being consulted, but the value of any one of very many consultations became deflated for experts, and the price they paid in frequent interruptions became inflated."¹⁹ This implies that, the more prestige an agent received, the less was the increment of value of that prestige; the more advice an agent gave, the greater was the increment of cost

of that advice, the cost lying precisely in the foregone value of time to do his own work. Blau suggests that something of the same sort was true of an agent who went to a more competent colleague for advice: the more often he went, the more costly to him, in feelings of inferiority, became any further request. "The repeated admission of his inability to solve his own problems . . . undermined the self-confidence of the worker and his standing in the group."²⁰

The result was that the less competent agents went to the more competent ones for help less often than they might have done if the costs of repeated admissions of inferiority had been less high and that, while many agents sought out the few highly competent ones, no single agent sought out the latter much. Had they done so (to look at the exchange from the other side), the costs to the highly competent in interruptions to their own work would have become exorbitant. Yet the need of the less competent for help was still not fully satisfied. Under these circumstances they tended to turn for help to agents more nearly like themselves in competence. Though the help they got was not the most valuable, it was of a kind they could themselves return on occasion. With such agents they could exchange help and liking, without the exchange becoming on either side too great a confession of inferiority.

The highly competent agents tended to enter into exchanges, that is, to interact with many others. But, in the more equal exchanges I have just spoken of, less competent agents tended to pair off as partners. That is, they interacted with a smaller number of people, but interacted often with these few. I think I could show why pair relations in these more equal exchanges would be more economical for an agent than a wider distribution of favors. But perhaps I have gone far enough. The final pattern of this social structure was one in which a small number of highly competent agents exchanged advice for prestige with a large number of others less competent and in which the less

¹⁸ *Ibid.*, p. 108.

¹⁹ *Ibid.*, p. 108.

²⁰ *Ibid.*, p. 109.

competent agents exchanged, in pairs and in trios, both help and liking on more nearly equal terms.

Blau shows, then, that a social structure in equilibrium might be the result of a process of exchanging behavior rewarding and costly in different degrees, in which the increment of reward and cost varied with the frequency of the behavior, that is, with the frequency of interaction. Note that the behavior of the agents seems also to have satisfied my second condition of equilibrium: the more competent agents took more responsibility for the work, either their own or others', than did the less competent ones, but they also got more for it in the way of prestige. I suspect that the same kind of explanation could be given for the structure of many "informal" groups.

SUMMARY

The current job of theory in small-group research is to make the connection between experimental and real-life studies, to consolidate the propositions that empirically hold good in the two fields, and to show how these propositions might be derived from a still more general set. One way of doing this job would be to revive and make more rigorous the oldest of theories of social behavior—social behavior as exchange.

Some of the statements of such a theory might be the following. Social behavior is an exchange of goods, material goods but also non-material ones, such as the symbols of approval or prestige. Persons that give much

to others try to get much from them, and persons that get much from others are under pressure to give much to them. This process of influence tends to work out at equilibrium to a balance in the exchanges. For a person engaged in exchange, what he gives may be a cost to him, just as what he gets may be a reward, and his behavior changes less as profit, that is, reward less cost, tends to a maximum. Not only does he seek a maximum for himself, but he tries to see to it that no one in his group makes more profit than he does. The cost and the value of what he gives and of what he gets vary with the quantity of what he gives and gets. It is surprising how familiar these propositions are; it is surprising, too, how propositions about the dynamics of exchange can begin to generate the static thing we call "group structure" and, in so doing, generate also some of the propositions about group structure that students of real-life groups have stated.

In our unguarded moments we sociologists find words like "reward" and "cost" slipping into what we say. Human nature will break in upon even our most elaborate theories. But we seldom let it have its way with us and follow up systematically what these words imply.²¹ Of all our many "approaches" to social behavior, the one that sees it as an economy is the most neglected, and yet it is the one we use every moment of our lives—except when we write sociology.

HARVARD UNIVERSITY

²¹ *The White-Collar Job* (Ann Arbor: Survey Research Center, University of Michigan, 1953), pp. 115-27.

DURKHEIM'S *SUICIDE* AND PROBLEMS OF EMPIRICAL RESEARCH¹

HANAN C. SELVIN

ABSTRACT

The theoretical content of *Suicide* has been thoroughly discussed, but relatively little attention has been paid to the ways in which Durkheim tested and refined his theories with empirical data. Durkheim's empirical analyses are multivariate: each additional variable is systematically incorporated into the relationships that have previously been studied. The analyses are also replicative: the same relationship is studied in widely varying contexts. A careful study of the consequences of these procedures, both intended and unintended, yields valuable insights into current problems of theoretically oriented empirical research.

Sixty-one years after it first appeared in print, Émile Durkheim's *Suicide*² is still a model of sociological research. Few, if any, later works can match the clarity and power with which Durkheim marshaled his facts to test and refine his theory. The stature of this work is even more impressive when one remembers that Durkheim lacked even so rudimentary a tool as the correlation coefficient. Yet the methodology of *Suicide* is important to those now engaged in empirical research, not merely to historians of sociology. Durkheim recognized and solved many of the problems that beset present-day research. Others he formulated so lucidly—perhaps because he did not exile his methodology to appendixes—that their solution is relatively simple with the tools now available. To use a fashionable term, this paper will explore some of the methodological “continuities” that stem from *Suicide*, much as the essays based on *The American Soldier*³ and *The Authoritarian Personality*⁴

have done with these more recent classics.

“Methodology” has several meanings to sociologists. To some it means questionnaires, interviews, punched cards—the hand tools of research. To others, such as Durkheim himself and Parsons,⁵ it is the assumptions and concepts used in constructing a theory. Here it will be used to mean the systematic examination of the procedures, assumptions, and modes of explanation in the analysis of empirical data.⁶ This focus on Durkheim's methodology is not meant to minimize the importance of his theoretical insights; the value of methodological investigations, after all, is that they lead to more effective theorizing about social behavior. But Durkheim's theoretical development has been discussed by many authors,⁷ including others in this *Journal*, while his analytical procedures have not received the attention they deserve.

¹ Émile Durkheim, *The Rules of Sociological Method*, trans. Sarah A. Solvay and John H. Mueller (Glencoe, Ill.: Free Press, 1938); Talcott Parsons, *The Structure of Social Action* (Glencoe, Ill.: Free Press, 1949), pp. 20–27 and chap. ix.

² Paul F. Lazarsfeld and Morris Rosenberg (eds.), *The Language of Social Research* (Glencoe, Ill.: Free Press, 1955), p. 4.

³ Parsons, *op. cit.*; Harry Alpert, *Émile Durkheim and His Sociology* (New York: Columbia University Press, 1939), Parts II and III; Émile Benoit-Smullyan, “The Sociologism of Émile Durkheim and His School,” in Henry Elmer Barnes (ed.), *An Introduction to the History of Sociology* (Chicago: University of Chicago Press, 1948), chap. xxvii. His chapter contains a considerable bibliography.

¹ I am deeply indebted to Paul F. Lazarsfeld for acquainting me with the kind of analysis on which this paper is based.

² Émile Durkheim, *Suicide*, trans. John A. Spaulding and George Simpson (Glencoe, Ill.: Free Press, 1951).

³ Robert K. Merton and Paul F. Lazarsfeld (eds.), *Continuities in Social Research: Studies in the Scope and Method of “The American Soldier”* (Glencoe, Ill.: Free Press, 1950).

⁴ Richard Christie and Marie Jahoda (eds.), *Continuities in Social Research: Studies in the Scope and Method of “The Authoritarian Personality”* (Glencoe, Ill.: Free Press, 1954).

MULTIVARIATE ANALYSIS

Central to Durkheim's methodology is his use of what has been called *multivariate analysis*: "the study and interpretation of complex interrelationships among a multiplicity of characteristics."⁸ Much of the empirical analysis in *Suicide* can be viewed as the progressive introduction of additional variables. It will be useful to examine one of these analyses in detail, for it includes several of the procedures to be considered in this paper.

The first chapter on egoistic suicide (Book II, chap. ii) begins with the relation between religion and suicide rates for three groups of countries—the predominately Protestant, the mixed Protestant and Catholic, and the predominantly Catholic.⁹ But, as Durkheim points out, this comparison includes countries with radically different social conditions and requires consideration of the relation between religion and suicide *within* each country. Bavaria, the German state with the lowest proportion of Protestants, has the lowest proportion of suicides. And, in what may seem a mere piling-up of instances, the provinces within Bavaria also exhibit this same relationship: "Suicides are found in direct proportion to the number of Protestants and in inverse proportion to that of Catholics." Prussia and the Prussian provinces are the site of a similar analysis. Then the analysis is repeated for a third country: Switzerland. Here Durkheim takes advantage of the fact that both French- and German-speaking areas contain some cantons that are largely Catholic and others that are largely Protestant. This allows him to hold constant the effect of language as well as nationality ("race") while examining the effect of religion on suicide.

All the preceding analyses are based on data for nations or other large aggregations; thus the discussion of Bavaria cites the relatively high rate of suicides in provinces with high proportion of Protestants and the low rate in provinces with many Catholics. The

⁸ Lazarsfeld and Rosenberg, *op. cit.*, p. 11.

⁹ This and the following two paragraphs are taken from *Suicide*, pp. 152–56.

implications of this procedure will be considered later; here it is important to note only that Durkheim recognized the difference between relationships based on aggregate data and those based on individual data, for he goes on to say that "in a fairly large number of cases the number of suicides per million inhabitants of the population of each confession has been directly determined." And he presents data on the suicide rates by religion for twelve periods of time in five countries, as well as some fragmentary data for France.

After disposing of the "deviant case" of Norway and Sweden, Durkheim considers the low suicide rate among Jews. As compared with Protestants and Catholics, Jews are more likely to live in cities and to pursue intellectual occupations—both conditions that are associated with higher suicide rates. Therefore, Durkheim reasons, if the reported rate of suicide among Jews is lower, despite these conditions, the "true" Jewish rate must be even lower than the figures reveal it to be.

As this passage makes plain, multivariate analysis meant more to Durkheim than simply considering the separate relationships between suicide and the several independent variables—religion, nationality, and language. Each new variable is progressively incorporated into the preceding analyses, so that several variables are considered jointly. The methodology of multivariate analysis is most clearly seen in the case where a relationship between one independent variable (say, religion) and the dependent variable (suicide) is "elaborated" by the introduction of a third variable or "test factor" (say, nationality). Lazarsfeld, Kendall, and Hyman have defined three major types of elaboration: explanation, interpretation, and specification.¹⁰ Explanation is the attempt

¹⁰ Paul F. Lazarsfeld, "Interpretation of Statistical Relations as a Research Operation," in Lazarsfeld and Rosenberg, *op. cit.*, pp. 115–25; Patricia L. Kendall and Paul F. Lazarsfeld, "Problems of Survey Analysis," in Merton and Lazarsfeld, *op. cit.*, pp. 133–96, esp. pp. 135–67; Herbert H. Hyman, *Survey Design and Analysis* (Glencoe, Ill.: Free Press, 1955), chaps. vi and vii.

to "explain away" the apparent meaning of an observed relationship. For example, the association between religion and suicide might have been a manifestation of nationality, since countries like Germany have both a high suicide rate and many Protestants. Looking into this possibility, Durkheim finds that the original association between religion and suicide persists when national differences are taken into account; nationality is therefore not an explanation of this relationship.

Once convinced that nationality and language do not explain away the association between religion and suicide, Durkheim turns to the interpretation of this relationship: what is the chain of variables connecting two such disparate phenomena as Protestantism and a high suicide rate? A spirit of free inquiry, according to Durkheim, is the most important link in this chain: Protestantism fosters free inquiry and free inquiry in turn leads to a higher rate of suicide.

Although Durkheim lacked the statistical techniques to develop these ideas rigorously, he saw their central place in theoretically oriented research. The relationship between two variables "... may not be due to the fact that one phenomenon is the cause of the other but to the fact that they are both the effects of the same cause, or, again, that there exists between them a third phenomenon, interposed but unperceived, which is the effect of the first and the cause of the second."¹¹

Specification, the third mode of elaboration, identifies the conditions under which a relationship holds true in greater or less degree. For example, the effect of religion on suicide is less in the German cantons of Switzerland than in the French. Since specification appears in many forms in *Suicide* and since its role in the development of sociological theory differs from the other modes of elaboration, it will be considered at some length.

Specification leads to the development of multivariate theories of behavior in a way that is not true of explanation and interpre-

tation. The aim of specification is to construct three-variable relationships—to say that, as in the example just cited, the effect of religion on suicide is greater in one place than in another. Note that this statement cannot be decomposed into a set of two-variable relationships. Explanation, on the other hand, involves a three-variable association only as an intermediate step, either toward rejecting the apparent finding or toward affirming its provisional meaning; in either case, the result is not a three-variable relationship. Interpretation, likewise, uses the three-variable association only to produce a series of two-variable relationships, to show that these relationships are linked by the variables they have in common.

This greater complexity of specification, its essential three-variable nature, leads to more complex problems in analysis. Durkheim's successes and failures in coping with some of these problems are instructive. Three problems will be considered: (1) the joint effects of group and individual characteristics on individual behavior; (2) the theoretical problems stemming from the statistical concept of "interaction"; and (3) the question of when to stop an empirical analysis.

The variety of analyses that come under the heading of specification is suggested by Hyman's classification; among other ways, one can specify a relationship according to the interest and concern of respondents, the time and place at which it occurs, or the conditions and contingencies on which it depends.¹² Durkheim's analysis provides still another type based on the "units of analysis." This type of specification of which there are several varieties, has been called "contextual analysis":¹³ it involves the joint effects of an individual characteristic and a group characteristic on rates of individual

¹² Hyman, *op. cit.*, pp. 295–311.

¹³ Hanan C. Selvin and Warren O. Hagstrom, "Contextual Analysis: The Joint Effects of Group and Individual Characteristics on Behavior" (in preparation). I am indebted to Robert K. Merton for first calling my attention to the necessity of specifying the social context of a relationship between individual characteristics.

¹¹ *Rules*, p. 131.

behavior. In discussing the lower suicide rate among married people, Durkheim points out that in France the difference between the married and the single (his "coefficient of preservation") is greater among the men, while in the Grand-Duchy of Oldenburg it is greater among the women.¹⁴ That is, the social and cultural differences between France and Oldenburg are manifested in two essentially different ways: (1) They exert a *direct* effect; the over-all suicide rate is noticeably higher in France than in Oldenburg. (2) They exert an *indirect* effect; the *relationship* between sex and suicide is different in France and in Oldenburg. In other words, national characteristics have a differential impact on the sex-suicide association in the two countries, the difference between the sexes being greater in France than in Oldenburg.

Methodological devices like contextual analysis are more than ingenious ways to manipulate data. As Merton has emphasized, they are important in opening new directions for theory.¹⁵ Durkheim's contextual analysis raises questions about the ways in which group and individual characteristics interact to affect behavior. For example, under what conditions do national characteristics produce such a marked reversal in the association between individual attributes and behavior?

The negative side of this case can also be found in *Suicide*: where Durkheim lacked adequate statistical techniques, he was occasionally led into theoretical contradictions. At one point he asserts that "the relation between the aptitude for suicide of married persons and that of widowers and widows is identically the same in widely different social groups, from the simple fact that the moral condition of widowhood everywhere bears the same relation to the moral constitution characteristic of marriage."¹⁶ But Durkheim's data on Oldenburg and France lead to the opposite conclusion—that the re-

lation between the suicide rates of married persons and widows and widowers was *not* the same in the two countries. What Durkheim lacked and what has since become available is a precise conception of statistical interaction, the ways in which the association between two variables depends on the values of a third variable.¹⁷

Durkheim's treatment of statistical interaction and of the theoretical relationships that it measures is notably inconsistent. Sometimes, as here, he ignores the presence of interaction in his data. Elsewhere, he correctly notes its presence, remarking, for example, that seasonal differences in suicide are less pronounced in cities than in rural areas.¹⁸ And in another place he assumes, without any evidence for or against his assumption, that the interaction of temperature and location is zero: "... if the temperature had the supposed influence, it should be felt equally in the geographical distribution of suicides."¹⁹

One possible reason for Durkheim's inconsistency is that he had not formalized his analytical procedures. In effect, each time he came to a case of specification, it had to be reasoned through from the beginning. Formalizations such as the Lazarsfeld-Kendall-Hyman types of elaboration enable the analyst to recognize the same principle at work in different instances and therefore to treat them similarly. Such formalizations also reveal a link between apparently disparate phenomena; thus the formal model of elaboration also illuminates the seemingly unrelated problem of when to stop an analysis.

Durkheim's treatment of "race" and suicide provides a case in point.²⁰ Arguing that the high rate of suicide in Germany "might be due to the special nature of German

¹⁷ The phenomenon of statistical interaction has been given many different names (e.g., specification, conditional relationship, differential impact, differential sensitivity, and non-additivity of effects).

¹⁸ *Suicide*, p. 120.

¹⁹ *Ibid.*, p. 113.

²⁰ *Ibid.*, pp. 86-87.

¹⁴ *Suicide*, pp. 177-80.

¹⁵ Robert K. Merton, *Social Theory and Social Structure* (Glencoe, Ill.: Free Press, 1949), chap. iii.

¹⁶ *Suicide*, p. 307.

civilization," he decides to "see whether the German retains this sad primacy outside of Germany."²¹ To this end he examines the suicide rates in the provinces of Austria-Hungary, in which German-speaking people range from 1.9 to 100 per cent, and finds "not the least trace of German influence" on the suicide rate. However, a close examination of Durkheim's data, particularly of the five provinces that have high proportions of Germans and disproportionately few suicides, leads to quite different conclusions. These provinces—Upper Austria, Salzburg, Transalpine Tyrol, Carinthia, and Styria—comprise the western part of present-day Austria. If these five contiguous provinces are removed, the Spearman rank correlation for the remaining ten provinces is 0.95, indicating an almost perfect relationship between the suicide rate and the proportion of German-speaking people.

The important point here is not substantive but methodological. Durkheim stopped his analysis as soon as he found a "zero" relationship. This procedure is perhaps more common in research today. Small associations are considered a signal to turn to other matters, especially when the associations are not statistically significant. The reasoning behind this assumption is never made explicit, but it would seem to be that, if two variables are not associated when other items are left free to vary, they will not be associated when these other items are "held constant." That is, if the total association between two variables is zero, the partial associations will be zero. Sometimes this is true; often it is not. Hyman's passage on the "elaboration of a zero relationship"²² in-

dicates that this may happen when the two partial relations are approximately equal in size and opposite in sign. For example, a surprisingly small association between job satisfaction and participation in community organizations resulted from a positive association between participation and satisfaction among members of the working class and a negative association of approximately the same size in the white-collar class.

To my knowledge, Hyman's is the only published discussion of this problem. It may be useful, therefore, to make two further points suggested by Hyman's brief treatment. First, he implies that this kind of relationship is uncommon and even accidental. Actually, it may occur frequently under certain conditions—for example, in the kind of contextual analysis discussed above, where people are assigned to groups instead of being born into them or choosing them themselves. A study of leisure-time behavior in army training companies found that many small or zero associations between behavior and an individual characteristic, such as marital status, resulted from opposite and approximately equal associations in companies with different "leadership climates."²³

Second, still another type of elaboration of a zero relationship may be ranged alongside the two identified by Hyman. A zero association between two variables may occur even when both partial associations are in the same direction. The hypothetical example in Table 1 shows that this case would interest the student of political behavior. At both levels of education, the people with more information tend to choose the Democratic party, yet the "collapsed" table of information and party affiliation without regard to education will show that, among both the more-informed and the less-informed, 50 per cent are Democrats. The two partial associations are positive (and about

²¹ By today's standards Durkheim's table shows a moderately high degree of association. The Spearman rank correlation is 0.57. In general, Durkheim regarded anything much less than perfect rank correlation as "independent." The reason why he could demand and find such high levels of association, while survey researchers are content with much less impressive relationships, has to do with the differences in the numbers of cases on which the associations are based. See the discussion of "grouping" in G. Udny Yule and M. G. Kendall, *An Introduction to the Theory of Statistics* (14th ed.; New York: Hafner, 1950), pp. 313-14.

²² *Op. cit.*, pp. 307-10.

²³ Hanan C. Selvin, *The Effects of Leadership Climate on the Nonduty Behavior of Army Trainees* ("University Microfilms Publications," No. 19.256 [microfilmed Ph.D. dissertation, Columbia University, 1956]), Appendix F.

the same size); the total association is zero. Unrealistic as this example may be (although it could describe a university town with a Democratic newspaper), it does demonstrate the importance of looking into those zero associations that theory or previous research suggests should not have been zero. A zero association between two variables may therefore result from any one of three different conditions in the partial relationships: zero associations in both partials, equal and opposite associations, or associations in the same direction. Only the first of these is a signal to stop the analysis.

THE FUNCTIONS OF REPLICATION IN
EMPIRICAL RESEARCH

Another multivariate procedure that is conspicuous in *Suicide* and that deserves

Durkheim's lavish use of replications is all the more noteworthy in contrast with modern survey research, where a relationship is often demonstrated only in a single table. One reason why Durkheim used so many replications is undoubtedly that his data came from official records; it cost him little more to study suicide in six countries than in one. On the other hand, in contemporary surveys the researcher must gather his own data, often at great expense, so that one community is a practical limit. However, as will be seen, this explanation rests on too narrow a conception of replications; a careful study of Durkheim's procedures will show that there are abundant opportunities for replication in every survey. A second reason for demonstrating a hypothesis in only one table is the belief that a close ad-

TABLE 1
INFORMATION AND PARTY AFFILIATION
EDUCATION HELD CONSTANT
(Hypothetical Data)

	LESS EDUCATED		MORE EDUCATED	
	Much Information	Little Information	Much Information	Little Information
Per cent Democratic.....	38	24	74	63
N.....	(200)	(100)	(100)	(200)

intensive inquiry is *replication*, the systematic restudy of a given relationship in different contexts.

Again using the first chapter on egoistic suicide as an illustration, we find that the original table relating religion and suicide is followed by no less than the *seventeen* replications in three pages. Why so many replications? Durkheim's answer is not altogether clear. He does demand that the facts cited to support a thesis be "numerous enough not to be attributable to accidental circumstances—not to permit another explanation—to be contradicted by no other fact."²⁴ But these principles are not systematically explained. This section will therefore examine some of the uses of replications in *Suicide* and will consider what functions replications may serve in current research.

²⁴ *Suicide*, p. 95.

herence to modern techniques of statistical inference—tests of significance, confidence intervals, and the like—guarantees the statistical soundness of the conclusions. This point of view has recently been vigorously attacked and staunchly defended.²⁵ Instead of repeating these arguments here, it will be shown that properly conducted replications may achieve the same ends more effectively. First, however, it is necessary to clarify the different kinds of replications.

"Unit replications" are the most frequent in *Suicide*; in these the original finding is

²⁵ Hanan C. Selvin, "A Critique of Tests of Significance in Survey Research," *American Sociological Review*, XXII (1957), 519-27; David Gold, "Comment" and Hanan C. Selvin, "Reply," *American Sociological Review*, XXIII (1958), 85-86; James Beshers, "Comment" and Hanan C. Selvin, "Reply," *American Sociological Review*, Vol. XXIII (April, 1958).

re-examined for different groups of subjects—for example, the excess of military over civilian suicides is confirmed for eight different countries of Europe.²⁶ Durkheim further replicates this finding within the Austro-Hungarian Empire; the military-civilian difference persists in the various military areas.²⁷ These two examples suggest that unit replications may be divided into two subtypes: "external replications" and "internal replications."

In external replications the conclusions of one study are tested independently in another study, usually conducted by a different investigator.²⁸ Cross-cultural comparisons are a familiar example. Demographers and others who work with official records also conduct external replications as a matter of course. But this procedure is difficult and expensive for anyone who must gather his own data.

Just the opposite is true of internal replications, in which a finding is restudied for smaller groups within the original set of subjects. In the passage on military and civilian suicides there are several external replications, based on the different countries, and several internal replications for areas within one country.²⁹ Although these replications have a geographical basis, a finding may be replicated in groups formed on any set of variables. In the chapter on anomic suicide (Book II, chapter v), Durkheim presents a table of suicide rates by occupation for eight countries. Here the various countries act as external replications for his original finding on occupations and suicide. But the same table could also serve as an internal replication of the national differences considered in the earlier chapter on egoistic suicide, the comparisons between

over-all national suicide rates being replicated for the comparable occupational groups within the countries.³⁰ Of course, all replications are not equally desirable; some criteria for choosing replications will be taken up after considering the uses of replications.

Replications, properly conducted, may serve at least two functions. They may provide a more valid "test of significance" where the usual tests cannot be legitimately applied, and they may lessen the seriousness of the "ecological fallacy," in which relationships between characteristics of individuals are wrongly inferred from data about groups.

REPLICATIONS AND STATISTICAL SIGNIFICANCE

Some reasons why tests of statistical significance are inapplicable in survey research have been noted in another paper.³¹ Perhaps the most important reason—and the one that bears on the use of replications—is that the tests assume an experimental situation, in which differences between subjects can be randomized. Agronomists, for example, can convert constant differences in the fertility of their experimental plots into chance differences by randomizing the assignment of their plants; in effect, they toss a coin to determine whether a plant goes into the experimental plot or the control plot. But sociologists can seldom randomize. If Catholics are more numerous in Italy and Protestants in Germany, the sociologist must cope with these stubborn facts. He cannot randomly assign Protestants and Catholics to the two countries.

Randomization turns such systematic differences, or "correlated biases," into random differences. Tests of significance can then be used to measure the probability that the observed difference between experimental and control groups *could* have been produced by the accidents of randomization. Without randomization, these correlated

²⁶ *Suicide*, p. 228.

²⁷ *Ibid.*, p. 235.

²⁸ For a collection of external replications and an interesting treatment of the factors making for successful replications see Robert C. Hanson, "Evidence and Procedures Characteristics of 'Reliable' Propositions in Social Science," *American Journal of Sociology*, LXIII (1958), 357-70.

²⁹ *Suicide*, pp. 228-39.

³⁰ *Ibid.*, p. 258.

³¹ Selvin, "A Critique of Significance Tests."

biases remain as possible sources of the observed difference between the groups, along with the experimental variable whose influence is being measured. Thus the interpretation of the difference in suicide rates between Italy and Germany as the result of religious differences must take into account the many other differences between the two countries—education, cultural norms, and so on, which may account, in part at least, for the relationship between religion and suicide. Some of these differences could have been removed by cross-tabulation, had Durkheim had the necessary data. Others, not necessarily the least important, could not. Consequently, it appears misleading to ask for the probability that the observed relationship may be “due to chance” without first being reasonably certain that correlated biases did not produce it.

Replications, properly conducted, can take care of correlated biases and random errors at the same time. When Durkheim replicates within Germany the relationship previously studied among the various countries of Europe, he is removing the host of biases correlated with nationality. And when he further replicates within Bavaria and within Prussia, constant differences between these states no longer are mixed in with the religion-suicide relationship. Finally, his replications based on individual data remove some of the provincial differences. At each level of replication, additional correlated biases are controlled: the original relationship between religion and suicide gains more and more support.

Replications can thus lessen the likelihood that correlated biases may have produced the observed result. It is necessary only that the variable on which the replications are based be theoretically relevant (i.e., that there is reason to believe it is causally related to both the independent and the dependent variable). Now what of the random factors that may have produced the observed result? Here, too, replications are useful: they lead toward a valid test of significance which is simple to understand and easy to compute.

Consider once again the seventeen replications of the relationship between religion and suicide. How likely is it that this relationship resulted from some combination of random errors? That is, how often would one expect to find seventeen consecutive replications all showing Protestants more likely to commit suicide if, in fact, Protestants and Catholics are equally prone to suicide? Assuming that the replications are independent, that the outcome of one replication does not affect the outcome of another, the probability of such a series of replications is $(\frac{1}{2})^{17}$, a result significant at the 0.00001 level.³²

Some cautions are in order. First, the validity of this computation, as of the ordinary test of significance, rests on the elimination of correlated biases. Since one can almost always conceive of relevant variables that have not been controlled and since there is no randomization to turn these correlated biases into random events, this condition is never fully satisfied. However, by applying this test only within series of replications, some, at least, of the correlated biases are controlled. The more variables that are controlled in any one replication, the more convincing this test of significance becomes.

Because he is dealing with official statistics that seldom are classifiable according to more than three variables, Durkheim's control over the correlated biases is relatively loose. Survey analysts, on the other hand, can more easily control these variables. For example, to test the hypothesis that socioeconomic status affects voting behavior, one would compare subgroups of high and low status that are similar on as many relevant variables as can be manipulated simultaneously. Such a test might compare two groups of young, single, urban,

³² Samuel A. Stouffer, “Quantitative Methods,” in Joseph B. Gittler (ed.), *Review of Sociology* (New York: John Wiley & Sons, 1957), pp. 45–46. Stouffer's use of replications in *The American Soldier* (Princeton: Princeton University Press, 1949), I, 92–95, is a notable example (he refers to replications there as the “method of matched comparisons”). See also Selvin, *The Effects of Leadership Climate*, pp. 43–47, 183–204.

Protestant, male high-school graduates, one group of high socioeconomic status, the other of low status. This comparison would be repeated for all possible combinations of these six variables; if enough of these comparisons are in the same direction as the original relationship, one can be reasonably confident that the original relationship is not the result of random errors, assuming, of course, that there are no other important correlated biases.

In controlling for correlated biases, the subgroups should be formed on relevant variables; in testing for statistical significance, the subgroups should be independent. Consequently, if both criteria are satisfied, replications will tend to accomplish both tasks simultaneously. Relevance depends on the implicit or explicit theory that is being used, and no rules can guarantee it. On the other hand, independence can in general be insured by seeing that no individual appears in more than one subgroup in a given replication. For example, it would not be efficient to replicate Durkheim's religion-suicide relationship by studying it first among married and single men and then among older and younger men in the same country, for some man will appear in both sets of comparisons.³³

One empirical procedure used by Durkheim appears at first glance to be another form of replication. In demonstrating that the relationship previously found between time of day and the suicide rate really depends on the "intensity of social life," he musters a variety of indicators of social activity—accidents, rail travel, and express receipts.³⁴ Social activity and suicide turn out to be highly associated, thus supporting his interpretation of the time-of-day relationship. Although this seems to be another type of replication, serving the same ends as those discussed above, this kind of item replication lacks one essential element of the

unit replications: independence of observations. In fact, one does not even seek independence in item replications. These replications help to demonstrate that an indicator is *valid*—in other words, that it means what the analyst says it means. The greater the variety of indicators of social activity that Durkheim can relate to suicide, the greater his assurance that social activity—and not some accidental correlate of it—is what accounts for the variations in suicide.³⁵

Replications thus serve Durkheim as a means of testing and refining his empirical hypotheses. The preceding discussion has suggested that they may serve these functions even better in survey research, where large numbers of variables can be easily manipulated. Another function of replications, of less general application but often of crucial importance in studies based on "ecological" data, will be examined in the following section.

REPLICATION AND THE "ECOLOGICAL FALLACY"

Robinson³⁶ first called the attention of sociologists to the fallacy of assuming that associations computed from group means or group proportions are valid estimates of the associations that would be obtained from individual data.³⁷ Most of Durkheim's ta-

³³ Item replication is, in a sense, the inverse of scaling. In item replication, one begins with a concept and seeks a variety of indicators to clarify its meaning. In scaling, one begins with a set of items and asks whether there is a single underlying concept that accounts for them.

³⁶ William S. Robinson, "Ecological Correlations and the Behavior of Individuals," *American Sociological Review*, XV (1950), 351-57. This problem seems to have been discovered and treated independently in psychology, statistics, and economics, as well as in sociology, although not so fully as in Robinson's paper (see Edward L. Thorndike, "On the Fallacy of Imputing the Correlations Found for Groups to the Individuals or Smaller Groups Composing them," *American Journal of Psychology*, LII [1929], 122-24; Yule and Kendall, *op. cit.*, pp. 310-15; Kenneth H. Arrow, "Mathematical Models in the Social Sciences," in Daniel Lerner and Harold D. Lasswell [eds.], *The Policy Sciences* [Stanford: Stanford University Press, 1951], p. 134).

³⁷ Robinson's paper concentrates on linear regres-

³³ Note that two or more time replications in the same table of *Suicide* are always separated by a period of years, thus increasing the independence of the replications.

³⁴ *Suicide*, pp. 118-20.

bles are based on such fallacious reasoning. Thus he reports that the rate of suicide in departments of France varies according to the proportion of "persons of independent means."³⁸ This result is consistent with either of the following hypotheses: *none* of the people who commit suicide has independent means, or *all* of them have independent means. In this table the ecological association between characteristics of departments reveals nothing about the individual association between a person's wealth and whether or not he commits suicide.

Every case of ecological associations does not entail the ecological fallacy. Menzel has shown that ecological associations are not only permissible but necessary when the "unit of analysis" is a group rather than the individuals in it.³⁹ However, Durkheim never theorizes about wealthy and poor departments, only about wealthy and poor individuals. And if he were interested in group characteristics—at the level, say, of departments or provinces—why would he replicate for successively finer subdivisions within these groups, in two cases carrying the replications down to individual data?⁴⁰ It is clear that Durkheim was guilty of the ecological fallacy.

To say that Durkheim's procedures were fallacious does not detract from his conclusions.⁴¹ They may be true, even if they do not necessarily follow from his data. In effect, Durkheim recognized this problem and attempted to solve it in the only way open to him—the systematic use of replications for units smaller than those in his original relationship. When he was able to carry the

sion and correlation, but the same problem arises, whatever the measure of association.

³⁸ *Suicide*, p. 245.

³⁹ Herbert Menzel, "Comment on Robinson's 'Ecological Correlations and the Behavior of Individuals,'" *American Sociological Review*, XV (1950), 674; see also the discussion of the "modifiable unit" in Yule and Kendall, *op. cit.*

⁴⁰ *Suicide*, pp. 154, 175.

⁴¹ Andrew F. Henry and James F. Short, Jr., have made the most recent attempt to formulate a comprehensive theory of suicide in their *Suicide and Homicide* (Glencoe, Ill.: Free Press, 1954).

replications down to individual data, there was, of course, no ecological fallacy: the hypothesis stated for group data is confirmed for individual data. However, even when Durkheim did not have individual data, the procedure of replicating in smaller units may lead, as Duncan and Davis have shown,⁴² to useful estimates of the individual association.

Durkheim's data cannot be used to illustrate this procedure, for a reason to be stated shortly. Consider, instead, the following ecological data from a hypothetical city in which there are 20 election districts of 100 voters. Each of the first 10 districts has 10 per cent Republicans and 20 per cent voting for Eisenhower. (These are group attributes; nothing is said about the number of individuals who are both Republicans and for Eisenhower.) The other 10 districts are 80 per cent Republican and 90 per cent for Eisenhower. However one chooses to measure association—by product-moment correlation, percentage differences in a fourfold table, or any other index—this is a perfect ecological association: all the districts with many Republicans are for Eisenhower, as against none of the districts with few Republicans. This is formally the kind of relationship studied by Durkheim.

Now consider any one of the first 10 districts. The number of Republicans for Eisenhower is not known, but it is easy to see that it must be between a maximum of 10 and a minimum of zero. The corresponding values for the other 10 districts are 80 and 70. If each district had the maximum possible number of Republicans for Eisenhower, the total for the city would be $(10 \times 10) + (10 \times 80) = 900$; the minimum total for the city would be $(10 \times 0) + (10 \times 70) = 700$. Table 2 shows the maximum and minimum values for the four cells of the individual association. If the association between Republicanism and preference for Eisenhower is a maximum in each district, then, for the city as a whole, 100 per cent of the Republicans and 18.2 per cent of the

⁴² Otis Dudley Duncan and Beverly Davis, "An Alternative to Ecological Correlation," *American Sociological Review*, XVIII (1953), 665-66.

Democrats prefer Eisenhower, a difference of 81.8 per cent. This is something less than the perfect ecological association; there, every Republican district was for Eisenhower and every Democratic district for Stevenson. Similarly, if the within-district associations are as small as possible, then 77.8 per cent of the Republicans and 36.4 per cent of the Democrats are for Eisenhower, a difference of 41.4 per cent.

The ecological data for the districts thus lead to bounds for the individual association in the city as a whole. Durkheim's procedure of replicating in successively smaller units would likewise seem to generate bounds for the individual associations on which his analysis is really based. In fact, Goodman

D, as shown. But if the proportion for Eisenhower is replaced by the suicide rate and the proportion Republican by any one of Durkheim's independent variables (say, the proportion of Protestants), then all Durkheim's data are located in region *B*. (This is the region in which the suicide rate is less than the proportion of Protestants and also less than the proportion of non-Protestants.) No amount of replication, therefore, will lead to bounds for the individual association between suicide and Protestantism or between suicide and any of Durkheim's independent variables. Suicide is too rare an event, compared with the rates of the other variables he used. Ecological reasoning in *Suicide* is not limited to geographical data.

TABLE 2
PARTY AFFILIATION AND VOTE
(Hypothetical Data)

INDIVIDUAL CANDIDATE CHOICE		INDIVIDUAL PARTY AFFILIATION		TOTAL
		Republican	Democratic	
Eisenhower	{Max.	900	200	1,100
	{Min.	700	400	
Stevenson	{Max.	0	900	900
	{Min.	200	700	

has shown that the smallest units give the closest possible bounds; nothing would be gained in the example above by aggregating the districts into larger areas.⁴³ Durkheim's reason for replicating at several levels instead of going directly to the smallest units is probably that his data were uneven (i.e., that he did not have comparable figures for the provinces in all his countries).

The only difficulty in applying this procedure to Durkheim's data is that it will not work. Goodman's analysis indicates that bounds can be inferred only when the points in the ecological scatter plot are located in at least two of the four regions in Figure 1. The two sets of points for the hypothetical election data are located in regions *A* and

"A proof of the slight effect of marriage [on suicide] is the fact that the marriage rate has changed very little since the first of the [nineteenth] century, while suicide has tripled."⁴⁴ Here, again, the real question is whether married people are more or less likely than single people to commit suicide. With these data, however, a positive, negative, or zero individual association is possible. Data aggregated over time can lead to false interpretations, just as can data aggregated over geographical units. In both cases the difficulty is removed by studying the data within units rather than "ecologically." There is no ecological fallacy when the independent variable and suicide are examined for individuals within each geographical area or for individuals within each time period. In other words, both versions of the ecological fallacy can be considered as cases of "spurious association." This, it will be recalled, is an erroneous inference that is "explained away" by holding con-

⁴³ Leo A. Goodman, "Some Alternatives to Ecological Correlation" (Chicago: Statistical Research Center, University of Chicago, 1957) (dittoed). This comprehensive paper also includes another method for estimating the "individual regression" from ecological data under certain assumptions. Unfortunately, these assumptions are not met in Durkheim's data.

⁴⁴ *Suicide*, p. 185.

stant a variable that accounts for both the items being studied. The associations between region and religion and between region and suicide may account for the ecological association between religion and suicide; holding region constant removes any possibility of a false inference from this association. Similarly, the association between suicide and the marriage rate may lead to false inferences unless it is examined within relatively short time periods.⁴⁵ There is thus a close methodological connection between problems of ecological association and procedures of multivariate analysis.

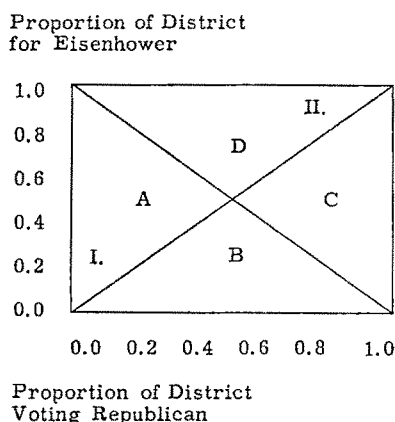


FIG. 1.—Conditions under which ecological associations yield bounds for individual associations.

This section has considered the uses of replications in testing sociological hypotheses and in making valid inferences from characteristics of groups to characteristics of their members. In both cases attention has been focused on the relationship of groups and subgroups; the description of the group, as given by Durkheim, has been taken for granted. But here, too, there are useful lessons to be learned from a careful examination of Durkheim's procedures.

A PROBLEM IN DESCRIBING GROUPS

The first section of the chapter on anomic suicide begins: "It is a well-known fact that economic crises have an aggravating effect

on the suicidal tendency."⁴⁶ Durkheim then establishes that poverty is not the link between economic crises and suicide. He argues that poverty "tends rather to produce the opposite effect. There is very little suicide in Ireland, where the peasantry leads so wretched a life. Poverty-stricken Calabria has almost no suicide; Spain has a tenth as many as France."⁴⁷ Here is an association between groups (countries) and individual behavior, in which the relationship is attributed to one property of the groups—their poverty. But Calabria, Ireland, and Spain are not only poorer than France; they are also more Catholic than France and, as Durkheim was at pains to show in his analysis of egoistic suicide, less educated. There is no necessary reason why poverty should be singled out as the cause of the lower suicide rate in these countries. Religious or educational differences would have accounted equally well for the variations in suicide.

Essentially this same problem appears in different guise when one tries to infer the intensity of some form of behavior from data about its incidence. For example, Durkheim assumes that districts with high divorce rates have many families that are near divorce.⁴⁸ This statement may be true; it is not necessarily true. Two countries with the same average intensity of marital unhappiness may, because of their legal codes, have very different rates of divorce. To move legitimately from incidence to intensity in this way, one must take into account the other group characteristics that affect the dependent variable.

The source of these difficulties is clear. It is the oversimple description of a group according to a single variable. What can be done to avoid such problems? The obvious answer is to hold the other group characteristics constant by cross-tabulation, just as one does with individual characteristics. To study the influence of poverty on suicide, Durkheim would have had to find areas that were alike on other variables, such as

⁴⁶ *Suicide*, p. 241.

⁴⁷ *Ibid.*, p. 245; italics added.

⁴⁸ *Ibid.*, p. 263.

⁴⁵ This is explanation in which the "ordering principle" is not "time" but "level of aggregation" or "level of complexity" (cf. Kendall and Lazarsfeld, *op. cit.*, p. 196).

religion, urbanization, and education, and different only in relative wealth. As a practical matter, this is impossible. Murdock's cross-cultural comparisons are based on as many as 250 different cultures, but even this number is too small to allow more than one variable to be held constant in any table.⁴⁹ The smaller the groups, the easier such statistical manipulation of group variables becomes, at least in principle. But surveys that study the joint effects of group and individual characteristics on behavior seldom have the resources to include the hundreds of groups that would be necessary if group characteristics were to be manipulated by cross-tabulation. (Studies based on census data can easily handle thousands of groups, but they do not often have data on the joint effects of individual and group variables.)

If groups differing in only a single characteristic are practically impossible to find in survey research and if large numbers of groups are impossibly expensive, is there any alternative? The most attractive alternative—perhaps the only one—is to abandon the attempt to deal with one group characteristic at a time and to describe the groups with as many variables as necessary. Where the number of groups is too small for cross-tabulation, they should be described in terms of a multivariate typology.⁵⁰ In the first example above, for instance, Calabria, Ireland, and Spain would have been described not simply as poor but as poor, Catholic, and having a low level of education. Theoretical simplicity is thus sacrificed for theoretical and empirical accuracy. These countries would differ from France on three or more independent variables instead of one, but they would be described in all their relevant aspects.⁵¹

It is only fitting to close this argument for the typological analysis of group characteristics by noting that here, too, Durkheim pointed the way. In his chapter "How To

Determine Social Causes and Social Types" (Book II, chap. i), he argues for the classification of suicides, not as the simple act of killing one's self, but according to "morphological types" determined by the psychological and other characteristics of each suicide. It is only because the necessary data were not available that he was compelled to use his "etiological types." The same logic and the greater ease with which group data may now be gathered suggest that groups be described as multivariate types rather than by a single variable.

Despite its occasionally archaic language ("suicidogenic current"), the empirical analysis in *Suicide* is as vital today as it was in 1897—perhaps more so, since the quantitative approach that Durkheim pioneered has since become widely accepted among sociologists. But overemphasis on the quantitative aspects of *Suicide* would be as dangerous as total neglect, if it furthered the current tendency to substitute technical virtuosity for hard thinking about empirical data, thinking that is guided by theory and is directed toward enriching theory. This, after all, is the essential message of *Suicide*: that methodology is valuable insofar as it springs from the needs of theory and that theory is most fruitful when it is continually tested and refined in empirical research.

UNIVERSITY OF CALIFORNIA (BERKELEY)

⁵¹ When the number of variables and the number of groups is as large as fifteen or twenty, it becomes economical to use factor-analytic procedures for determining the dimensions of the typology (see Andrew W. Halpin, *The Leadership Behavior of School Superintendents* [Columbus: College of Education, Ohio State University, 1956], pp. 3-10; Selvin, *The Effects of Leadership Climate*, pp. 11-39). The descriptions of group characteristics in these two works indicate that factor analysis is ideally suited to the study of groups (while it is, at best, a crude tool for the study of individuals). Briefly, the reason is that the correlations of group means and group proportions are virtually free from the idiosyncratic and random errors that attenuate the correlations of individual data, so that relatively few factors account for most of the variation in a large number of variables.

⁴⁹ George Peter Murdock, *Social Structure* (New York: Macmillan Co., 1949).

⁵⁰ Allen H. Barton, "The Concept of Property-Space in Social Research," in Lazarsfeld and Rosenberg, *op. cit.*, pp. 40-53.

SOCIAL MORPHOLOGY AND HUMAN ECOLOGY

LEO F. SCHNORE

ABSTRACT

This essay re-examines Durkheim's *Division of Labor* from the standpoint of contemporary ecological theory. Durkheim's analysis of differentiation is reconstructed, and a series of criticisms of the work are reviewed. A number of striking parallels are found between Durkheim's "morphological" thinking and the structural concepts and analytical apparatus in use within human ecology throughout its development. The paper ends with a plea for greater attention to the construction of structural taxonomies and to the examination of other morphological problems.

INTRODUCTION

Émile Durkheim, of course, was not himself a human ecologist. The ecological viewpoint did not develop within sociology until near the end of Durkheim's life, and then in America.¹ There is no evidence that this new approach to social phenomena exerted any profound influence upon his thought, despite the fact that he regarded "social morphology" as one of the major branches of sociology. In Durkheim's scheme, this field was to be devoted to two major inquiries: (1) the study of the environmental basis of social organization and (2) the study of population phenomena, especially size, density, and spatial distribution.² These areas of interest obviously converge with those of human ecology as it was originally formulated.

This paper consists of an exegesis and a critique of one of his major theoretical contributions and a consideration of the broad

implications of his "morphological" analysis for contemporary human ecology. It is concerned, for the most part, with Durkheim's doctoral dissertation, *De la division du travail social: étude sur l'organisation des sociétés supérieures*, first published in 1893.³ More particularly, the discussion is largely limited to Book II, where he dealt with the "causes" of division of labor and where the morphological approach was most explicitly used. The brief exegesis is based on a selective restructuring of his main argument, which is unfortunately scattered through many pages. We trust that taking up the crucial elements in his thought in somewhat different order does no violence to the essential logic of his position. This procedure has been adopted in order to point up the contrasts between his morphological theory of differentiation and the alternative explanations that were available at the time that he wrote.

EXEGESIS

First, it must be emphasized that Durkheim's intention in Book II of *Division* was

¹ Durkheim died in 1917, and the first use among sociologists of the term "human ecology" did not appear until 1921, in Robert E. Park and Ernest W. Burgess (eds.), *Introduction to the Science of Society* (Chicago: University of Chicago Press, 1921), pp. 161-216. However, Durkheim was familiar with the work of Ernst Haeckel, who coined the word "ecology" in 1868 and who is often described as the father of plant ecology.

² Taken from Durkheim's essay, "Sociologie et sciences sociales" (1909); cited in Harry Alpert, *Émile Durkheim and His Sociology* (New York: Columbia University Press, 1939), p. 51. Durkheim's own discussion of social morphology appears in scattered essays and reviews in *L'Année sociologique* (old series), e.g., "Note sur la morphologie sociale," II (1897-98), 520-21.

³ Paris: Alcan, 1893; translated by George Simpson as *The Division of Labor in Society* (New York: Macmillan Co., 1933; Glencoe, Ill.: Free Press, 1947). All citations to *Division* hereafter refer to the 1947 edition. Occasional reference will also be made to *Les Règles de la méthode sociologique* (Paris: Alcan, 1895), a collection of essays that had appeared in *Revue philosophique* in 1894. *Les Règles* was translated by Sarah A. Solovay and John H. Mueller and edited by George E. G. Catlin as *The Rules of Sociological Method* (Chicago: University of Chicago Press, 1938; Glencoe, Ill.: Free Press, 1950). All subsequent citations to *The Rules* refer to the 1950 edition.

to account for differentiation and its obvious increase in Western societies. The very subtitle is the key: "A Study of the Organization of Advanced Societies." Second, it is necessary to preserve the historical context of his work. The division of labor had long interested social philosophers, especially in the West. As early as 1776, Adam Smith had pointed to division as the main source of "the wealth of nations," and the concept itself can be traced at least to the Greeks. Unfortunately, these earlier writers gave scant attention to the determinants of differentiation, contenting themselves with analyses of its nature and its implications for economic efficiency and productivity.

In the latter half of the nineteenth century, however, increasing effort was given to explaining the process, with special reference to the "advanced" societies of the time. Comte dealt with the matter at some length, discussing the nature of differentiation as a generic social phenomenon.⁴ Tönnies and Simmel also examined the problem in publications that preceded Durkheim's by only a few years.⁵ By the time that Durkheim began his work, however, the dominant views in intellectual circles were still a peculiar admixture of utilitarian and evolutionary "explanations," both best represented in the works of Herbert Spencer. In large part, Durkheim's analysis must be seen as a reaction against the Spencerian view.

⁴ August Comte, *The Positive Philosophy*, translated and edited by Harriet Martineau (New York: D. Appleton, 1853).

⁵ Ferdinand Tönnies, *Gemeinschaft und Gesellschaft* (1887), translated and edited by Charles P. Loomis as *Fundamental Concepts of Sociology* (New York: American Book Co., 1940). Although it is not cited in *Division*, Durkheim had previously reviewed *Gemeinschaft und Gesellschaft* in highly favorable terms (see *Revue philosophique*, XXVII [1889], 416-22). Georg Simmel's *Über soziale Differenzierung* appeared in 1890, but Durkheim indicated that he did not see it until after 1893, when *Division* first appeared. For a general critique of Simmel see Durkheim's "La Sociologia ed il suo dominio scientifico," *Rivista italiana di sociologia*, IV (1900), 127-48.

Durkheim's own analysis actually began in Book I, with a distinction between two forms of organization somewhat similar to the types sketched by Maine and Tönnies. The first type ("mechanical") was used by Durkheim to describe the relatively undifferentiated or "segmented" mode of organization characteristic of small and isolated aggregates, in which little control has been achieved over the local environment. The basis of social unity is likeness or similarity. There is minimal differentiation, chiefly along age and sex lines, and most members are engaged most of the time in the same activity—collecting, hunting, fishing, herding, or subsistence agriculture. The "social segments" of the community (families and kinship units) are held together by what they have in common, and they derive mutual support from their very likeness. Unity is that of simple "mechanical" cohesion, as in rock forms, and homogeneity prevails.⁶

Durkheim was fully aware that structural differentiation is a variable characteristic of aggregates, for he recognized another and fundamentally different mode of organization. He saw that modern Western society was based increasingly upon differentiation, and his concept of the "organic" type of organization was designed to describe the complex and highly differentiated structural arrangements of his own time. According to Durkheim, a complex and heterogeneous society, like all but the most rudimentary organisms, is based on an intricate interdependence of specialized parts. Labor is divided; all men do not engage in the same activities, but they pro-

⁶ "We say of these societies that they are segmental in order to indicate their formation by the repetition of like aggregates in them" (*Division*, p. 175). This type is not to be understood as somehow lacking any differentiation whatsoever (see pp. 129, 173, 177, 180). As Redfield has suggested, homogeneity in simpler societies is more than merely "occupational," extending to biological characteristics and even to outlook. Small size and extreme isolation appear to be crucial factors in the development of both genetic and cultural homogeneity (see Robert Redfield, "The Folk Society," *American Journal of Sociology*, LII [1947], 292-308).

duce and exchange different goods and services. Moreover, not only are individuals and groups differentiated with respect to functions, but whole communities and nations also engage in specialized activities. In short, there has been a breakdown of internal "segmentation" *within* communities and societies and a reduction of isolation *between* them, although mechanical solidarity never completely disappears.⁷

With this distinction between major types of organization in mind, Durkheim's task in Book II was to explain the conditions under which "mechanical" organization is superseded by the "organic" form. According to the mode of analysis that prevailed at the time that he wrote, Durkheim viewed this change in social organization as comprising a kind of "evolutionary" sequence, and much of his theory was cast in these terms. However, it would be extremely misleading to portray his work as that of an uncritical evolutionist, for Durkheim possessed a sensitive, critical mind and he considered and rejected a number of alternative hypotheses that had been widely accepted as explanations of increasing differentiation.⁸

With respect to the popular utilitarian version, Durkheim vigorously attacked the idea that differentiation was somehow the product of man's rational desire to increase his own happiness. In fact, he rejected all individualistic interpretations. The notion that social structure is merely the product of the motivated actions of individuals was apparently almost repugnant to him. It ran directly counter to his conception of society as an entity *sui generis*, and it obviously violated his most famous principle: that "the determining cause of a social fact

⁷ See *Division*, p. 229, and Durkheim's assertion that "mechanical solidarity persists even in the most elevated societies" (p. 186). Some critics erroneously accuse him of failing to see that both forms of integration can be found in every society.

⁸ Durkheim usually tried to dispose of competing hypotheses before setting out his own views. *Division* contains a perfect example of his didactic style, which Alpert calls the method of "argumentum per eliminationem" (*op. cit.*, pp. 84-87).

should be sought among the social facts preceding it and not among the states of the individual consciousness."⁹

Durkheim then turned his attention to the evolutionary portion of the Spencerian argument. The organismic analogy, of course, was in vogue at the time, and Spencer had used it brilliantly. As to the division of labor in society, Spencer had held that "along with increase of size in societies goes increase of structure. . . . It is also a characteristic of social bodies, as of living bodies, that while they increase in size they increase in structure. . . . The social aggregate, homogeneous when minute, habitually gains in heterogeneity along with each increment of growth; and to reach great size must acquire great complexity."¹⁰ In other words, Spencer's theory of differentiation—despite its cosmic overtones and utilitarian underpinnings—reduced to an explanation based on sheer population size. At the very least he pointed to a universal association between size and differentiation.

Durkheim recognized the potential role of population increase in bringing about further differentiation. Along with Adam Smith, he was aware of the permissive effect of sheer size.¹¹ Large aggregates allow greater differentiation to emerge, but Durkheim concluded that the population-size factor was a necessary, but not a sufficient, cause. His reasons for this conclusion are particularly instructive. In contrast to Spencer, who exemplified the deductive method of proceeding from first

⁹ *The Rules*, p. 110. In the course of his argument, Durkheim cited comparative suicide rates as "proof" to the contrary. Whatever the merits of this argument, it is interesting to note that Durkheim here anticipated his later work in this area. At another point, he dealt with religious phenomena (Book I). A number of writers have observed that *Division* contained the seeds of all his later work.

¹⁰ Herbert Spencer, *Principles of Sociology* (London, 1876; New York: D. Appleton & Co., 1884 and 1892), I, 459 (1892 ed.).

¹¹ See Smith's famous aphorism to the effect that "the division of labor is limited by the extent of the market" (*The Wealth of Nations* [New York: Modern Library], p. 17).

principles, Durkheim was very much the inductive analyst. In fact, he showed the underlying weakness of Spencer's theory by pointing to "deviant cases." Concretely, he called attention to large, densely settled areas in China and Russia clearly characterized, not by extreme differentiation (organic solidarity), but by homogeneity (mechanical solidarity).¹²

Having thus rejected the Spencerian argument on empirical grounds, Durkheim tried to explain the absence of any marked differentiation in these places in the face of great size and density. It is at this point that Durkheim introduced a series of essentially sociological concepts, the first of which must be seen as an "intervening variable." First, he noted that social segmentation had not broken down (i.e., that there was minimal contact between the constituent parts of Chinese and Russian society). In the face of limited contact, these parts remained homogeneous, very much like each other with respect to structure and functions, representing a proliferation of essentially similar village units. Durkheim asserted that this "segmentation" disappears and that division increases only with an increase in "moral" or "dynamic density." In contrast to physical density—the number of people per unit of space—"dynamic density" refers to the density of social intercourse or contact or, more simply, to the rate of interaction—the number of interactions per unit of time. Until this rate of interaction reaches a high (although unspecified) level, the constituent social segments or parts remain essentially alike. According to Durkheim: "The division of labor develops . . . as there are more individuals sufficiently in contact to be able to act and react upon one another. If we agree to call this relation and the active commerce resulting from it dynamic or moral density, we can say that the progress of the division of labor is in direct ratio to the moral or dynamic density of society."¹³ In other words, differentiation tends to in-

crease as the rate of social interaction increases.

Durkheim then asked the next logical question: Under what conditions does this rate of interaction increase? In answer, he first observed that dynamic density "can only produce its effect if the real distance between individuals has itself diminished in some way."¹⁴ He then pointed to two general ways in which this might come about: (1) by the concentration of population, especially in cities, i.e., via increases in *physical density*; (2) by the development of more rapid and numerous means of transportation and communication. These innovations, "by suppressing or diminishing the gaps separating social segments . . . increase the [dynamic] density of society."¹⁵

Thus, to demographic factors (essentially the Spencerian explanation), Durkheim added a technological emphasis. An increase in population size and density *plus* more rapid transportation and communication bring about a higher rate of interaction. However, the crucial questions still remain: what brings about differentiation? Why should a simple increase in the rate of interaction produce greater division of labor? If social units (whether individuals or collectivities) are brought into more frequent contact, why should they be obliged to specialize and divide their labor? A

¹² *Division*, p. 257. Later, Durkheim graciously credited Comte with this basic idea (see *ibid.*, pp. 262–63). In the quoted passage and elsewhere, Durkheim spoke as if the individual were the referent. However, the treatment of change that he applied to interindividual relations appears to be even more appropriate in the analysis of the changing relations between areal units or whole aggregates in the process of differentiation.

¹⁴ *Ibid.*, p. 257.

¹⁵ *Ibid.*, pp. 259–60. Durkheim went on to say that one can usually substitute physical density ("this visible and measurable symbol" or index) for dynamic density, but that they are not inevitably correlated (see n. 11, p. 260). The point apparently troubled Durkheim, for in *The Rules* he repeats this idea in the form of an apology for having confused the two types of density (see *The Rules*, p. 115).

¹³ *Division*, p. 261. This thought is further developed in *The Rules*, p. 115.

simple identification of "factors" obviously was not enough; Durkheim was also compelled to indicate the mechanism that would produce further differentiation under the prescribed circumstances. As it turns out, he had in mind a particular type of interaction, viz., competition.

It is in his identification of competition as the vital mechanism that Durkheim borrowed most heavily upon Darwinian thought, and it is this part of his theory that has been most widely distorted. Durkheim's argument was based on Darwin's observation that, in a situation of scarcity, increased contact between like units sharing a common territory leads to increased competition. Being alike, they make similar demands on the environment. Inspired by the Malthusian account of population pressure on limited resources, Darwin had been led to stress the resultant "struggle for existence" as the essential condition underlying the differentiation of species. In the human realm, Durkheim reasoned in turn, individuals or aggregates offering the same array of goods or services are potential, if not active, competitors. Thus, according to Durkheim,

If work becomes divided more as societies become more voluminous [i.e., larger in size] and denser, it is not because external circumstances are more varied, but because struggle for existence is more acute. Darwin justly observed that the struggle between two organisms is as active as they are analogous. . . . Men submit to the same law. In the same city, different occupations can co-exist without being mutually obliged to destroy each other, for they pursue different objects. . . . The division of labor is, then, a result of the struggle for existence, but it is a mellowed dénouement. Thanks to it, opponents are not obliged to fight to a finish, but can exist one beside the other. Also, in proportion to its development, it furnishes the means of maintenance and survival to a greater number of individuals who, in more homogeneous societies, would be condemned to extinction.¹⁶

The division of labor is thus seen by Durkheim as essentially a mode of resolving

competition and as an alternative both to Darwinian "natural selection" and to Malthusian "checks."

CRITIQUE

One might conclude from the foregoing that Durkheim merely substituted one variety of evolutionism for another, by pointing to a Darwinian struggle for existence between competitors as the mainspring of differentiation, rather than Spencerian cosmic forces leading inexorably to increased division. Indeed, the common interpretation of *Division* has been along these lines. Consider, for example, Benoit-Smullyan's remarks:

Having disposed of the psychologistic and individualistic explanations of the division of labor, Durkheim now turns to his own morphological explanation. . . . Division of labor is due to changes in social structure arising out of an increase in material and moral density. The increase in population intensifies competition and thus forces individuals to specialize, in order to survive. Thus Durkheim, rather reluctantly, comes to rest his entire explanation upon the factor of an assumed natural increase in population. This is obviously a biologicistic rather than a sociologicistic type of explanation.¹⁷

¹⁶ Émile Benoit-Smullyan, "The Sociologism of Émile Durkheim and His School," in Harry Elmer Barnes (ed.), *An Introduction to the History of Sociology* (Chicago: University of Chicago Press, 1948), p. 508. This point has been widely misunderstood. Sorokin, for example, says that "as soon as Durkheim puts this problem, he has to recognize at once its dependence on the factor of procreation and multiplication of the people—a factor essentially biological. Increase of labor division is principally the result of an increase of population. Such is his answer" (Pitirim Sorokin, *Contemporary Sociological Theories* [New York and London: Harper & Bros., 1928], p. 480). In a similar vein, Parsons says: "What he ends up with is population pressure, not in any analytical sense a social element at all, but essentially biological. In so far as this is Durkheim's main line of thought it is a familiar one here; it is the breakdown of utilitarianism into radical positivism, in this case the 'biologizing' of social theory" (Talcott Parsons, *The Structure of Social Action* [New York: McGraw-Hill Book Co., 1937; Glencoe, Ill.: Free Press, 1949], p. 323). Regarding Durkheim's explanation, Alpert has written: "If we accept this statement at face value, we must conclude that Durkheim's

¹⁶ *Division*, pp. 266-70.

On the contrary, Durkheim tried to spell out the conditions under which one variety of "social evolution" would occur, by pointing to the factors underlying increased structural complexity. Far from assuming natural increase and then using population growth as the explanation (à la Spencer), Durkheim clearly asserted that differentiation will accompany growth only if interaction increases concomitantly; moreover, he suggested that this intensification of interaction ordinarily occurs as a result of technological changes that facilitate contact, exchange and communication.¹⁸ Thus Durkheim rejected a single-factor explanation—whether it be the individual's desire for happiness, cosmic evolutionary force, or population size—and proceeded to construct a multiple-factor theory.

But what of the charge that Durkheim disobeyed his own rules and thus became guilty of "biological reductionism"? This question can be answered best by recalling the explanatory concepts that he employed (i.e., dynamic density and competition). Both refer to interaction and can hardly be called intrinsically biological constructs without stretching the meaning of "biological" to the point where it loses all discriminatory value. If anything, these are clearly sociological concepts. Moreover, Durkheim's technological emphasis—his stress upon the role of improvements in transportation and communication—cannot properly be called "biological" reasoning.

As to the dependence of the theory upon an assumed natural increase, Durkheim's critics have again fallen into error. An in-

casual explanation of the division of labor is couched, contrary to his own methodological postulate that social facts must be explained socially, in biological, or, more exactly, in demographical terms. It is no wonder, then, that his book has been qualified as Malthusian" (*op. cit.*, p. 91).

¹⁸ It is true that he did not go on to "explain" growth or technological innovation. The task that he had set for himself was the explanation of differentiation, and to confine his discussion to the implications of increase in size and density—whatever their sources—was an entirely legitimate scientific procedure.

crease in effective population size can obviously occur in several ways, of which natural increase is only one. Following out the implications of Durkheim's thought, it is readily apparent that improvements in transportation and communication can bring into sustained contact previously separate areas and populations. Historically, such "growth by merger" has frequently involved political merger, whether by violent subjugation or peaceful assimilation, and often has witnessed an extension of the area of regular economic exchange. These political and economic changes can be subsumed under the "biological" rubric only with difficulty, if at all.¹⁹

Although Merton is also inclined to view parts of Durkheim's explanation as biological, he has pinpointed the truly sociological character of Durkheim's analysis in the following passage:

It is true that he finds the "determining cause" of increased division of labor in the growth and heightened density of populations, which is primarily a biological factor, but it is only in so far as this demographic change is associated with increased social interaction and its concomitant, enhanced competition, that the stipulated change will occur. *It is this social factor—the "dynamic density" as he terms it—which Durkheim finds actually determinant. . . .* To the extent that this differentiation is generalizable as a social process it may be said to be associated with competition between individuals

¹⁹ The critics cited above do not seem to appreciate the fact that changes in effective population size need not depend upon natural increase or decrease. Demographers have long been aware that the organizational response to population pressure in a given area has frequently taken the form of a splitting-off process, in which a whole segment of a local community moves off to establish a new colony. This process, to which biblical reference may be found, apparently had a large role in the spread of mankind over the earth; it may be labeled "fission." Growth by merger, however, presents a contrasting phenomenon. Although it often generates substantial migratory streams, migration is not intrinsic to the process, nor is natural increase or decrease necessarily involved. To preserve the metaphor, this form of increase may be called growth by "fusion." Both processes, of course, refer to sociological (organizational) changes and not to biological changes.

and between groups, whatever the factors leading to such competition.²⁰

Even Durkheim's "evolutionism" is not really biological in orientation. Although he did use the language of evolutionary thought, he clearly rejected most of the prevailing evolutionist views on the nature of social change. There is no idea here of unilinear, irreversible development in a fixed sequence of stages, no suggestion of "progress" as a necessary consequence of greater complexity, no hint of blind cosmic forces animating the whole process, as in Spencer's thought.²¹ Durkheim simply attempted to specify the social conditions under which a particular change in social organization tends to occur. In addition, he attempted to identify the general mechanism by which like units become unlike, through the resolution of competition. Unfortunately, the process is not described in any detail. Presumably the unsuccessful competitors (individuals, groups, or territorial aggregates) take up new functions and somehow become integrated in a more inclusive and complex system.

At any rate, it should be evident by this point that most criticisms of Durkheim's theory of differentiation have been misplaced. As they have been stated, they might better be aimed at Spencer—the theorist against whom Durkheim was contending throughout his entire analysis. The unfortunate effect of these errors of inter-

pretation is plain: to the extent that these secondary sources are read in place of the original work, a whole generation of American sociologists has been given an essentially incorrect image of one of Durkheim's most important theoretical contributions. American sociology is probably the poorer for it. Durkheim clearly viewed "the origin of social species" as the product of social and not biological forces. If his analysis were not so clear on this issue, the apparent unanimity of his critics would be more compelling.

To say that most of the prevailing criticisms of Durkheim's theory are themselves unsound, however, is not to say that the theory is entirely satisfactory as it was originally stated. The major difficulty stems from his treatment of competition. In view of the great importance that he attached to it, his discussion is surprisingly brief. If differentiation is the resolution of competition that does occur and if a more complex organizational pattern does emerge to integrate the new specialties, it may be correct to view these developments as due to increases in effective size and improvements in the facilities for movement. However, a number of writers have suggested that differentiation is not the only resolution of competition.²²

"Competition" occurs whenever the number of individuals or units with similar demands exceeds the supply, whether it be food, raw materials, markets, or occupational positions. As Durkheim suggested, differentiation represents a less harsh resolution of competition than that stressed by Darwin and Malthus. But in the case of human populations, the competition resulting from an increase in demand (popula-

²⁰ Robert K. Merton, "Durkheim's Division of Labor in Society," *American Journal of Sociology*, XL (1934), 325-26; italics added. In addition to calling population change "biological," Merton is in error regarding the implied universality of the association between competition and differentiation.

²¹ See *Division*, pp. 141-42. Merton has erroneously identified Durkheim as a unilinear evolutionist (*op. cit.*, pp. 324-25). Marjolin provides a more accurate view of Durkheim's position: "For him there does not exist any single general human society, but only particular societies which follow diverse evolutions, and it is not possible to consider the conditions which they have reached as stages in a single developmental sequence" (see Robert Marjolin, "French Sociology—Comte and Durkheim," *American Journal of Sociology*, XLII [1937], 694).

²² As Parsons has noted, Durkheim "recognizes the fact that there is more than one possible outcome of this intensification [of competition]. It might lead simply to the elimination by natural selection of a larger proportion of those born" (*op. cit.*, p. 322). Benoit-Smullyan indicates emigration or war as additional alternatives (*op. cit.*, p. 530). Alpert extends the list: "Of course, there are many other ways out, such as migration, suicide, civil war, crime, etc." (*op. cit.*, p. 94).

tion) theoretically can be resolved in a number of ways. Among them are the following:

Demographic changes.—(1) As Durkheim recognized, following Darwin and Malthus, an increase in the death rate can bring population into line with resources. (2) Similarly, a decrease in the birth rate can have the same effect, although not so immediately. (3) Migration may remove excess numbers at least temporarily and thus reduce demand.

Technological changes.—A number of possible developments may redefine and expand the effective environment, thus altering the supply. (4) Previously unused local resources may be brought into use via technological innovation or diffusion; the result is a more intensive use of environmental elements already present but unexploited. (5) Technological changes in transportation and communication, whether indigenous or borrowed, can make new areas and new resources available; such changes may also improve the internal distribution of commodities. (6) The substitution of mechanical for human energy may increase production and release manpower for other pursuits, including new occupations; thus the shift in the energy base of modern societies can be viewed as a process of displacement of the affected sectors of the population.

Organizational changes.—As noted above, previously isolated areas, resources, and peoples can be absorbed by conquest or assimilation. However, internal reorganization of a given population can also result in supporting increased numbers. (7) "Revolutionary" changes may occur; the surplus formerly held by the few may be distributed among the many, and increased numbers can be supported, with perhaps an even higher average level of living. (8) The converse can also occur; for a variety of specific reasons, the average level of living may be lowered, permitting a given area and its resources to support even greater numbers. (9) Finally, as Durkheim sug-

gested, occupational and territorial differentiation may occur.

This list of "alternatives" is probably not exhaustive, but it suggests that further differentiation is only one of a number of ways to resolve competition. It is also clear that these alternatives are not mutually exclusive, for a number of them have occurred simultaneously in the Western world.²³ This observation suggests that the changes that have occurred are concomitants of differentiation itself. Indeed, closer analysis reveals that each of these "alternatives" involves either (1) elimination of excess numbers, (2) expansion of the resource base, or (3) functional differentiation, or some combination of these changes.

In view of the importance that Durkheim attached to competition, however, it is unfortunate that he did not present a more explicit and systematic treatment of its resolution. He was inclined to invoke competition and to let it go at that. In passing, he remarked that "Spencer ably explains in what manner evolution will be produced, if it does take place, but he does not tell us the source producing it."²⁴ Durkheim, on the other hand, pointed to the sources of differentiation but offered little

²³ When they occur, it is clear that these changes need not "run in the same direction." The modern nations of the West have lowered their birth rates, extended the environment in breadth and in depth, substituted machines for men, and have become more highly differentiated at the same time that gigantic streams of migration were set loose. Meanwhile, death rates did not rise but declined, and the level of living did not fall but rose dramatically. Whether or not these other changes are intrinsic to differentiation, Durkheim took a rather oversimplified view of the entire process. It must be noted that he clearly recognized the first alternative listed above and that others (especially the fifth) are implicit in his analysis. It will be seen that alternatives 1, 2, 3, 7, and 8 have their primary effect upon the population, or "demand" side of the competitive equation, while alternatives 4, 5, and 6 have their major impact upon the resource, or "supply" side. Interestingly enough, alternative 9 (differentiation) has important effects upon *both* supply and demand; not only is the number of competitors effectively reduced, but the efficiency of differentiation presumably increases the supply.

²⁴ *Division*, p. 265.

in the way of a detailed account of the manner in which it is produced. In fairness, of course, it must be said that such a statement has yet to appear.

A more serious weakness in Durkheim's theory is the inadequate attention accorded the physical environment. He apparently was reluctant to give such factors as climate and topography any major role in his analysis. In part, this probably is due to the restrictive character of his own rules, adherence to which obliged him to seek the explanation of social facts in other social facts. He tended to dismiss the physical environment as a relevant variable and to regard the "social environment" as the ultimate source of differentiation. But this procedure has its own blind alleys; for one thing, the analyst does not get "outside the system" in his search for relevant variables.

To accept Durkheim's view of the limited role of environmental variability is to ignore two key considerations, the first of which is implicit in his own thought. (1) As suggested above, the effective environment can be altered by technological and organizational changes. These changes redefine the environment by bringing new resources into use—local resources already "there" but unexploited or resources found at sites that were previously inaccessible because of limited transportation facilities and exchange mechanisms. Although the initial impetus may not be the environment itself, it may become an important condition with respect to further organizational change. (2) Environmental variability must be viewed in static as well as dynamic terms. The plain fact of the matter is that the physical environment confronting mankind is almost infinitely variable, in the sense that there are enormous geographical differences from place to place. Some of these differences may favor organizational change. Long before Durkheim's time, Adam Smith perceived the significance of this factor, pointing to the greater likelihood of differentiated units appearing at the water's edge.²⁵ Since Durkheim

wrote, the role of a favorable geographic position has been stressed frequently in discussions of the sites of early civilizations and of the deep-water orientation of most great cities throughout history. This emphasis also appears in Cooley's famous "break-in-transportation" theory, and it can be easily merged with Durkheim's own views on the crucial role of transportation and communication technology.²⁶

Nonetheless, Durkheim's own theory clearly minimizes the potential relevance for organization of variations in physical environment. The corrective probably lies in adopting the modern geographer's concept of the environment as a vital premisive factor with respect to human activities. This approach is best summed up in the view known as *possibilisme*, wherein the environment is viewed as a set of limiting conditions, which may be narrow or broad, depending upon the technological devices and modes of organization that prevail in a given population.²⁷

Despite these minor shortcomings, Durkheim provided a highly useful framework

²⁵ Charles Horton Cooley, "The Theory of Transportation," *Publications of the American Economic Association*, Vol. IX (May, 1894), entire issue; reprinted in Robert Cooley Angell (ed.), *Sociological Theory and Social Research* (New York: Henry Holt & Co., 1930). In fairness, it should be pointed out that Spencer appreciated the role of environmental variation, although his thinking on this issue is not easily reconciled with his general theory of differentiation. Spencer observed that all physical resources are not perfectly ubiquitous and that some areas are better suited to specialization in a narrow range of production (Spencer, *First Principles*, p. 381; cited in Durkheim, *Division*, p. 263). Durkheim's rejection of the environment as a significant factor in differentiation led him away from a potentially fruitful line of analysis—the study of territorial division of labor. He did note that "since the 14th century, the interregional division of labor has been developing" (*ibid.*, p. 188). In addition, his most detailed example involves an instance of territorial differentiation (*ibid.*, pp. 268–69).

²⁷ This view is commonly associated with the names of Jean Brunhes and Paul Vidal de la Blache. For a recent statement along these lines see Robert S. Platt, "Environmentalism versus Geography," *American Journal of Sociology*, LIII (1948), 351–58.

²⁶ Smith, *op. cit.*, chap. i.

for the analysis of social structure and particularly for the examination of changes in structure. From the ecological standpoint, *Division's* major contribution is its stress upon the significance of technological advances for the development of a more elaborate division of labor. As Durkheim correctly pointed out, the efficiency of transportation and communication affects the degree to which spatially separate and functionally dissimilar activities may be interrelated. This is especially evident in the case of territorial differentiation, in which whole areas are devoted to specialized functions. Such a development clearly depends upon the loss of isolation and the establishment and maintenance of sustained contact.

Division provides, though only in outline, a framework for studying one of the most salient aspects of social organization, viz., the degree of structural differentiation. It can be applied to static, cross-sectional analysis as well as to dynamic, longitudinal study. Although it stands in need of certain modifications, his morphological theory seems particularly useful in approaching the problem of structural differentiation within and between areally based aggregates, i.e., communities. It is to this contention that the following section is addressed.

IMPLICATIONS FOR HUMAN ECOLOGY

The very first point to be made is that ecologists concern themselves with precisely the same problem as that attacked by Durkheim in Book II of *Division*. Just as he tried to explain one aspect of structure, contemporary ecologists attempt to identify the factors determining variations in structure. Hawley, for example, defines human ecology as the study of the form and development of the community. At one point, he adopts Durkheim's exact phraseology and describes the ecologist's objective as the elucidation of "the morphology of collective life in both its static and its dynamic aspects."²⁸ Although he represents a more traditional ecological viewpoint,

Quinn also declares that the logic of ecological inquiry points to the study of "the occupational pyramid" as essential subject matter, despite the unfortunate preoccupation of some ecologists with spatial distributions.²⁹ Thus modern human ecology deals with the Durkheimian problem of "morphology" and takes the same dependent variable (structure) as its *explanandum*. This is despite the fact that ecologists of Hawley's persuasion frequently limit themselves to discussing community structure, avoiding Durkheim's broader concern with society.

Second, once the environment is brought into the picture, modern ecology can be regarded as working with essentially the same array of *independent* variables—most broadly, population, technology, and the environment. Building on Hawley's theory, Duncan has labeled the resulting scheme "the ecological complex."³⁰ Although it tends to be implicit rather than explicit, Hawley's own effort seems to consist of treating community structure as the product of the interaction of these broad factors. The structure of a given community is viewed as a collective adaptation on the part of a population to its total environment (including other organized populations, as well as physical features), an adaptation that is strongly modified by the technological equipment in use and by certain "purely" demographic attributes of the population itself, notably its size, rate of growth, and biological (age-sex) composition.³¹

²⁸ Amos H. Hawley, *Human Ecology: A Theory of Community Structure* (New York: Ronald Press, 1950), p. 67.

²⁹ James A. Quinn, *Human Ecology* (New York: Prentice-Hall, 1950), p. 14.

³⁰ Otis Dudley Duncan, "Human Ecology and Population Studies," in Philip M. Hauser and Otis Dudley Duncan (eds.), *The Study of Population* (Chicago: University of Chicago Press, forthcoming).

³¹ In contrast to earlier ecological emphases, spatial distributions of population and human activities enter into Hawley's thinking only as convenient indexes of organizational form; in this view,

Thus the general relevance of Durkheim's thought to modern ecology is clear. He worked with essentially the same broad factors, taking one of them (structure) as his dependent variable. Moreover, his general mode of analysis is highly similar to that employed in current ecological theory. This becomes particularly apparent when one considers Hawley's treatment of differentiation, which clearly follows Durkheim in its major outlines.³² Moreover, there are obvious formal parallels between Durkheim's *mechanical-organic* typology and the concepts of *commensalism* and *symbiosis*, *categoric* and *corporate groups*, and *independent* and *dependent communities* in Hawley's work.³³ Both writers point to (a) two modes of relationship, or forms of interaction, between like and unlike unit parts and to (b) two major forms of organization, depending upon which type of relationship is most prominent. Also deserving stress here is their common search for the factors that explain the progressive breakdown of isolation, the welding-together of larger and more inclusive functional units, and the emergence of a more complex structure.

An even more recent variety of ecological thought—Julian Steward's "cultural ecology"—is amenable to interpretation along the lines suggested here. In other words, the "ecological complex" appears to be in use throughout much of Steward's work, despite the fact that he does not consciously focus upon organization as the *explanandum*, preferring to work with "culture," a much broader dependent variable, and despite the fact that he gives a much larger role to the physical environment than either Durkheim or Hawley.³⁴ Durk-

heim's influence on Steward is apparently more indirect, via Durkheim's contribution to the development of "functional anthropology."

But we need not confine ourselves to the most recent statements of the ecological position to see the relevance of Durkheim's thought. A Durkheimian approach has informed human ecology since its inception. In one of his most influential essays—"The Urban Community as a Spatial Pattern and a Moral Order"—Robert E. Park identified the subject matter of human ecology as "what Durkheim and his school call the morphological aspect of society."³⁵ It has probably also occurred to the reader that the use of the concept of competition in Durkheim's work is highly similar to Park's. To quote Park: "Competition determines the distribution of population territorially and vocationally. The division of labor and all the vast organized economic interdependence of individuals and groups of individuals characteristic of modern life are a product of competition."³⁶ Thus both Durkheim and Park saw structure as ultimately emerging out of competition in a context of scarcity, although Park was no more helpful than Durkheim in providing

space is of interest only to the extent that it reflects structure. The same thing can be said for temporal patterns, which also have value as indexes of organization (see Amos H. Hawley, "The Approach of Human Ecology to Urban Areal Research," *Scientific Monthly*, LXXIII [1951], 48-49).

³² Hawley, *Human Ecology*, chap. xi.

³³ *Ibid.*, chap. xii.

³⁴ Steward criticizes Hawley as "uncertain in his position regarding the effect of environmental adaptations on culture" and indicates that he prefers to give this factor a larger causal role (Julian H. Steward, *Theory of Culture Change* [Urbana: University of Illinois Press, 1955], p. 34). This greater stress on physical-environmental factors is undoubtedly related to the fact that ethnologists are more frequently concerned with simpler societies, where the physical environment is literally a more fundamental determinant, pressing upon small and stable local populations that survive by means of relatively simple and unchanging technology and organization. To use Duncan's "ecological complex" once again, where technology and population are relatively constant, the environment assumes the position of the dynamic causal variable with respect to organization.

³⁵ Originally published in 1925 as "The Concept of Position in Sociology" (see Robert E. Park, *Human Communities: The City and Human Ecology* [Glencoe, Ill.: Free Press, 1952], p. 166).

³⁶ Park and Burgess, *op. cit.*, p. 506.

a detailed account of the process as a whole.³⁷

In addition, it should be pointed out that Durkheim anticipated much of McKenzie's theoretical work, especially the latter's treatment of the rise of "metropolitan" communities. In Durkheim's analysis, we have seen that great stress is given to advances in transportation and communication technology, which lessen isolation and break down "social segmentation." McKenzie showed that this theory can be readily given an areal referent, since formerly isolated and territorially distinct populations are frequently brought into more intimate contact by virtue of improvements in transportation and communication. McKenzie saw the key feature of metropolitan development as the emergence of an intricate territorial division of labor between communities that were formerly almost self-sufficient, and he viewed the whole process as mainly due to technological improvements. In fact, McKenzie went so far as to characterize the metropolitan community as "the child of modern facilities for transportation and communication."³⁸

Although Durkheim's analysis was largely at the societal level and dealt mainly with occupational differentiation, McKenzie used an essentially similar model in treating communities and regions, analyzing the problem of territorial differentiation. The process of differentiation is presumably the same in each case. Units that are brought into contact via technological improvements become competitors; such units necessarily compete to the extent that they offer the same goods and services to the same population. In the communal or regional context, the resolution of this competitive situation is frequently effected

by territorial differentiation. Certain areal units, including whole communities, then give up certain functions and turn to new specialties. A case in point is the historical "flight" of certain specialties, particularly infrequently purchased goods and services, from nearby smaller cities to the metropolis, following the development of the automobile. In the process, formerly semi-independent centers, which once offered a rather full range of services, came to take up more narrowly specialized roles in a larger and more complex division of labor—the metropolitan community as a whole.³⁹

At any rate, whether we examine earlier or more recent versions of human ecology, Durkheim's stamp is clearly imprinted.⁴⁰ In order to provide maximum utility in ecological analysis, Durkheim's theory needs certain modifications, particularly along the lines of bringing the environment into the schema as a factor worthy of recognition. As a result of its conceptual heritage from biology, human ecology has a rather full appreciation of the role of the physical environment as it affects social structure. This is not to say, however, that the ecologist is an environmental determinist; rather, he points to the relevance of the environment as it is modified and redefined by the organized use of technology.

³⁹ That differentiation is not the only mode of resolution of competition is again dramatically demonstrated by the experience of many rural service centers and hamlets after the coming of the automobile. With their markets usurped by larger centers now within easy access, a great number of these smaller places literally disappeared.

⁴⁰ This review has been confined to American developments. In France, Durkheim's morphological interests were carried on by his students, especially Maurice Halbwachs. In addition to extending Durkheim's analysis of suicide and "collective representations," Halbwachs' *Morphologie sociale* (Paris: Armand Colin, 1938) drew heavily upon his mentor's views and—at the same time—incorporated an ecological perspective that is often strikingly similar to Park's. Halbwachs visited the United States and taught at the University of Chicago in 1930 (see his "Chicago, expérience ethnique," *Annales d'histoire économique et sociale*, IV [1932], 11–49).

³⁷ On the use of competition as an all-explanatory concept in the earlier ecological literature see Amos H. Hawley, "Ecology and Human Ecology," *Social Forces*, XXIII (1944), 398–405.

³⁸ See R. D. McKenzie, *The Metropolitan Community* (New York: McGraw-Hill Book Co., 1933); see also Leslie Kish, "Differentiation in Metropolitan Areas," *American Sociological Review*, XIX (1954), 388–98.

To paraphrase a recent compendium of valuable ecological data, man has a key role in changing the face of the earth.⁴¹ Although the human ecologist's initial concern may be with the interaction between "man and his total environment," as a sociologist he inevitably turns to a study of the organized relations between man and man in the environmental setting, i.e., to morphological considerations. As Park said for ecology, it is "not man's relation to the earth which he inhabits, but his relations to other men, that concerns us most."⁴² And in following out the interaction of a given aggregate with other organized populations, the ecologist necessarily concerns himself with what Durkheim called "the social environment."

CONCLUSIONS

The only American sociologists to make any intensive use of Durkheim's earliest and most ambitious work are those who have adopted the ecological perspective. Very little attention has been given to Durkheim's "social morphology," and his theory of differentiation has been widely misunderstood. Most American writers who have discussed *Division* have drawn upon Book I, where Durkheim treated the effects of division with his customary insight. His later works, especially those dealing with suicide and religion, have been much more influential in this country. In these later studies Durkheim was more frequently dealing with individual behavior, especially as it is "normatively defined" and modified by group ties.

This selective emphasis by American writers is probably related to the main drift of American sociology in this century (i.e., toward increasing concern with social-psychological considerations). Instead of taking social structure as the phenomenon to be explained—the dependent variable—most American sociologists habit-

ually deal with social structure as an independent variable with respect to individual behavior. More particularly, structure is usually treated as it is perceived by the individual.

Now it must be made very clear that this procedure is an entirely legitimate enterprise; the variables with which one works and their analytical status depend upon the problem to be investigated. Moreover, this approach has vastly illuminated the human situation. Since the individual is somehow regarded as a less abstract unit than the organized aggregate and as a more interesting subject for study, social psychology has grown rapidly and has made giant strides toward acceptance in the scientific community. Witness the present status of "behavioral science." For all its past progress and future promise, however, the social-psychological sector of sociology still deals with some of the consequences of structural arrangements, leaving the determinants of structure to someone else.

In the light of these considerations, Durkheim's conception of *collective representations*—"shared norms and values" in the contemporary lexicon—provides an interesting sidelight on the position of social psychology within sociology. Durkheim regarded these social phenomena as mere "emanations" of underlying social morphology or structure.⁴³ If one accepts this position, then he holds that the social psychologist be concerned with little more than the derivative manifestations or passive reflections of underlying structural arrangements. Such a view clearly poses the analysis of structure itself as a logically prior problem. However, if current sociological output is any measure, few of us are inclined to grant any kind of priority to a morphological approach.

It is true that Durkheim himself turned more and more to the analysis of individual behavior in his later years, but he rarely departed from his original position regard-

⁴¹ William L. Thomas, Jr. (ed.), *Man's Role in Changing the Face of the Earth* (Chicago: University of Chicago Press, 1956).

⁴² Park, *Human Communities*, p. 165.

⁴³ See Durkheim's "Représentations individuelles et représentations collectives," *Revue de métaphysique et de morale*, VI (1898), 273-302.

ing the undesirability of attempting to explain "social facts" by reference to individual characteristics.⁴⁴ This is in dramatic contrast to the direction taken in American sociology: toward the view that has been labeled "voluntaristic nominalism." As the most significant characteristic of American sociology, our fundamental postulates have recently been identified as follows: "The feeling, knowing, and willing of individuals—though limited by cultural prescriptions and social controls—are taken to be the ultimate source of human interaction, social structure, and social change. . . . Social behavior is interpreted voluntaristically. Social structures are real only as they are products of individuals in interaction."⁴⁵ One must be impressed by the fact that so many American theorists now acknowledge a heavy indebtedness to Durkheim. If this voluntaristic position is actually dominant, however, we have only succeeded in turning Durkheim upside down.

Be that as it may, Durkheim's conception of "social morphology" suggests that one of the most promising areas of structural analysis lies in the development of a general taxonomy of aggregates and collectivities. Few sociologists seem to have addressed themselves to this task in recent years. To the extent that "types of society" are used today, they represent minor variants of the dichotomies presented long ago by Tönnies, Durkheim, and other writers of the nineteenth century. More important, most of the refinements and reformulations of these typologies in recent years have been left to writers like Redfield and Steward. In other words, a genuinely sociological tradition is being kept alive by the efforts of anthropologists.

⁴⁴ Durkheim's shifting interests are mirrored not only in the subjects of his later books but also in his writings in the old series of *L'Année sociologique*. Although references to "social morphology" are less frequent after about 1905, it should be noted that it was maintained as a major caption as long as Durkheim himself held the editorship.

⁴⁵ Roscoe C. Hinkle, Jr., and Gisela J. Hinkle, *The Development of Modern Sociology* (New York: Doubleday, 1954), p. 73.

With respect to "types of community," the initiative has been taken by economists and geographers, despite the fact that many areas of current sociological interest absolutely require close attention to the community context. To choose only the most obvious example, community studies of stratification would probably be enormously improved if the over-all structure and functions of the selected research sites were indicated with some precision according to their taxonomic types. For one thing, the overgeneralizations that seem to emerge from many such studies might be far less frequent.⁴⁶ It is probably unfortunate that the few sociologists currently attempting to develop a systematic taxonomy of communities appear to be those who employ an ecological framework.⁴⁷

As for types of groups within communities and societies, we have not advanced very far beyond the rather rudimentary notions of "in-" and "out-groups" and "primary" versus "secondary" groups. Both of these dichotomies, of course, tend to be employed within a social-psychological context. The only notable recent addition to this limited array of group types is the notion of "membership" versus "reference" groups. However, the latter turn out not to be groups at all, for the distinction rests not upon structural or functional attributes of aggregates but upon the identifications

⁴⁶ See Seymour M. Lipset and Reinhard Bendix, "Social Status and Social Structure: A Re-examination of Data and Interpretations," *British Journal of Sociology*, II (1951), 150-68 and 230-54; Harold W. Pfautz and Otis Dudley Duncan, "A Critical Evaluation of Warner's Work in Community Stratification," *American Sociological Review*, XV (1950), 205-15; and Ruth Kornhauser, "Warner's Approach to Stratification," in Reinhard Bendix and Seymour M. Lipset (eds.), *Class Status and Power* (Glencoe, Ill.: Free Press, 1953), pp. 224-55.

⁴⁷ See Otis Dudley Duncan and Albert J. Reiss, Jr., *Social Characteristics of Urban and Rural Communities, 1950* (New York: John Wiley & Sons, 1956), pp. 215-370; for a more limited set of sub-community types see Leo F. Schnore, "Satellites and Suburbs," *Social Forces*, XXXVI (1957), 121-27.

GEORG SIMMEL'S STYLE OF WORK: A CONTRIBUTION TO THE SOCIOLOGY OF THE SOCIOLOGIST¹

LEWIS A. COSER

ABSTRACT

The peculiarities of Simmel's style of work are accounted for in terms of a consideration of his role within the academic structure of the Germany of his time. His non-conformity and innovation are explained, at least in part, in terms of his academic role set and the pressures exerted by the social structure of the academy.

Contemporaries of Georg Simmel often remarked upon peculiarities in his style of work—peculiarities that distinguished his work in a rather striking manner from that of other major sociologists. They stressed the dazzling brilliance of his writings and the brittle elegance, but they also noted the lack of systematic exposition and the almost studied disorderliness of his method.

More recently, Kurt H. Wolff observed that "Simmel often appears as though in the midst of writing he were overwhelmed by an idea, by an avalanche of ideas, and as if he incorporated them without interrupting himself, digesting and assimilating only to the extent granted him by the onrush."²

Up until now it has been generally assumed that these characteristics of Simmel's style might possibly be attributed to his personality and that sociological perspective was hence unlikely to provide significant clues for its understanding. I shall attempt in the following pages to show that this assumption may be unwarranted and that significant insights into what may first appear to be a purely psychological problem might be gained if we consider Simmel's role within the academic structure of the Germany of his time as well as his status in the intellectual community.³ Sociological perspective may give us clues other perspec-

tives fail to provide. Such an approach aims, to quote Robert K. Merton, to teach us something "about the processes through which social structures generate the circumstances in which infringement of social codes constitute a 'normal' (that is to say, an expectable) response."⁴

Simmel's biographers have remarked upon the fact that Simmel, though generally considered one of the leading intellects of his time, never received the academic recognition he seemed so amply to deserve.⁵ For fifteen years Simmel lectured as a *Privatdozent* at Berlin University. Within the German university system of the time a position of *Privatdozent* indicates an apprenticeship status, that of an unpaid novice in the hope of making an academic career.⁶ However, while still a *Privatdozent*, Simmel already published a number of his most significant works.⁷ It would seem, then, that

³ For earlier attempts at a sociology of the academy see several contributions to *Versuche einer Soziologie des Wissens*, ed. Max Scheler (Munich: Duncker & Humblot, 1924), especially the papers by Max Scheler, Paul Honigsheim, and Helmuth Plessner.

⁴ *Social Theory and Social Structure* (rev. ed.; Glencoe, Ill.: Free Press, 1957), pp. 131-32.

⁵ Cf. Nicholas J. Spykman, *The Social Theory of Georg Simmel* (Chicago: University of Chicago Press, 1925), pp. xxiii-xxix; Wolff, *op. cit.*, pp. xxiii-xix.

⁶ Cf. Abraham Flexner, *Universities* (New York: Oxford University Press, 1930), pp. 324 ff.

⁷ See Erich Rosenthal and Kurt Oberlaender, "Books, Papers, and Essays by Georg Simmel," *American Journal of Sociology*, XL (November, 1945), 238-47.

¹ This paper has greatly profited from a critical reading of an earlier version by Robert K. Merton of Columbia University, Kurt H. Wolff of Ohio State University, and Rose L. Coser of Wellesley College. I am grateful to them.

² *The Sociology of Georg Simmel* (Glencoe, Ill.: Free Press, 1950), p. xix.

Simmel, who had already achieved wide renown as a philosopher and sociologist, still had not attained a status within the academy commensurate with his intellectual achievements and his scholarly productivity. While he was generally viewed as a mature scholar, he was structurally still in an apprentice status within the academy.

In 1900 Simmel was finally promoted to the rank of *Ausserordentlicher Professor*. The reader who may be unfamiliar with the structure of German universities before World War I should be reminded here that *Extraordinarii* were then not members of the faculty. They had no full standing within the academy and no decision-making power; they were shut off from participation in its affairs and were most insecure financially.⁸ Theirs was an auxiliary and marginal status. Yet Simmel, by now a world-famous scholar, occupied this position for practically the rest of his career. He was given a full professorship in the provincial University of Strasbourg in 1914, but he soon fell ill and died in 1918.

This is not the place to consider in detail the reasons why Simmel was assigned inferior status within the academy. Anti-Semitism, academic jealousy, the feeling among some of his colleagues that his was a "destructive" rather than a "constructive" intellect—all these seemed to have played a part.⁹ Our concern here is less with the process through which status incumbents are selected and recruited than with the consequences that the incumbency of certain status positions is likely to have for the incumbent; our concern here is with role behavior rather than with role allocation.

It may be argued that Simmel's inferior status was itself a consequence of his particular style of work. But, even if it could be shown that Simmel's style of work in his earlier writings often called forth negative judgments from his professional status superiors, we must still ask why, instead of at-

tempting to conform more closely to the expectations of the senior members of the academy, he persisted in publishing works that proved to be similar in style to his earlier contributions. Why did he disregard the negative sanctions of members of the academy and accentuate those characteristics that they could be expected to disapprove?

Expectations of colleagues and superiors within the faculty typically exert pressure upon the incumbent of junior-status positions to live up to the rules of the academic game. These rules require, among others, intellectual discipline, the observance of fixed standards of scholarship, respect for the boundaries of the various specialized fields, attention to the contributions of senior men, etc.¹⁰ Those who attempt to create *ab ovo* are likely to be considered "unreliable," "outsiders," and hence to be mistrusted. As Plessner has argued: "Only those who are capable of developing the New out of the Old fit into the framework of scholarship."¹¹ Any reader of Simmel will be well aware that his work hardly lived up to these norms. It should therefore not be too surprising that he encountered powerful opposition among representative academic role partners, apparently already quite early in his career.

But if Simmel did not conform to these expectations, although surely desiring to be accepted in the academy, we are led to inquire whether the academic structure itself did not offer him opportunities for an alternative type of behavior. The concept of "role-set," recently introduced by Robert K. Merton, may serve us well here: a social status, Merton argues, involves not a single associated role but an array of associated social roles. Merton calls attention to the fact that role partners who are differentially located in the social structure may have differing expectations as to the behavior of

⁸ Flexner, *op. cit.*, pp. 234 ff. and 354 ff.

⁹ Marianne Weber, *Lebenserinnerungen* (Bremen: Johs. Storm, 1948), pp. 385-86; Wolff, *op. cit.*; Spykman, *op. cit.*

¹⁰ See Helmuth Plessner, "Zur Soziologie der modernen Forschung," in *Versuche einer Soziologie des Wissens*, ed. Scheler, pp. 407-25; Logan Wilson, *The Academic Man* (New York: Oxford University Press, 1942).

¹¹ *Op. cit.*, p. 422.

a person occupying a particular status.¹² So it seems to have been with the position of university instructor at Simmel's time. This position may entail, for those so inclined, a set of roles differing rather pronouncedly from the purely scholarly role. The German university teacher was expected to contribute to ongoing scholarship, and thus to address himself to his colleagues, but he was also expected to lecture to students. Many preferred to keep their lecture work at a minimum or to restrict it to work in small seminars with selected students. Others, among them Simmel, gave considerable emphasis to their activity as lecturers. Academic colleagues and superiors, however, were often rather ambivalent with respect to members of the faculty who spent what they considered excessive time in lecturing. What Logan Wilson has said about the American academy, where lecturing is a much more important and highly priced activity than in the German university, applies a fortiori to the latter: "The chief acclaim of the teacher comes from below, which source is not important as a means of raising one's status. The acclaim from one's peers is frequently of the sort that decries too much attention to teaching, and belittles the popular teacher as a mere showman."¹³

While the popular teacher may incur the displeasure of peers, he may in exchange gain the approval of other role partners—his lecture public or audience.¹⁴ But, in effect, he lives up to expectations distinct from those of his peers and superiors. This presents a major structural basis for the possible disturbance of a stable role-set among university teachers who have the ability to be popular. The audience does not necessarily judge the lecturer in terms of his systematic and methodical gathering

of evidence and his disciplined pursuit of painstaking research endeavors but rather in terms of the brilliance of his performance, the novelty of his ideas, and the ability to fascinate.

All contemporary accounts agree that Simmel lived up to such expectations superlatively. He was considered one of the most brilliant, if not the most brilliant, lecturer of his time. He attracted students from the most varied disciplines; foreign visitors; unattached intellectuals from the world of publishing, journalism, and the arts; and a goodly number of members of "society" in search of intellectual stimulation. It is no exaggeration to say that many of Simmel's lectures were public events and often described as such in the newspapers.¹⁵

His style of delivery seems to have enthralled his audience. A contemporary writes:

One could observe how the process of thought took possession of the whole man, how the haggard figure on the lecture platform became the medium of an intellectual process the passion of which was expressed not in words only; but also in gestures, movements, actions. When Simmel wanted to convey to the audience the core of an idea, he not only formulated it, he so-to-speak picked it up with his hands, his fingers opening and closing; his whole body turned and vibrated under the raised hand. . . . His intensity of speech indicated a supreme tension of thought; he talked abstractly, but this abstract thought sprang from lived concern, so that it came to life in the listener. . . .¹⁶

Another contemporary observer writes in a similar vein:

He "thinks aloud" somebody said of him. One could add: He thinks visibly, one imagines seeing how a thought occurs to him. . . . One can see how his brain operates, how he joins ideas like a carpenter joins wood. . . . One is led to participate in the construction. One doesn't

¹² *Op. cit.*, pp. 369 ff. Merton defines "role-set" as "that complement of role relationships which persons have by virtue of occupying a particular social status."

¹³ Wilson, *op. cit.*, p. 192.

¹⁴ Cf. Florian Znaniecki, *The Social Role of the Man of Knowledge* (New York: Columbia University Press, 1940).

¹⁵ See, e.g., Emil Ludwig, "Simmel auf dem Katheder," *Die Schaubühne*, X (April, 1914), 411-13; Theodor Tagger, "Georg Simmel," *Die Zukunft*, LXXXIX (October, 1914), 36-41; Paul Fechter, *Menschen und Zeiten* (Gütersloh, 1948), pp. 52-56.

¹⁶ Fechter, *op. cit.*

really listen, one participates in the thought process.¹⁷

Do we not have here some warranty to assume that, hurt and rebuffed as he may have been by the lack of recognition within the academy, Simmel came to rely increasingly on the approval of his lecture audience and hence to accentuate in his written style as well as in his oral delivery those characteristics that brought applause? It is interesting to note in this connection that, although his contemporaries have indicated that Simmel's audience gained the impression that he was "thinking aloud" while he lectured, so that they imagined that they—the listeners—were, so to speak, assisting, he was in actual fact wont to give some lectures several times with virtually no changes.¹⁸ It would seem that Simmel cared so much for audience reaction that at times, like Churchill in a later day, he deliberately gave the impression before his audience that he was struggling with his ideas when he had, in fact, worked out his thoughts long before. This he may have done to enlist and maintain the interest of his audience.

Not only Simmel's lectures but the bulk of his writings also exhibited the characteristics that his lecture audiences prized. The unmatched brilliance of some of his essays is clearly related to the brilliance of his oral delivery; in fact, many of his essays, if not most, were first presented in lecture form.

In his published papers Simmel addressed himself much more frequently to the non-academic audience. The bibliography compiled by Rosenthal and Oberlaender¹⁹ reveals that, of the 180 articles published in his lifetime in various journals, newspapers, and reviews, only 64 were published in scholarly journals, while 116 appeared in non-scholarly publications destined for a wider cultivated public such as liberal newspapers, art magazines, and literary monthlies.²⁰

¹⁷ Tagger, *op. cit.*

¹⁸ Fechter, *op. cit.*

¹⁹ *Op. cit.*

This tells us about his reading public, but it does not give us sufficient evidence to support our claim that the academy's negative sanctions pushed Simmel to seek approval elsewhere. His non-academic orientation, after all, could be an index of his secondary interest in the academy.

We have claimed that Simmel's development was part of a social process in which he addressed himself to two publics—his scholarly colleagues, on the one hand, and his eager listeners, on the other. We have implied that his success with non-specialized audiences met with further negative sanctions from his colleagues and induced him to seek success with a non-academic audience. Such a claim can be supported only if we can follow Simmel's development over time. Since his published articles are on record, a simple count of their publications in various reviews and papers, distinguishing between dates of appearance, can be used as an index.

While Simmel was still a *Privatdozent*, his hopes of being accepted in the academy must have been higher than later, when recognition was not forthcoming. If we distinguish between the periodical writings published before the turn of the century (i.e., during the time he served as a *Privatdozent*) and later years, we find that in the earlier period 50 per cent of his writings appear in scholarly journals, as against only 28 per cent in the latter (Table 1).²¹

At the start of his career Simmel apparently communicated with both scholarly and non-scholarly audiences; later he

²⁰ Kurt Wolff's supplementary bibliography lists an additional 20 contributions to non-scholarly journals and only 2 to scholarly journals (*op. cit.*, pp. liv-lv).

²¹ It would be possible, we believe, to show that what holds true for Simmel's published papers in the periodical press also applies to his books. But this would involve a somewhat complex content analysis, which we cannot undertake at this time. Yet even a cursory glance at the bibliography will show that Simmel's more systematic and scholarly work was mainly published in the earlier stages of his career.

tended to publish more and more in non-scholarly publications.²² Earlier, he still attempted to live up to the expectations of the academy; in later years, while he was not unmindful of the academy, the non-academic audience loomed larger.²³

We have said enough to make it at least plausible that Simmel's self-image must have been molded to a large extent by the particular audience which rewarded him and that his intellectual production was influenced by the saliency of the demands that his non-academic role partners made on him. The pressures on his role set exerted by these role partners led to appropriate modifications of self-definitions and to appropriate role behavior. His auxiliary status in the academy exerted pressure on him to find a supportive audience at the margin of the academy, and the attempt to live up to their expectations which he had provoked involved him in a further process of alienation from the demands of the academy. As in the case of "The Stranger," of whom he wrote so perceptively and so movingly,²⁴ his relations to the academy were a compound of nearness and remoteness. He was inorganically appended to the academy, yet he was an organic member of the group. He could afford to maintain such a difficult marginal role because he found support and encouragement among his non-academic listeners.²⁵

²² Though we are in no position to prove this, there are many indications that the audience to which he communicated by the spoken word was composed of the same type of individuals, often perhaps the same physical individuals, whom he addressed in the many non-scholarly journals for which he wrote.

²³ It might be, of course, that, as Simmel became progressively better known, and esteemed, by others than the academic audience, publishers of lay periodicals increasingly prevailed upon him to publish with them. In short, it may have been a case not only of Simmel's initiative in searching out another audience but also of a greater interest by the agents of that audience in what he had to say. This could be construed as an interactive process between Simmel and the lay audience, with the publishers acting as intermediaries.

²⁴ Wolff, *op. cit.*, pp. 402-8.

Simmel, the marginal man, the stranger, presented his academic peers not with a methodical, painstakingly elaborated system but with a series of often disorderly insights, testifying to amazing powers of perception. Yet Simmel's very quest for originality stemmed in part from his self-image as a scholar. The academy does not, of course, prize purely routine work; it requires that its members contribute original results²⁶—such results to be achieved, however, through academically approved means, within the academic rules of the game. Simmel conformed to the goals of the academy, but he rejected the norms governing the

TABLE 1

PERIOD OF PUBLICATION OF SIMMEL'S PAPERS
BY TYPE OF JOURNAL

JOURNAL TYPE	BEFORE 1900		AFTER 1900		TOTAL No.
	No.	Per Cent	No.	Per Cent	
Scholarly . .	31	50	33	28	64
Non-scholarly	31	50	85	72	116
Total . . .	62	100	118	100	180

ways and means for their attainment.²⁷ We have shown that his innovation can be accounted for, at least in part, in terms of his academic role-set, that is, by the pressures exerted by the very social structure of the academy. That structure led him to engage in non-conforming behavior but at the same time to the development and cultivation of originality. May we not discover here that academic disciplines sometimes profit from

²⁵ It is interesting to note that Simmel had no disciples in the academy, though he exerted some influence, whereas he had many followers among the literary intelligentsia.

²⁶ Cf. Robert K. Merton, "Priorities in Scientific Discovery," presidential address read at the Annual Meeting of the American Sociological Society, August, 1957 (*American Sociological Review*, XXII [December, 1957], 6).

²⁷ See Merton's "Social Structure and Anomie," in *Social Theory and Social Structure*, esp. pp. 140-41.

what is judged to be nonconformist behavior in some of their practitioners?²⁸ Nonconformist behavior can have both manifest and latent functions. The discipline of sociology owes manifold leads to Simmel's

travail—leads that may serve as points of departure for generations of patient and methodical investigators.

UNIVERSITY OF CALIFORNIA

A CONTEMPORARY ACADEMIC VIEW OF GEORG SIMMEL

[EDITOR'S NOTE.—By a fortunate twist of circumstances, the *Journal* received permission to publish a letter from a colleague of Georg Simmel. The contents of the letter were so appropriate to Mr. Coser's article that we have taken the liberty of making it a sort of "empirical" appendix to his remarks.

The letter was written in response to a request for an evaluation of Simmel from the Kulturerministerium of Baden. Simmel was then (1908) being considered for one of the two chairs of philosophy at Heidelberg, for which he had been recommended by Goethein and Max Weber.

The letter, which will appear in a forthcoming memorial volume by Michael Landmann and Kurt Gassen (title unknown [Munich: Duncker & Humblot, 1958]), was translated for the *Journal* by Herbert Menzel, of Columbia University. We are grateful to Arnold Simmel, of Columbia, for bringing the letter to our attention.]

As you no doubt expect, I will express my opinion about Professor Simmel quite frankly. I do not know whether or not he has been baptized, nor did I want to inquire about it. He is, at any rate, a dyed-in-the-wool Israelite, in his outward appearance, in his bearing, and in his manner of thinking. It is possible that this fact has stood in the way of his receiving a call abroad and of his promotion here (it is said that he was under consideration for a post in Vienna); but it is not necessary to adduce this fact by way of explanation. For his academic and literary merits and successes are very circumscribed and limited. His lectures are well attended. But it has long since been his habit to hold two-hour lectures, which are invariably in high demand in Berlin. He speaks exceedingly slowly, by dribs and drabs, and thus offers only little material, although it is well rounded, succinct, and polished. These features are very much appreciated by certain categories of students who are very numerous here in Berlin. In addition, he spices his words with clever sayings. And the audience he recruits is composed accordingly. The ladies consti-

tute a very large portion—even for Berlin. For the rest, there [appears at his lectures] an extraordinarily numerous contingent of the oriental world, drawing on those who have already settled here as well as on those who are still flooding in semester after semester from the countries to the East. His whole manner is in tune with their orientation and taste.

One does not come away from his lectures with too much of positive value; but it is pleasant to be offered up this and that titillating stimulation or volatile intellectual pleasure. To this must be added the fact that a Semitic lecturer—wholly, partially, or philo-Semitic, whatever he is—will find fertile soil, no matter what the circumstances, at a university where the corresponding part of the student body numbers several thousand, given the cohesion that prevails in these circles.

I do not imagine that the University of Heidelberg would be especially advanced by attracting that kind to its lecture halls. In fact, it is impossible for me to believe that the level of Heidelberg would be raised by allowing even broader space than it already occupies among the faculty to the world view and philosophy of life which Simmel represents, and which, after all, are only

²⁸ See, in this connection, Merton's crucial distinction between deviancy and non-conformity (*ibid.*, pp. 360-68).

too obviously different from our German Christian-classical education. I believe that such admixture as may be desirable for healthy development has been attained. After all, there can be only limited justification for tendencies which undermine and negate more than they lay foundations and build up, during an era which is inclined to set all pillars asway—and that not always out of scholarly zeal, but also out of a thirst for notoriety.

Simmel owes his reputation chiefly to his "sociological" activity. Upon it was based the request to grant him the (honorary) title of professor [at Berlin, on an earlier occasion.—TRANS.]. But this request was primarily supported by Schmoller, who is always so ready to engage in innovations. It is my view, however, that sociology has yet to earn its position as a scholarly discipline [Wissenschaft]. It is, in my opinion, a most perilous error to put "society" in the place of state and church as the decisive [maszgebend, literally, yardstick-providing] organ of human coexistence. It would not

seem right to me to give official standing to this orientation at this early date, especially not at a university which is as important to state and nation as Heidelberg is to Baden and Germany—least of all in the person of one who operates more by wit and pseudo-wit [geistreiches und geistreichelndes Denken] than by solid and systematic thinking.

Nor can I find that one comes away with much of permanent value from Simmel's writings (insofar as they have become familiar to me). It is hardly possible to treat of the mental life of the metropolis in a sparser and more biased way than he did in his lecture of that title at the Gehe Foundation in Dresden. I believe that there are more desirable and productive occupants for Heidelberg's second chair of philosophy than Simmel.

I regret that I must render an unfavorable judgment. . . .

[Signed] DIETRICH SCHAEFER

SOME HYPOTHESES ON SMALL GROUPS FROM SIMMEL

THEODORE M. MILLS

ABSTRACT

Forms of play, of games, and of similar phenomena reflect group processes, according to Simmel, and, according to him, dyads lack the integrative capabilities of triads. Just how play-form reflects group characteristics and how dyads and triads compare are issues which experimental sociology may clarify. To this end Simmel's points are reformulated as hypotheses, and experimental procedures for testing them are briefly noted.

Partly from a growing impression that Simmel's conceptions of group process have yet to be incorporated into our more general sociological theories and partly out of curiosity, I select for this special issue of the *Journal* several notions representing his theoretical interests, re-formulate them as hypotheses, attempt to specify the appropriate variables, and suggest briefly how they may be tested experimentally. That Simmel did not test his propositions by a similar procedure does not mean that it is foreign to his purposes; by one incisive example after the other and by continual return to, and revision of, a theme, he worked carefully toward closer approximations of what he considered universal properties of social processes. The techniques of experimentation are different, but its aim need not be. With imaginative experiments there is the possibility of attaching to a proposition an estimate of the probability that it is true. I have chosen a few of Simmel's statements for detailed consideration rather than present the fuller array that might reflect more accurately the variety of his interests.

PLAY-FORM AND GROUP PROCESS

"The more profound, double sense of 'social game' is that not only [is] the game . . . played in society . . . but that, with its help, people actually 'play society.'"¹

From this notion of Simmel's and from the subsequent one—that we may understand more fully societal forces at work by

analyzing the sociological form of its play—I would like to develop in this first section two hypotheses for possible test in the small-group laboratory. One hypothesis deals with the ability to play in any form (some groups can only play; some work while at play); the other involves four mechanisms used by groups to translate their group life into play-form.

There is, according to Simmel, a two-way relationship between the processes of the day-to-day group and the game, the party, social play, and other instances of "sociability." The first is that play-form is clearly and emphatically dissociated and autonomous from other affairs of the group; the second, that basic issues in the life of the group are represented in its form of play, either directly, or inversely, by their negation or some other mechanism. In respect to the first, Simmel writes:

Actual forces, needs, impulses of life produce the forms of our behavior that are suitable for play. These forms, however, become independent contents and stimuli within play *itself or, rather, as* play. . . . All these forms are lifted out of the flux of life and freed of their material with its inherent gravity. On their own decision, they choose or create the objects in which they prove or embody themselves in their purity. This is what gives play both its gaiety and the symbolic significance by which it is distinguished from mere joke. . . . It is their [art and play] origin which keeps them permeated with life, that they draw their depth and strength. . . . Yet their significance and their very nature derived from that fundamental change through the forms engendered by the purposes and materials of life, are separated from them, and themselves become the purpose and the

¹ Kurt H. Wolff (ed.), *The Sociology of Georg Simmel* (Glencoe, Ill.: Free Press, 1950).

material for their own existence. From the realities of life they take only what they . . . can absorb in their autonomous existence.²

And further:

All the forms of interaction or sociation among men—the wish to outdo, exchange, the formation of parties, the desire to wrest something from the other, the hazards of accidental meetings and separations, the change between enmity and cooperation, the overpowering by ruse and revenge—in the seriousness of reality, all of these are imbued with purposive contents. In the game they lead their own lives; they are propelled exclusively by their own attraction.³

This tacit understanding that play is not work, fantasy, not fact, and make-believe, not reality, gives these forms enough freedom so that they can reflect reality, fact, and the issues at work in the group. In designating "sociability," or the social game, as the play-form of social interaction, Simmel suggests that its connection to these interactions is similar to the relationship of art to reality and that its task is "to make the joining and breaking up of sociated individuals the exact reflection of the relations among these individuals."⁴ It "transfers the serious, often tragic character of these problems into symbolic play of its shadowy realm which knows no frictions, since shadows, being what they are, can not collide."⁵ It "is spared the frictions with reality by its merely formal relationship to it. Yet, because of this, it derives from reality, even to the mind of the more sensitive person, a significance and a symbolic, playful richness of life that are the greater, the more perfect [the play-form] is."⁶

Everett C. Hughes calls Simmel "the Freud of the study of society."⁷ One can

agree with him if Simmel's initial insight that certain group processes are most clearly reflected in symbolic forms of play proves as useful as Freud's conviction that unconscious motives of the individual are revealed in dreams and fantasies. Yet in one respect Simmel leaves off where Freud went to work. Aside from a few additional hints and several strikingly clear illustrations, he developed the matter no further. The necessary tools are lacking—a schema for analyzing the form of group process, a similar one for the sociological study of forms of play, and a set of guides for translating from one to the other. Nonetheless, from the few hints, I shall try to extend his exploration by setting up several guiding hypotheses I think can be investigated empirically.

The capacity to create, or to participate in, forms of play.—Simmel conceived of "thresholds" for play. If these are too low, and if they are crossed, the distinction between reality and play is dissolved; the dissociation which gives play its symbolic character breaks down. In effect, the audience shoots the villain; or, in Simmel's illustration, the reaction to coquetry destroys its artfulness:

As long as [the man] rejects [coquetry's] attractions or . . . is its mere victim [who] without any will of his own is dragged along by its vascillations between a half "yes" and a half "no," coquetry has not yet assumed for him a form that is commensurate with sociability. . . . It does not attain [this] until he asks for no more than this freely suspended play which only dimly reflects the erotically definitive as a remote symbol; [not] until he is no longer attracted by the lust for the erotic element or by fear of it.⁸

Presumably, the threshold is crossed when unresolved issues, associated with other contexts and aroused by symbols in play, control the person. Pressing needs obliterate the boundary between work and play, making the latter an occasion for acting on these needs.

⁸ Wolff, *op. cit.*, p. 51.

² *Ibid.*, p. 42.

³ *Ibid.*, p. 49.

⁴ *Ibid.*, p. 54.

⁵ *Ibid.*

⁶ *Ibid.*, p. 44.

⁷ Georg Simmel, *Conflict: The Web of Group Affiliations*, trans. Kurt Wolff and Reinhard Bendix (Glencoe, Ill.: Free Press, 1955).

Sociability [or play] also shies away from the entirely subjective and purely inwardly spheres of [one's] personality. Discretion which is the first condition of sociability in regard to one's behavior toward others is equally . . . required in regard to one's dealing with oneself: in both cases, its violation causes the sociological art form of sociability to degenerate into a sociological naturalism. . . . These thresholds are passed both when individuals interact from motives of objective content and purpose and when their entirely personal and subjective aspects make themselves felt.⁹

Groups, as well as individuals, have play thresholds. The first hypothesis is that the level of the threshold is a function of the group's capability for meeting external and internal demands satisfactorily (as defined by the standards of the group). As external demands and internal strains increase for a group, we can predict lower thresholds; eventually the unresolved issues will spill over, transforming play into problem-solving. And, as we move from one type of group to another—from the effective, versatile, well-organized group possessing a common basis for resolving new problems and meeting new demands to groups that are unsuccessful, rigid, emotionally distressing, and lacking the capacity for new solutions to problems as well as a means of working through differences—as we move along this continuum, we expect increasing difficulty both in dissociating work from play and in creating sociologically and psychologically significant forms of play. Just as an estimate of a person's sexual and social maturity is his artfulness at the game of coquetry, so an estimate of a group's resources is its ability to "leave" itself and play itself.

Symbolic mechanisms.—Another estimate of a group's resources may prove to be the kind of mechanism it employs in playing itself. By taking certain liberties in interpreting Simmel, I present four symbolic mechanisms which may explain connections between the ordinary processes of the group and its play-form. By taking still greater liberties, I arrange them in order—

from those likely to be employed by effective and integrated groups to those likely to be used by groups in a state of disintegration. Simmel's suggestion that these connections are similar to the relation of art to reality is instructive here. On the one hand, it encourages specification of the mechanisms, but, on the other, it warns against assuming that only a few exist. As a beginning, I offer the following four.

1. In its play-form the group re-creates characteristics of group processes which have been, and are, its principal sources of effectiveness, of gratification, and of pride. For example, in playing "house," children re-create family roles and relationships, boys' clubs establish an authority structure and give themselves surnames, and therapy groups in their "coffee hours" elect from their ranks a substitute leader as a source of security and of pride. Repeated in the game of charades is the sense of accomplishment in having overcome communication blocks, and repeated in the dance is the joint kinesthetic pleasure of man and woman moving together. By its mode of abstraction, this mechanism enables members to relive and enjoy the essentials of certain group processes and in this way elaborate their sources of enjoyment. Moreover, since the play-form itself is a new and separate part of group culture, reflecting certain essentials of the group, the mechanism provides a new cultural object of group identification. New pride can be taken in the play-form.

2. The group in its play-form recreates characteristics of group process which have been and are the principal sources of strain. Competition and its associated "wish to outdo . . . desire to wrest something from the other [and] . . . overpowering by ruse and revenge"¹⁰ are duplicated in warlike games in which power is represented by numbers, aristocratic rank, and combinations of these. Room is left for skill, ruse, and deceit, but aptitude in these respects does not alone determine

⁹ *Ibid.*, p. 47.

¹⁰ *Ibid.*, p. 49.

gain or loss. Restrictions on self-revelation and limits to frankness are duplicated in play-forms which place high value upon being able to assess what the other has in his "hand." Moreover, deviant numbers of the group are invited as playmates. Though this mechanism still maintains the boundary between group process and the play-form, it presents in disguised form the problematic issues of group life and presents them, in a sense, "out there," where they may be observed and possibly comprehended. In another sense, it presents them "here" to be participated in, experimented with, played with. One's role in respect to these processes may be rehearsed, revised, and reintegrated with his other functions in the group. It is analogous to the more self-conscious process of psychodrama,¹¹ and perhaps also to interaction in individual and group psychotherapy (though the issues in the three cases may be entirely different).

3. The group in its play-form adopts features which stand in contrast to its regular features. Ambiguity of norms is replaced by rigidly circumscribed patterns of play; there is a rule for every conceivable contingency. In other instances, reticence is replaced by obligations to be open and friendly. Prohibited exchanges of information become appropriate, and suppressed feelings may be expressed. As Simmel suggests, "A characteristically sociable . . . trait is the courtesy with which the strong and extraordinary individual not only makes himself equal to the weaker, but even acts as if the weaker were the more valuable and superior."¹² Though here again play-form is determined largely by sources of group strain, two important (and unique) functions are performed by the contrasting features: (a) tension may be expressed and reduced through acting upon the principles and patterns which represent the antithesis of the sources of high tension; but (b), perhaps even more

important, in creating and incorporating this inverse picture of itself, the group extends its culture to encompass not only what the group *is* but precisely that which it is *not*. Instead of observing, rehearsing, or experimenting with problematical issues, the group creates a fantasy. Yet, once created, acted on, and solidified into the play-form of the group, the fantasy becomes a part of group culture and, therefore, a component of the object of group identification. Compared to those above, this mechanism indicates that a group has more serious unresolved issues and a lower threshold for play, in Simmel's sense.

4. The group in its play-form negates the problematic processes of group life. Status tension is denied and replaced by the principle of equality—designated by number or other universalistic signs—and assured by systematic rotation, the game being incomplete until all players assume all possible positions. The deviant member is less likely to be invited to the party. In other instances, games of chance negate the part played by individual skill, technical accomplishment, and special abilities in determining outcomes in the regular group. In substituting logical, mathematical, or other related principles for social ones, this mechanism protects members from associating in any way elements of work with elements of play. At the same time it probably detracts from enjoyment by eliminating the reverberations of regular group process. Group issues are kept at great distance, just as distant as the erotic element in the Platonic relationship. The function of the mechanism is to defend a diminishing threshold.

The second hypothesis is that, as groups become less able to cope with their external and internal demands and as the threshold for dissociating group life from play becomes lower, there is a tendency for groups to shift from the first through to the fourth mechanism. Correspondingly, as demands are increased in proportion to the capacity of a given group, a similar shift occurs.

¹¹ J. L. Moreno, *Who Shall Survive?* (rev. ed.; New York: Beacon House, 1953).

¹² Wolff, *op. cit.*, p. 49.

Empirical tests.—A group's capability for meeting internal and external demands is the independent variable in the two hypotheses. It may be varied systematically by screening, matching, and differentially classifying groups according to their demonstrated performance in response to tasks. Demands may be increased within a given set of groups by presenting them with tasks and situations, graduated in their difficulty and in their emotional involvement. Moreover, demands may be concentrated in specific areas of group process.

Ease and difficulty in communication, for example, can most easily be varied by using the Bavelas baffle board employed by Leavitt;¹³ success and failure in response to external demands may be varied by reports from the experimenter;¹⁴ problems regarding authority relations and inter-member relations may be induced by trained, role-playing leaders and members;¹⁵ finally, by selection according to basic interpersonal reflexes, values, personality needs, or libidinal ties, other demands may be induced and varied systematically. These are but a few available techniques for establishing degrees of strain in specified areas of group process.

The dependent variables in the hypotheses are the play threshold and the mechanisms. A variety of play situations may aid in developing adequate measures of these variables: groups may be asked (*a*) to create a game which they then play; (*b*) to select a game to play; (*c*) to re-enact a drama in which the issues either correspond to, negate, or represent a reversal of issues in their working group; (*d*) to exchange

roles in re-enacting their own process; or, finally, (*e*) to create group fantasies.¹⁶ Estimates of resistance to play are needed, and further progress is required in our analysis of the organizing principles of games,¹⁷ of expressive behavior in role-play, and, in the last case, of the sociological facets of fantasy material.¹⁸

Strategically, following up these notions of Simmel's promises at least two contributions. First, play-form reflects not only the existence of certain issues but, as a product of the whole group—a product that has been created or accepted through a process of give and take, rejection, selection, and amalgamation—it reflects the relative importance of issues. As a group product, it represents a balance of group forces—a kind of balance that is difficult to deduce from answers to an array of discrete questions. According to Simmel, it “may more completely, constantly and realistically reveal the deepest nature of [group] reality than could any attempt at grasping it more directly.”¹⁹ Second, a fuller understanding of the symbolic connections among play-form, fantasy, etc., and group process may strengthen the role of leaders of therapy, training, and discussion groups in assessing, not personal motivation, but the salience of issues confronting the group as a whole.

COMPARATIVE DYNAMICS OF DYADS AND TRIADS

In the translated works of Simmel there are over sixteen propositions contrasting the dynamics of small, undifferentiated systems with larger, differentiated ones. Though he fails, often, to specify the

¹³ H. J. Leavitt, “Some Effects of Certain Communication Patterns on Group Performance,” *Journal of Abnormal and Social Psychology*, XLVI (1951), 38–50.

¹⁴ J. W. Thibaut, “An Experimental Study of the Cohesiveness of Underprivileged Groups,” *Human Relations*, III (1950), 251–78.

¹⁵ A. Paul Hare, Edgar F. Borgatta, and Robert F. Bales (eds.), *Small Groups* (New York: A. A. Knopf, 1955), pp. 516–23.

¹⁶ W. E. Henry and H. Guetzkow, “Group Projection Sketches for the Study of Small Groups,” *Journal of Social Psychology*, XXXIII (1951), 77–102.

¹⁷ See Hare, Borgatta, and Bales (eds.), *op. cit.*, pp. 35–37, for a résumé of an analysis of poker play.

¹⁸ T. M. Mills, “Sign Process Analysis” (unpublished Ph.D. thesis, Harvard University, 1952).

¹⁹ Wolff, *op. cit.*, p. 56.

boundaries between the two types, he employs throughout his discussion the abstract models of the two-element and three-element systems. For the purpose of this section I apply his propositions solely to the two- and three-person groups and present several hypotheses which, in my interpretation, summarize the chief line of his argument.²⁰

Concerning dyads and triads, he suggests: (1) that, while an impending sense of death of the relationship permeates the two-person group, a sense of permanency exists in the triad;²¹ (2) that dyads lack but triads possess an object which represents to its members the relationship as a whole or the collectivity;²² (3) that in the dyad affection may culminate in intimacy but that in the three-person group it tends to be either checked or restricted to a subpart;²³ (4) that delegation of functions on universalistic criteria cannot be easily accomplished in the two-part group, as it can in the triad;²⁴ (5) that scapegoating disintegrates the dyad, while it may serve temporarily as a rebuilding expedient in the triad;²⁵ and (6) that the assumption on the part of one member of a supraindividual role, representing the relationship itself, expells a single partner, but it need not in the case of two partners.²⁶

²⁰ For studies on the effect of group size see Hare, Borgatta, and Bales (eds.), *op. cit.*, pp. 665-66, for a list of references; also Robert F. Bales, A. Paul Hare, and Edgar Borgatta in Joseph Gittler (ed.), *Review of Sociology* (New York: John Wiley & Sons, 1957), chap. xii; and Philip E. Slater, "Contrasting Correlates of Group Size," to be published in *Sociometry*. For studies of three-person groups see Theodore M. Mills, "Power Relations in Three-Person Groups," in Dorwin Cartwright and Alvin Zander (eds.), *Group Dynamics* (Evanston, Ill.: Row-Peterson, 1953), pp. 428-42; and other references listed by W. E. Vinacke and A. Arkoff, "An Experimental Study of Coalitions in the Triad," *American Sociological Review*, XXII (1957), 406-14.

²¹ Wolff, *op. cit.*, p. 124.

²² *Ibid.*, p. 123.

²³ *Ibid.*, p. 126.

²⁴ *Ibid.*, p. 134.

²⁵ *Ibid.*, p. 152.

²⁶ *Ibid.*, p. 36.

Concerning small and large systems, Simmel suggests: (1) that small systems tend "to burn up all their energy," while larger ones maintain residual strength;²⁷ (2) that, while decisive stands must be taken in smaller systems, in larger ones the group as a whole may suspend many;²⁸ (3) that norms cover a wider range of behavior in small than in large systems but incorporate a broader range of tolerance; (4) that small, undifferentiated groups contain no subpart which mediates between the individual and the collectivity;²⁹ (5) that stress in the small system results either in solidification or in collapse, while it tends to reduce the larger system into two-part systems;³⁰ (6) that small groups possess means for headling complex conflicts between persons, while larger ones, lacking this faculty, are more capable of controlling conflicts between organized subunits;³¹ (7) that in small systems reintegrating mechanisms are limited chiefly either to keeping faith until positive forces reappear or to altering the norms, as illustrated by the comment, "We no longer talk about that," whereas larger systems can reorganize so as to minimize conflict and maintain a constant boundary in respect to its external situation;³² (8) that, on the reciprocity assumption, one party's cathexes (and related affective attributes such as reticence or self-revelation, faithful or unfaithfulness, gratitude or ingratitude, frankness or deceit) largely determine the nature of the undifferentiated system, whereas in differentiated ones, contrary affective relationships may not only exist side by side but also share in a common conception of the collectivity;³³ and, finally, to interpolate from Simmel, (9) that, since it is easier to elicit certain behavior

²⁷ *Ibid.*, p. 92.

²⁸ *Ibid.*

²⁹ *Ibid.*, p. 97.

³⁰ Simmel, *op. cit.*, p. 65.

³¹ *Ibid.*, p. 67.

³² *Ibid.*, p. 67.

³³ *Ibid.*, p. 67.

from one party to one's self than to induce certain behavior between other parties, group process is less determined by a single party's needs in the larger system than in the smaller, undifferentiated one.

Let me not only subsume all these propositions under models of the dyad and the triad but also extract, with no intention of exhausting Simmel's argument, a major theme to serve as a guide in our comparative study. The hypothesis is that over a wide array of tasks and situations, and with increasing pressure from external demands, the two-person group tends to readjust at a lower level of integration than the three-person group.

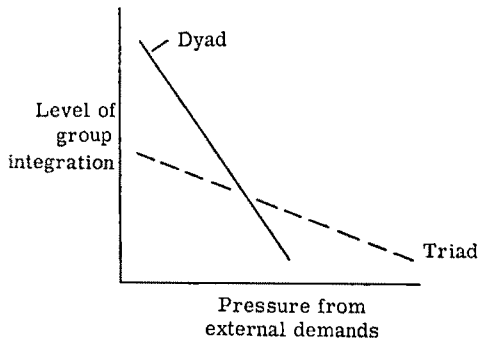


FIG. 1

Initial qualifications are necessary. If we take as a working hypothesis Bales's proposition that an inverse relationship exists between (a) the expenditure of resources upon adaptive and goal attainment problems and (b) internally disorganizing tendencies,³⁴ we should expect both two- and three-person groups to exhibit negatively sloped curves, where degree of external pressure is represented along the x axis and level of group integration along the y axis. The first qualification is that the negative slope of the curve for two-person groups is greater than the negative slope of the three-person groups, and, second, at some undetermined point they intersect.

³⁴ Robert F. Bales, *Interaction Process Analysis* (Cambridge, Mass.: Addison-Wesley Press, 1950).

The dyad, lacking the variety of reintegrating mechanisms, tends to decompose more readily in response to increasing demands than the triad; yet, as external demands diminish, the dyad, with its capacity for intimacy, etc., is capable of attaining a higher level of integration than the three-person group. These curves are roughly sketched in Figure 1.

In spite of the limitations of our measures of group integration, the careful modern experimenter might test the hypothesis, particularly if he induces emphatically different degrees of external pressure.

Changes in membership.—Simmel, with characteristic perspicacity, warns against the simple conclusion that all pairs or all threesomes function in the same way. Under some conditions the addition of a new member solidifies the dyad (as a child often unites parents), while in other circumstances it separates them (as illustrated in the warning, "If you want to lose your friend, introduce him to your sister"). Still, under other conditions, as observed by Spiegel, the newcomer may be used, in his relations to the others, to absorb the unresolved issues of the original pair.³⁵ Simmel illustrates these processes but fails to specify the conditions under which we might expect one pattern or another. Let me suggest two experimental procedures which may contribute to a more useful formulation of the dynamics of pairs and threesomes.

Properties of the dyad may be assessed by comparing the way it absorbs a newcomer with assimilation processes in groups of three or more.³⁶ Moreover, newcomers may be introduced into a series of differentially composed pairs (selection being on the basis of faithfulness, gratitude, conflict, reticence, or deceit, as Simmel might propose, or on the basis of sex, status, complemen-

³⁵ John Spiegel, "The Resolution of Role-Conflict in the Family," *Psychiatry*, XX (1957), 1-16.

³⁶ See Theodore M. Mills *et al.*, *Group Structure and the Newcomer* (Oslo: Oslo University Press, 1957), for an experimental study of the expansion of the triad.

tarity of needs, values, or interpersonal anxieties and defenses). How do attributes of pairs affect the dynamics of taking in a new member? What sorts of pairs tend to unite, tend to separate, tend to transfer conflict into relations with the newcomer? Do affective, behavioral, and cognitive aspects of the original dyad change together, or do they change differentially? Moreover, in respect to the hypothesis above, is there the expected increase in variety and effectiveness of integrative mechanisms? How does integrative capacity compare between variously composed original pairs? Finally, under increasing external pressure, how do the curves for these groups compare with the curve for three-person groups mentioned above?

Though not so fully discussed by Simmel, the expedient of taking away a member may help resolve one issue involved in the central hypothesis; namely, does the supraindividual component, mentioned by Simmel, depend strictly and unconditionally upon the existence of three parties? Is the integrative faculty of the threesome lost upon the withdrawal of a member, or is it somehow retained? Does a system, which possesses faculties for handling conflict, and whose members have experienced and participated in successful reintegration, automatically lose this ability when the necessary element for the creation of the faculties disappears? Or do members somehow operate as if the third element were present, thereby retaining its function as part of the culture of the pair? To what extent and under what conditions is there irreversibility of these processes? Does number determine the matter, or does the life-history of the group determine it?

Moreover, upon the departure of a member, we may ask how the organization of the initial triad affects the relationship between the two who remain. How do the affective, behavioral and cognitive aspects of their relationship change? In what sorts of triads is integrative capacity lost, in what sorts is it retained? Finally, under increasing external demands, how do the

curves of these remaining pairs compare with those which have not had a third member? Control groups for this project would consist simply of dyads and triads in which no change in membership occurs. In conclusion, it should be noted that the simple expedient of adding and subtracting members to clarify the dynamics of very small groups amounts to an important extension of Simmel's treatment, for he works to a great extent on the implicit assumption that groups increase but do not decrease in size—an assumption which probably biased his appreciation of the role of numbers in the process and organization of groups.

COMMENT

It is likely that the results of modern investigation of these matters will have been anticipated by Simmel, for it is his habit, after posing a concept or a proposition, to illustrate its feasibility with one example, its limitations with another, its general application with still others, and its antithesis by a final example, until it is fully circumscribed. As Hughes notes, he is tentative.³⁷ Habitually, he anticipates both negative and positive results. The seductive power of this habit of learning and of teaching is attested to by his early influence on American sociology. Yet students of science have come to recognize that a proposition, even though artfully suspended by positive and negative illustrations, is a long way from one about which one can make a probability statement. The task—the laborious task, indeed—of arriving at these probabilities Simmel leaves to his students.

Though the purpose of this paper is to suggest several steps that might be taken in this direction, certain of Simmel's propositions are selected, rather than others, because they are of strategic importance to the development of social science. Within immediate social relations he observed configurations, and changes in configurations, of libidinal, communicative, and power ties

³⁷ Simmel, *op. cit.*, p. 8.

SUICIDE, HOMICIDE, AND THE SOCIALIZATION OF AGGRESSION¹

MARTIN GOLD

ABSTRACT

While emphasizing the separation from psychology, Durkheim believed that sociological explanation could be facilitated by a knowledge of psychological variables. This viewpoint is brought to bear on some of the theory and data of Henry and Short's *Suicide and Homicide*. After presentation of a theory relating sociological variables to the socialization of aggression, hypotheses are derived similar to those of Henry and Short. These hypotheses are tested, employing an index of preference for suicide or homicide, the Suicide-Murder Ratio (SMR). Consideration of the socialization process as it is related to the American class structure resolves some issues raised by Henry and Short's theory of external restraint and effectively predicts the findings. The results are considered in terms of Durkheim's belief that the choice of suicide or homicide resulting from anomie is purely a psychological problem and from the standpoint of Henry and Short's assertion that the choice is in part determined by sociological variables.

No one has contributed more significantly to the establishment of sociology as a separate discipline than Émile Durkheim, and nowhere did he make this separation more secure than in *Suicide*. Then will an article frankly "social-psychological" in orientation, which argues from suicide-rate data, seem incongruous in a *Journal* issue dedicated to Durkheim's memory? Durkheim would not have found it so, for our approach is one he advised.

While Durkheim saw the need to emphasize the separation of sociology from other sciences, especially psychology, we do not think he intended to isolate it. In *The Rules of Sociological Method* he specifically denies that there are no links between sociology and psychology.

We do not mean to say, of course, that the study of psychological facts is not indispensable to the sociologist. If collective life is not derived from individual life, the two are nevertheless closely related; if the latter cannot explain the former, it can at least facilitate its explanation. First, as we have shown, it is indisputable that social facts are produced by action on psychological factors. In addition, this very action is similar to that which takes place in each individual consciousness and by which are transformed the primary elements.²

¹ The author wishes to thank D. R. Miller and G. E. Swanson for their aid during the research and on the manuscript. He is also grateful to J. F. Short for his helpful comments.

Granting the divisions among levels of generality or abstraction, each with its own reality, laws at any level of abstraction are formulated with proper regard for other levels. Just as psychological laws of perception must be consistent with what is known about neurophysiology or must bear the burden of inconsistency, so sociological laws must face up to current knowledge of psychological processes. For this reason findings at one level are clues at the other.

Our purpose is to explore the relationship between certain psychological and sociological theories and between relevant data from both disciplines which pertain to the choice of suicide or homicide as an expression of aggression. We will try to show that the choice of suicide or homicide, essentially a psychological problem, is determined in part by the individual's place in a social system. We will focus on socialization as the process by which sociological factors are translated into determinants of a psychological choice between directions of aggression.

Our research gains impetus from the work of the late Andrew F. Henry and James F. Short, reported in their book, *Suicide and Homicide*.³ A discussion of the

² Émile Durkheim, *The Rules of Sociological Method*, trans. S. A. Solvay and J. K. Mueller, ed. G. E. G. Catlin (Glencoe, Ill.: Free Press, 1950), p. 11.

³ A. F. Henry and J. F. Short, Jr., *Suicide and Homicide* (Glencoe, Ill.: Free Press, 1954).

sociological and psychological factors leading to these two ultimate forms of aggression is only one of the several problems they discuss, but, it is the one we pursue here. This paper is not intended as a critical review of their book. Rather it is a report of research which attempts to build upon and amplify a portion of the theoretical structure they presented.

We have two specific aims: one, primarily theoretical; the other, methodological.

First, Henry's and Short's theory will be examined from a social-psychological point of view. Where they have dealt separately with psychological and sociological antecedents of suicide and homicide, we will suggest some child-rearing links which mediate between social structural variables and intrapersonal determinants of behavior. Second, we will examine the way in which Henry and Short tested their hypotheses about the choice of suicide or homicide. It seems to us that a more appropriate methodology is needed, and we will suggest a possible alternative. Finally, we will use the suggested methodology to test hypotheses generated by Henry and Short and by the theory of socialization presented here. The findings will be compared.

THEORY OF EXTERNAL RESTRAINT

Henry and Short are interested in suicide and homicide as acts of aggression which originate in frustration. They theorize that degree of external restraint distinguishes individuals who choose to commit one rather than the other. An individual is externally restrained to the degree that his alternatives of behavior are limited by others. It is postulated that, the more an individual is externally restrained, the more likely it is that he will regard others as legitimate targets for aggression. Hence, the greater the degree of external restraint upon an individual, the more likely that he will commit homicide rather than suicide.

It is assumed that individuals in higher-status categories, as indicated by four of Parsons' criteria, are less externally re-

strained than those in lower-status categories and are therefore more likely to prefer suicide to homicide. The criteria are achievement, possession, authority, and power. The authors specify the following high- and low-status segments of the American population:

High Status	Low Status
Males	Females
White	Non-white
Aged 25-34	Aged 65 or more
Army officers	Enlisted men

They hypothesize that, given frustration, members of low status are more likely than members of high-status categories to commit homicide rather than suicide. Further, they follow Durkheim in assuming that individuals involved in more intimate social relationships are more externally restrained. Hence, married people and rural dwellers, who are more subject to external restraint, are therefore more likely to prefer homicide to suicide than single or divorced people and urbanites.

A number of Henry and Short's assumptions may be questioned. It is debatable that members of higher-status categories are less restrained externally than their lower-status counterparts. For example, the behavior appropriate for an "officer and gentleman" is in many respects more limited than that allowed an enlisted man. Drunkenness off the base, for example, is apt to earn the enlisted man mild reproof but to invoke strong penalties on an officer. Similarly, eccentricities tolerated in persons over sixty-five may result in institutionalization of a twenty-five year-old. External restraints on behavior are exerted not only by persons but also by norms—norms which may apply more stringently to persons in higher-status positions. Rather than arguing directly from status positions, let us consider other sources of behavioral restraints, specifically, limits imposed on expressions of aggression.

What are the interpersonal events through which restraints over aggression are

made manifest? What are the processes by which aggression is displaced from the restraining figures to other targets? Why the choice of the *self* as a legitimate target for aggression in the absence of any other legitimate target? In short, what are the social-psychological variables mediating between the sociological conditions and the psychological event?

THE SOCIALIZATION OF AGGRESSION

A body of theory exists which helps to link sociological variables with preferences for self or others as targets for aggression. A brief presentation of the theory and some supporting data will lead us to hypotheses about preferences for suicide or homicide similar to Henry and Short's.

One of the early lessons a child must learn, if he is to continue to live among others, is to control his rages. Sigmund Freud recognized the importance of hate affect as well as love in the developing personality and marked the ego's mastery of these affects as a critical point in personality development.

An individual may control his aggression in many ways. Miller and Swanson order the modes of control in their theory of defenses.⁴ An impulse, like the wish to destroy, is taken as an *action-tendency* which has four components: intended act, agent or actor, target object, and affect. Control can be established by manipulation of one or several of these components.

For example, the *intention* to destroy an object may be modulated into tongue-lashing. The aggressive *agent* may be distorted, as in projection: "He wants to destroy me; I don't want to hurt him." The impulse may be displaced to another *object* such as a socially acceptable scapegoat. The *affect* may be shifted, dislike displacing hate, or it may be distorted completely through the working of a reaction formation, hate becoming love. The action-tendency as a whole may

be postponed temporarily or frustrated indefinitely.

Which mode or modes of control are selected depends to a great extent on the culture in which the individual participates. Among the Sioux, for example, an infant's tantrums were a matter of pride to his parents, and he was hurt and frustrated as a child to encourage his rage. Rages were later controlled by venting them against extratribal enemies in forays which promised social rewards.⁵ Among the Alorese, on the other hand, aggression is suppressed at an early age and later finds expression in intratribal stealing.⁶

There is evidence, too, that modes of control and expression of aggression vary among the social classes in the United States. B. Allinsmith found that the TAT protocols of lower-class adolescent boys were more apt to include direct references to and direct expressions of aggression than those of middle-class boys.⁷ While lower-class boys told stories of attacking or fleeing from authority, middle-class boys either told stories devoid of hate or stories in which aggression was turned against themselves.

B. J. Beardslee aroused the anger of lower- and middle-class boys halfway through a set of story-completion projectives.⁸ The middle-class boys showed the greater increase in the use of defenses against aggression from the pre- to the post-arousal story endings.

How are these differences in controls of

⁵ E. H. Erikson, "Observations on Sioux Education," *Journal of Psychology*, VII (1937), 101-56.

⁶ A. Kardiner, *Psychological Frontiers of Society* (New York: Columbia University Press, 1945).

⁷ B. B. Allinsmith, "Parental Discipline and Children's Aggression in Two Social Classes" (unpublished Ph.D. dissertation, University of Michigan, Ann Arbor, 1954). Summarized in D. R. Miller and G. E. Swanson, "The Study of Conflict," *Nebraska Symposium on Motivation* (Lincoln, Neb.: University of Nebraska Press, 1956).

⁸ "The Learning of Two Mechanisms of Defense" (unpublished Ph.D. dissertation, University of Michigan, Ann Arbor, 1955). Summarized in Miller and Swanson, "The Study of Conflict," *op. cit.*

⁴ D. R. Miller and G. E. Swanson, *Inner Conflict and Defense in the Child* (New York: Henry Holt & Co., 1958).

aggression between the social classes established? Allinsmith suggests that one cause is the type of punishment meted out by parents to misbehaving children.⁹ Lower-class mothers report that they or their husbands are likely to strike their children or threaten to strike them. Middle-class mothers report that their type of punishment is psychological rather than physical. Middle-class parents are more apt to say to a naughty son, "After all I've done for you . . .," or, "You ought to be *ashamed*. We don't do that sort of thing."

Allinsmith reports that type of punishment is related to boys' TAT protocols; boys who are punished physically express aggression more directly than those who are punished psychologically. She suggests that type of punishment operates in two ways to generate this relationship. First, physical punishment clearly identifies the punisher. A son can see plainly who controls the flailing arm. The relationship between parent and child is, for the moment, that of attacker and attacked. Psychological punishment creates a more subtle relationship. It is often difficult for the son to tell where his hurt feelings are coming from. Their source is more likely to seem inside him than outside. If there is to be a target for aggression then, the physically punished child, who is more likely to be lower-class, has an external target readily available; the psychologically punished child does not have such a ready target. If he selects one, it is likely to be himself.

Second, the type of punishment a parent administers identifies for the child the approved behavior when one is hurt or angry. The punishing parent serves as a model whom the child imitates and whose behavior instructs the moral conscience—the super-ego.

Why is it that lower-class parents are more likely to employ physical punishment and middle-class parents psychological punishment? McNeil has gathered data which suggest that lower-class Americans gener-

ally express themselves physically, while members of the middle class express themselves conceptually.¹⁰ He found that lower-class adolescent boys are more spontaneous and expansive in their bodily expression of emotions in a game of statues, while their middle-class peers are more facile at the symbolic task of creating abstract drawings of emotions. He interprets these results as a reflection of the values and skills dominant in the two social classes. Lower-class boys are identifying with fathers who work with their bodies; middle-class boys are identifying with fathers who work with their heads.

Beardslee lends further support to the notion that children's behavior reflects these dominant class values.¹¹ She finds, as others have, that middle-class boys are apt to do better on tests of verbal intelligence than lower-class boys.

Selective factors are likely to be at work here. Since a great deal of social mobility in modern America is achieved in schools, where verbal ability is a core skill, boys who have such ability have a better chance of becoming middle-class adults. Degree of verbal facility is likely to affect modes of expression, such as the parents' expression of disapproval of the misbehavior of their children.

In an epidemiological study of psychopathology, Faris found a greater incidence of catatonia—a psychosis marked by inhibition of voluntary muscular movement—in the slum sections of cities, where manual workers are more concentrated.¹² But manic-depressive psychosis is not related to ecological areas. These findings support the statement that expression of and defenses against expression of emotion are more apt

⁹ E. B. McNeil, "Conceptual and Motoric Expressiveness in Two Social Classes" (unpublished Ph.D. dissertation, University of Michigan, Ann Arbor, 1953). Summarized in Miller and Swanson, "The Study of Conflict," *op. cit.*

¹¹ Beardslee, *op. cit.*

¹² R. E. L. Faris, "Ecological Factors in Human Behavior," in J. McV. Hunt (ed.), *Personality and the Behavioral Disorders* (New York: Ronald Press Co., 1944), II, 736-57.

⁹ Allinsmith, *op. cit.*

to involve the physical apparatuses in the lower class.

Several factors converge in the relationship between social class and modes of aggressive expression. We have already seen how differential skills and occupations may enter into this relationship. It may also be that class ideologies concerning interpersonal relations differ. In the bureaucratic middle class, stress may be laid on "getting along" with others, for economic success rests heavily on the development of harmonious social relations. In this context direct expression of aggression becomes a disruptive force.

But, in the working class, direct aggression is not so dysfunctional. If we assume less interdependency among people and less need for harmony in social relations, the forces against expressing aggression are not so strong.

In this framework, type of punishment becomes an index to the values and skills of a category. As such, it may serve, along with social class, as a predictive variable.

Let us return now to our concern with the preference for suicide or homicide as the mode of aggressive expression. The theory presented above generates predictions similar to Henry and Short's about the kinds of people who are likely to turn aggression outward compared to those who will turn it inward. The predictions are based on the assumption that type of punishment is both an index to and a factor in shaping values concerning expression of aggression. Physical punishment leads to outward expression, while children punished psychologically should turn their aggression against themselves. The derivations below make use of the relationship found between type of punishment and social class.

Of the seven comparisons made by Henry and Short, the theory of socialization of aggression makes predictions in six. There is no prediction here for the married-unmarried comparison.

Since non-whites are heavily concentrated in the working class, the theory of socialization of aggression offers the hypothesis that

non-whites should show a greater preference for homicide than whites. If we accept the common assumption that army officers are recruited from the middle class and that enlisted men are more likely to be working class, especially in times of peace, we can hypothesize that army officers are more likely than enlisted men to commit suicide.

Comparing rural to urban populations, it seems safe to assume that the proportion of urban people who work in bureaucratic settings should be greater than the rural proportion. If the previous argument about the source of physical and conceptual values and modes of expression is correct, urbanites should have the greater preference for suicide.

There is evidence that in America boys are more apt to be punished physically than are girls, regardless of race or social class.¹³ Therefore, females should have a greater preference for suicide than males.

Were we able to compare the childhood punishments administered to people now over sixty-five with the type borne by people now between twenty-five and thirty-four, we could predict to their preferred expression of aggression. Such data might be obtained by interviewing members of these two age categories or by content-analyzing the child-rearing literature their parents read. Unfortunately, data needed to test this prediction are not available. Similarly, stratified data are not available to test the prediction that middle-class people are more likely than working-class people to destroy themselves.

When we compare the hypotheses based on the socialization theory with those based on external restraint, we find that three of the four to be tested are identical. That is, both theories predict that greater preference for suicide should occur among whites, army

¹³ E. Douvan and S. Withey, *A Study of Adolescent Boys* (Ann Arbor: Institute for Social Research, 1955); and E. Douvan, C. Kaye, and S. Withey, *A Study of Adolescent Girls* (Ann Arbor: Institute for Social Research, 1956). I am grateful to these authors for making these data available to me.

officers, and urbanites. On the other hand, Henry and Short expect men to show a greater preference for suicide, since they are more externally restrained, while we predict women would. For women's childhood experiences are more apt than men's to be of psychological rather than physical punishment, indicative of an ideology of appropriate behavior expected from and to women.

METHODOLOGY

Before we go about testing our hypotheses, let us examine them a little more closely. We think that careful consideration of what we mean by "choice of" or "preference for" suicide or homicide suggests a more appropriate way of handling the data than Henry and Short employed.

In part I of *Suicide and Homicide* the authors try to establish that the business cycle is a common source of frustration for all segments of the population. But they find that suicide rates do not decrease uniformly in all segments of the population in prosperous times. In addition, they report that homicide rates increase during prosperity. But, if homicidal aggression is an index of frustration, this suggests that prosperity may in part be frustrating. The authors explain that lower-status categories are relatively more deprived and frustrated by prosperity, since they gain less, relative to higher-status categories.

At this point Henry and Short state their crucial hypothesis: People in higher-status categories are more likely to prefer suicide to homicide; people in lower-status categories, homicide to suicide. If this hypothesis is confirmed, then higher homicide rates during prosperity and higher suicide rates during depression are explained. Part I of their work takes this hypothesis as an important assumption.

In Part II the authors test this hypothesis. They raise the question: "Why does one person react to frustration by turning the resultant aggression against someone else, while another person reacts to frustration by turning the resultant aggression against himself?"¹⁴ They take upon themselves the

responsibility of proving that members of higher-status categories are more apt to react to frustration by committing suicide and members of lower-status categories by committing homicide.

To support this, they offer absolute rates of suicide and homicide for specific years or series of years. These data show that the suicide rates presented are higher in most of the higher-status categories and that the homicide rates presented are higher in most of the lower-status categories. But it appears that Henry and Short may not really prove their point with these data.

If Henry and Short wish to demonstrate that members of higher-status categories have a greater preference for suicide than members of lower-status categories, it is not enough to demonstrate a greater suicide rate for higher-status categories. This may only indicate that they are more frustrated and hence more aggressive in general. For example, the authors' work on business cycles indicates that members of higher-status categories may at any one time be more frustrated than those in lower. If this is true, they may commit more homicide as well. Nor is it enough to demonstrate that within the higher-status category the suicide rate is higher than the homicide rate. This does tell us that higher-status citizens prefer suicide to homicide certainly, but it does not show that the preference in this category is any greater than the preference in the lower-status category where the suicide rate may also be higher than the homicide rate.

To illustrate this point, we may consider data in chapters v and vi of *Suicide and Homicide*. The writers first present evidence that the male suicide rate is higher than the female rate and so find support for their hypothesis that the higher-status category prefers aggression against self more than the lower. In chapter vii they show that the male homicide rate is also higher than the female. We might conclude from these data only that men are either more frustrated than women or more given to both these ultimate forms of violence.

¹⁴ Henry and Short, *op. cit.*, p. 65.

In order to demonstrate a preference on the part of a population category, it is necessary to take their total amount of suicide and homicide into account. To get an index of preference for suicide over homicide, we can divide the suicide rate by the sum of suicide rate and the comparable homicide rate: (Suicide rate/Suicide rate+Homicide rate). We may call this the Suicide-Murder Ratio, or SMR.¹⁵ Comparing the SMR of one category with the SMR of another, the larger ratio demonstrates the greater preference for suicide.

Previously. We agree with Henry and Short that whites, urbanites, and army officers will demonstrate a greater preference for suicide—have a higher SMR—than non-whites, rural dwellers, and enlisted men. But, predicting to sex differences, Henry and Short think that males should have the greater preference for suicide, while the present author expects that females should.

In order to compute an SMR for a category, it is necessary to have data on the suicide rate and the homicide rate for the same population and for the same time

TABLE 1*

SUICIDE-MURDER RATIOS OF WHITES AND NON-WHITES, 1930-40

YEAR	SUICIDE RATE		HOMICIDE RATE†		SMR‡	
	White	Non-White	White	Non-White	White	Non-White
1930.....	18.0	5.9	5.9	39.5	75.3	13.0
1931.....	19.2	6.0	6.0	41.2	76.2	12.7
1932.....	19.7	6.8	5.9	40.0	77.0	14.5
1933.....	18.0	6.1	6.1	44.6	74.7	12.0
1934.....	16.6	5.9	5.7	46.5	74.4	11.3
1935.....	15.8	5.6	4.9	40.9	76.3	12.0
1936.....	15.7	5.2	4.5	41.4	77.7	11.2
1937.....	16.3	5.5	4.3	38.6	79.1	12.5
1938.....	16.4	5.5	3.8	34.9	81.1	13.6
1939.....	15.1	4.7	3.3	35.0	82.1	11.8
1940.....	15.2	5.1	3.2	34.2	82.6	13.0

* Source: United States Department of Health, Education, and Welfare, *Vital Statistics*, XXX, 467; XXXI, 483.

† Adjusted by age.

‡ All differences within years between white and non-white SMR's are significant beyond .0001 (two-tailed).

Whether or not this mode of data analysis yields results different from those obtained by Henry and Short remains to be seen. In any case, it appears to have two advantages. First, it seems a surer way to establish preference for suicide or homicide. Second, it enables us to test whether a difference in preference between categories is statistically significant. For SMR's are proportions, and, assuming an infinite population, tests of significance of differences between proportions may be applied to them.

RESULTS

At this point we will apply the suggested methodology to the hypotheses advanced

¹⁵ We had originally thought of calling this the Suicide-Homicide Ratio, but "SHR" is immortally Clark Hull's. Another SHR would only cause confusion.

period. It is not too difficult to get data on the number of suicides and the size of the population, so that a suicide rate can be computed. But data on homicides committed are not so easy to come by.

According to Henry and Short, "Cause of death by homicide statistics provide our most reliable comparison of homicide rates of whites and Negroes [since] the overwhelming majority of murders are committed by members of the same race as the person murdered."¹⁶ The same type of data is used here for the racial comparison. Similarly, on the assumption that most urban murders are committed by urbanites, and most rural murders by rural dwellers, cause of death by homicide statistics are used in the rural-urban comparison also.

Table 1 presents the data comparing the

¹⁶ Henry and Short, *op. cit.*, p. 82.

preferences of white and non-whites. Supporting our predictions, it demonstrates that, in every year from 1930 to 1940, whites clearly chose suicide over homicide more often than did non-whites.

Table 2 presents the data on the preferences of the urban compared to the rural population, controlling on race. The prediction that urbanites have the greater preference for suicide is confirmed. Although the

TABLE 2*

SUICIDE-MURDER RATIOS FOR URBAN AND RURAL RESIDENTS, 1930, 1932, AND 1933, FOR WHITES AND NON-WHITES

	Suicide Rate	Homicide Rate	SMR†
1930:			
White			
Urban.....	19.8	6.2	76.2
Rural.....	14.1	4.9	74.2
Non-white			
Urban.....	8.0	56.4	12.4
Rural.....	2.9	23.8	12.2
1932:			
White			
Urban.....	21.3	6.0	78.0
Rural.....	16.2	5.4	75.0
Non-white			
Urban.....	8.6	56.7	13.2
Rural.....	3.5	24.6	12.4
1933:			
White			
Urban.....	19.8	6.2	76.2
Rural.....	15.0	5.7	72.5
Non-white			
Urban.....	7.4	57.7	11.4
Rural.....	3.2	26.5	10.8

* Source: United States Bureau of the Census, *Mortality Statistics* for the years 1930, 1932, and 1933.

† Differences between SMR's within the white category are all significant beyond .01 (two-tailed). Differences within the non-white category are not significant (chance probability greater than .10 [two-tailed]).

differences in SMR's are not large, they are consistent over the three years we examined and within both race categories. It should be noted that presentation of the homicide rates alone would not have supported the hypothesis. According to the reasoning in *Suicide and Homicide*, the higher absolute homicide rates of the urban population would lead us to conclude that urban residents prefer homicide more than rural residents.

To compare army officers with enlisted men, it was necessary to use number of convictions for homicide as an estimate of

the homicide rate. Inasmuch as not all murderers are caught, our data provide us with an approximation. Further, since the figures on convictions for homicide are low in the armed forces (no officers were convicted for this offense in the year for which data were available), data on convictions for assaults against persons are also included. So the homicide rate here is, strictly speaking, an index of "violence against others."

Table 3 reveals that army officers have a greater preference for aggression against the self than do enlisted men. This is as predicted. Note that the data on the suicide rates alone do not reveal the true magnitude of the difference in preferences between these two categories. But, when the suicide and the homicide (assault) rates are combined in the SMR's, a large difference emerges.

TABLE 3*

SUICIDE-MURDER RATIOS OF ARMY OFFICERS AND ENLISTED MEN
JUNE, 1919—JUNE, 1920

Category	Suicide Rate	Homicide Rate†	SMR‡
Officers.....	5.9	7.1	45.4
Enlisted men.....	5.4	22.7	19.2

* Source: "Reports of the Adjutant General, Judge Advocate General, and the Surgeon General" *Annual Report of the Secretary of War, 1920* (Washington, D.C., 1921).

† Includes assaults on persons.

‡ Significance of difference in SMR's beyond .001 (two-tailed).

To test differences between preferences of males and females for one form of aggression or another, the homicide rate is computed on the basis of convictions for homicide. Reasoning in terms of external restraint, Henry and Short expect that men will show the greater preference for suicide. The present author, taking socialization processes into account, predicts that women will show the greater preference. Table 4 presents the relevant data.

In every year, for both race categories, women are more likely to choose suicide over homicide than men are. These findings reflect a problem Henry and Short encountered. They found, consistent with their prediction, that males have higher suicide rates, but they also found, contrary to expecta-

tions, that males have higher homicide rates. It is just this type of problem which the use of SMR's avoids. Further, these findings suggest that socialization practices loom as important mediating conditions between sociological categories and expressions of aggression.

But Henry and Short have still a point to make. They suggest that the female has a higher status than the male among Negroes; if this is so, they would predict that the male has the higher homicide rate. And, since Negroes are disproportionately represented in the homicide statistics, this rea-

aggression also explains why the difference in preference for suicide is greater between sexes among Negroes than among whites. This explanation does not seem co-ordinate with Henry and Short's. Our findings would be expected if the type of punishment received by boys and girls differs more among Negroes than among whites. Suppose the percentage of Negro boys who receive physical punishment is much greater than the percentage of Negro girls who receive such punishment but that the percentage of white boys who receive physical punishment is only slightly larger than the percentage of

TABLE 4*

SUICIDE-MURDER RATIOS OF MALES AND FEMALES, WHITE AND NON-WHITE
1930, 1932, AND 1933†

YEAR	SUICIDE RATE		HOMICIDE RATE		SMR‡	
	Male	Female	Male	Female	Male	Female
1930:						
White.....	27.7	7.9	2.7	0.1	91.2	98.6
Non-white.....	9.0	2.6	8.0	1.0	52.8	71.8
1932:						
White.....	30.9	8.0	3.1	0.1	90.9	98.8
Non-white.....	10.5	2.8	21.5	2.9	32.8	49.1
1933:						
White.....	28.0	7.6	3.1	0.2	90.0	97.4
Non-white.....	9.4	2.6	22.0	2.9	29.9	47.3

* Sources: United States Department of Health, Education, and Welfare, *Vital Statistics* for the years 1930, 1932, and 1933; United States Bureau of the Census, *Prisoners in State and Federal Prisons and Reformatories* for the years 1926-36.

† Data for 1931 were incomplete.

‡ All differences between male and female SMR's each year are significant beyond .001 (two-tailed).

soning would account for the higher male homicide rate in general. They conclude that "further research should show that the ratio of male to female homicide among Negroes is higher than the male to female homicide among whites."¹⁷

Table 4 supports this last hypothesis. Among the non-whites, who are predominantly Negroes, there are seven male murderers to one female murderer, while the ratio among whites is three to one. Even more important, a comparison of SMR's shows that there is a greater difference between the sexes in the non-white population in the preference for suicide over homicide. So Henry and Short find evidence here for their explanation of the findings.

However, the theory of socialization of

white girls who receive such punishment. Then it would follow that Negro boys should have a much lower SMR than Negro girls, while white boys would show an SMR only slightly lower than white girls. Table 5 presents the relevant punishment data on a nation-wide sample of schoolboys and girls. The subjects are aged fourteen to sixteen, and race is controlled.

Table 5 clearly validates the assumption that differences in type of punishment is greater among Negro boys and girls than among white boys and girls. Furthermore, the figures here directly parallel the SMR's in Table 4: the white children are less likely than the Negroes to be punished physically and to show the lower SMR's. There is strong evidence here, then, that the manner in which children are socialized, as indicated

¹⁷ *Ibid.*; p. 88.

by the way in which they are punished, is a factor in determining a later preference for suicide or homicide.

Let us raise one more issue. Henry and Short present us with a contradiction of factors affecting homicide and suicide rates in the "central disorganized sectors of cities": "From the general negative correlation between homicide and status position, we would expect the low status ethnic and Negro inhabitants of these areas to raise the homicide rate. From the suggested relation between homicide and strength of the relational system, we would expect the 'homeless men' and 'anonymous' residents of

centers with the periphery, which a theory of external restraint could not.

Although the necessary rates were not available to compute the appropriate SMR's, data gathered by Schmid, and cited by Henry and Short,¹⁹ support the hypothesis. They indicate that homicides are more concentrated in the hub than are suicides—quite a different picture from that presented by other areas.

DISCUSSION

Now, Durkheim tentatively regarded the choice of anomic suicide or homicide as a

TABLE 5*
TYPE OF PUNISHMENT RECEIVED BY FOURTEEN-TO-SIXTEEN-YEAR-OLD
MALES AND FEMALES, WHITE AND NEGRO

	Per Cent Physical†	Per Cent Other	Per Cent Never Punished	Per Cent Unknown	No.
White:					
Boys	8.9	85.8	3.5	1.4	649
Girls	6.2	90.0	3.2	0.5	769
Negro:					
Boys	50.0	50.0	0.0	0.0	40
Girls	22.0	73.2	4.9	4.9	41

* Source: E. Douvan and S. Withey, *A Study of Adolescent Boys* (Ann Arbor: Institute for Social Research, 1955); E. Douvan, C. Kaye, and S. Withey, *A Study of Adolescent Girls* (Ann Arbor: Institute for Social Research, 1956).
† Significance of differences in percentage of physical punishment given boys and girls: white, >.10; Negro, >.01 (two-tailed).

rooming houses in these areas to lower the homicide rate."¹⁸

This contradiction of factors becomes unimportant if socialization of aggression is recognized as a crucial mediating process between sociological categories and determinants of aggressive behavior. For, although there are ethnic and other differences within the central urban population, the overwhelming majority of these people, non-white or homeless, are in the working class. This fact suggests that certain socialization practices concerning aggression are generally present in the hub, which would lead to a choice of homicide rather than suicide. By considering socialization as primary, we can generate this straightforward hypothesis involving comparisons of urban

purely psychological matter, unrelated to sociological variables. In *Suicide* he writes:

Anomie, in fact, begets a state of exasperation and irritated weariness which may turn against the person himself or another according to circumstances; in the first case, we have suicide, in the second, homicide. The causes determining the direction of such over-excited forces probably depend on the agent's moral constitution. According to its greater or less resistance, it will incline one way rather than the other.²⁰

Henry and Short suggest that the choice of suicide or homicide, prompted by a state of anomie, is not purely a psychological matter. While their *Suicide and Homicide*

¹⁸ *Ibid.*, p. 93.

¹⁹ *Ibid.*, p. 92.

²⁰ Émile Durkheim, *Suicide*, trans. A. Spaulding and G. Simpson, ed. G. Simpson (Glencoe, Ill.: Free Press, 1951).

includes an insightful discussion of psychological determinants of this choice, they assert that sociological variables play an active and separate role as well,²¹ that is, external restraint growing directly out of position in the social structure conditions expression of aggression.

Our own position lies somewhere between the two. We assert that, if sociological variables condition expression of aggression, it is necessary to search for the manner in which these variables are translated into those psychological determinants which lie closer to the actual individual choice. This position has led us to examine the socialization process, particularly socialization of aggression. We have cited evidence that a pivotal child-rearing variable—type of punishment—is related to position in the social structure. We have tried to show why this relationship exists: outward aggression seems to be more disturbing to the interpersonal relationships inherent to the middle class than to those of the working class; outward aggression is more consistent with the role of men than of women in our society; and verbal ability is closely related to recruitment into social classes in our society and may have a good deal to do with the way parents punish children. It is a short step from these arguments to the choice of suicide or homicide.

We have pointed out that many of the relationships derived by Henry and Short from a theory of external restraint might equally well be derived from the association of particular socialization practices with social classes in America. Further, the problem of suicide and homicide rates in central

portions of large cities, unresolved by the former, may be resolved by the latter.

But this does not by any means make the concept of external restraint less useful. On the contrary, if by external restraint we mean the degree to which one's behavior is controlled by an external other, the findings emphasize its value, for the theory of socialization presented and tested here also involves external restraint as a core concept. Physical punishment operates to create a preference for homicide insofar as it represents a pattern of controls which allows expression of aggression and does not build in controls over direct expression. We have assumed that this type of punishment is consistent with a value system which manifests itself in other child-rearing practices as well.

But rather than formulate external restraints in terms of relationships between broad sociological categories, we have availed ourselves of the clues psychology has to offer, particularly, that the relationship between parent and child is the crucial one.

It is possible that our proposal here is not alternative to Henry and Short's. Perhaps it is in addition. Those researchers certainly make it clear that they would not ignore psychological factors. But our feeling is that we are dealing with a unity. We have attempted to bring a social-psychological orientation to bear on the problem in order to show how sociological and psychological factors are related in one process.

DEPARTMENT OF PSYCHOLOGY
AND
RESEARCH CENTER FOR GROUP DYNAMICS
UNIVERSITY OF MICHIGAN

²¹ *Ibid.*, pp. 106-9.

ÉMILE DURKHEIM: ENEMY OF FIXED PSYCHOLOGICAL ELEMENTS

HARRY ALPERT

ABSTRACT

Durkheim waged relentless battle against the psychological mode of social analysis which relies on the application of fixed psychological elements. As a consequence of his conception of society as a reality *sui generis*, he developed the methodological canon that social facts must be explained in terms of other social facts. Despite the logical and sociological difficulties involved, the fixed psychological elements approach still prevails. Durkheim's position is discussed in terms of studies of the incest taboo.

In his delightful and observant book, *On Leisure*, Robert Osborn tries to explain the strange predicament of modern man in terms of Hunger, Love, Ghost-Fear, and Vanity.¹ Shades of William Graham Sumner! And later on, Osborn adds: "And to whatever degree he is unfaithful to the simple NEEDS of his PSYCHE to that same degree he can hope to reap nothing but ANXIETIES and UNCERTAINTIES."² These views are symptomatic of the persistence and sturdy survival of the psychological mode of analysis against which Émile Durkheim waged intransigent battle.

Nearly twenty years ago, I suggested that, while Durkheim was well known among American sociologists, he was not known well.³ The evidence is clear, however, that Durkheim has become increasingly better known and better understood in American sociology. His concepts have become part of the common, standard materials of contemporary sociology, although, as Zetterberg recently suggested in the *American Sociological Review*, such familiarity may be greater among the commentators than among the dimensionists or modelists.⁴

However, at least one important—indeed,

¹ R. Osborn, *On Leisure* (New York: Simon & Schuster, 1957), p. 12. To allay the concern of Sumnerian purists, Love becomes Sex two pages later.

² *Ibid.*, pp. 42–44.

³ H. Alpert, *Émile Durkheim and His Sociology* (New York: Columbia University Press, 1939), p. 213.

⁴ This classification is proposed by H. L. Zetterberg in the *American Sociological Review*, XXIII (1958), 95–96.

central—canon of Durkheim's sociology has fallen, for the most part, on deaf ears. I refer to the methodological principle that Durkheim derived from his fundamental conception of society as a reality *sui generis*, namely, that social facts must be related etiologically to other social facts or, in the language of Durkheim, that one must explain the social socially.⁵

Durkheim developed this methodological position in characteristic polemic fashion. This led, unfortunately, to the unwarranted charge that he neglected psychology and ignored biology. A typical view in this connection is that expressed by Bronislaw Malinowski, who, in attempting to dissociate himself from identification with Radcliffe-Brown, stated that, "Professor Radcliffe-Brown is, as far as I can see, still developing and deepening the views of the French sociological school. *He thus has to neglect the individual and disregard biology*" (italics mine).⁶ It is difficult to reconcile this misreading of Durkheim with the patent evidence of the total body of Durkheim's published work. Talcott Parsons reflects far more accurately the significance of Durkheim's sociology when he writes: "Durkheim, by virtue of the path along which his

⁵ See H. Alpert, "Explaining the Social Socially," *Social Forces*, XVII (1939), 361–65.

⁶ B. Malinowski, "The Group and the Individual in Functional Analysis," *American Journal of Sociology*, XLIV (1939), 939, n. 1. Herbert Blumer has similarly distorted Durkheim's position (see his chapter on "Social Psychology" in E. P. Schmidt [ed.], *Man and Society* [New York: Prentice-Hall, 1937]).

sociological analysis took him, was led deep into psychological theory and made extremely important contributions to it."⁷

What Durkheim meant, as he and his more judicious interpreters have made abundantly clear, is that, for etiological purposes, psychological or biological *constants* are not directly relevant to an analysis of the causes and functions of social phenomena.

First, Durkheim insisted that his methodological schema did not apply to the totality of the empirical phenomena of human existence but only to those aspects or dimensions of human life which were *social* in nature, part of the social reality *sui generis*. To concern one's self with the social aspects of human behavior by no means implies a denial of the existence or of the importance of other aspects. It simply defines the set of problems undertaken for analysis. Durkheim does not dismiss psychological or biological factors; he simply excludes them as direct and self-sufficient causes of social facts.

Second, Durkheim inveighed, not against psychology or biology, but against a particular schema of etiological analysis. I have elsewhere described this analytical method as follows:

First, one posits a certain kind of human nature inherent in all individuals and made up of psychological entities such as faculties, attitudes, ideas, sensations, instincts, tendencies, desires, wishes, etc. These one accepts as irreducible and self-sufficient elements, that is, as units which require no explanation other than the fact that they exist and are "given" to experience. Then, one proceeds to "explain" a given social phenomenon by relating it to one or several of these already posited psychological elements. Does one wish to explain the custom of blood-revenge? One consults the table of psychological elements that one has prepared and picks out, let us say, the instinct for revenge, or, if one's table omits instincts, perhaps the desire to kill or the wish for new experiences will do. Is exogamy the order of the day? The elements are ever plentiful: the instinct of racial purity, the desire for novelty, the feeling

of guilt, etc. Is one puzzled by religion? One need not be for long. Consult the elements and one will "discover" the fear of the dead, the fear of nature, the desire for immortality, the instinct for myth-creation, etc.⁸

Let us agree that an element of parody is involved in this description of the "psychological method" to which Durkheim was radically opposed. The fact remains that, despite a generation of criticism against the "instincts" of McDougall, the "drives" of Sumner, the "social forces" of Ward, the "wishes" of Thomas, the "needs" of Malinowski, on the one hand and the Freudians, on the other, this is a methodological perspective which still colors a great deal of both popular social analysis and professional sociological thinking.

Despite the aesthetic simplicity of this approach, it must be eschewed on both logical and sociological grounds. Logically, it involves the fallacy of explaining a variable by a constant. The varieties of sexual behavior viewed cross-culturally or the vast individual differences in sex behavior viewed within a given population cannot be explained, except tautologically, in terms of a constant, fixed, biological or psychological element called "sex." Nor can "hunger" explain the forms of economic institutions or the modalities of economic behavior. Even Malinowski, with his strong emphasis on biologically derived needs, has noted that man does not behave in terms of "mere anatomy and unadulterated physiology."⁹ "Appetite," he continues, "or even hunger is determined by the social milieu."

This suggests the sociological error in the "fixed psychological elements" approach: it assumes a biologically given, presocial, and precultural individual. Durkheim devoted considerable effort to exposing the inadequacies of this standpoint. Man, he insisted, is a product as well as a creator of society, and, consequently, a theory of human nature must be the end result and not the starting point of a science of sociology. Granted the existence of social phe-

⁷ T. Parsons, Foreword to Émile Durkheim, *Education and Sociology* (Glencoe, Ill.: Free Press, 1956), p. 9.

⁸ Alpert, *Émile Durkheim and His Sociology*, p. 143.

⁹ Malinowski, *op. cit.*, p. 943.

nomena strictly identifiable as *social*, it follows that, as *social facts*, they can be explained and understood only in the context of the interactional and associational system we call "society." Consider, for example, the incest taboo. Unless one postulates an instinctive moral aversion to incest, as Francis Hutcheson did early in the eighteenth century,¹⁰ it is impossible to derive the incest taboo from the given biological nature of the presumed presocial, isolated individual. The incest taboo, as a social phenomenon arising out of the conditions of social life, must derive from and have its etiological basis in some requirement of social existence. Sociologists and anthropologists are not at the moment in agreement on the explanation of the prohibition against incest.¹¹ Durkheim's own approach to the incest taboo was in terms of the fundamental incompatibility between the kinds of personal relationships and sentiments involved in sexual intimacy and those appropriate for the maintenance of moral and social relations between mother and son, father and daughter, and brother and sister.¹²

White stresses the requirement "to make cooperation compulsory and more extensive, to the end that life be made more secure." Radcliffe-Brown insists that "the rules against sexual intimacy between kin outside marriage are to be understood by their

¹⁰ See E. A. Hoebel, "An Eighteenth Century Culturological Explanation of the 'Incest Tabu,'" *American Anthropologist*, LV (April-June, 1953), 280-81.

¹¹ See the discussions by L. A. White, A. R. Radcliffe-Brown, B. Z. Seligman, W. D. Wallis, and F. Rose: L. A. White, "The Definition and Prohibition of Incest," *American Anthropologist*, L (1948), 416-35; A. R. Radcliffe-Brown, "White's View of a Science of Culture," *American Anthropologist*, LI (1949), 503-12; B. Z. Seligman, "The Problem of Incest and Exogamy: A Restatement," *American Anthropologist*, LII (1950), 305-16; W. D. Wallis, "The Origin of Incest Rules," *American Anthropologist*, LII (1950), 277-79; F. Rose, "More on the Origin of Incest Rules," *American Anthropologist*, LIII (1951), 139-41. The quotations that follow are from these sources.

¹² É. Durkheim, "La Prohibition de l'inceste et ses origines," *Année sociologique*, I (1898), 1-70.

social function as serving to maintain the stability or equilibrium of family and kinship structures." He also argues that "the first requirement for a sociological theory is the recognition that incest is normally an offence against religion." And Brenda Seligman places the accent on the role of the incest barrier in providing the foundation for the development of the family from a biological group to a social group, a process that has been of great survival value. Whatever the final outcome of these discussions, we must give Durkheim full credit for having defined a significant problem and for having provided a sound methodological orientation. No theory after Durkheim of the prohibition of incest can reasonably involve either the older rationalistic explanations in terms of an imputed biological knowledge impossible to primitive peoples or the tautological views which derived incest taboos from instinctual fears or horrors. The social fact of incest taboo will have to be explained "socially."

The essence of the Durkheim's perspective was superbly expressed in this *Journal* in 1936 by Ellsworth Faris:

Institutions and customs precede individuals and personality results from participation in these ongoing social processes. Human personality, arising in communication, is the result of conduct which takes place in the presence of others and in contacts with friends and enemies, allies and opponents. Personality is mobile, self-developing, self-organizing. Groups precede babies and children are born into communities with customs. To assume fixed points of origin or stable elements which are combined into a personality is to reverse the order of development. Ideas, sensations, and wishes occur, but they are events and consequences, not elements. They must be defined in terms of the social process, not the process in terms of them.¹³

On the occasion of this centenary, it is hoped that renewed attention will be given to the implications of Durkheim's insistence on studying social phenomena sociologically.

NATIONAL SCIENCE FOUNDATION

¹³ E. Faris, "Of Psychological Elements," *American Journal of Sociology*, XLII (1936), 175-76.

NEWS AND NOTES

Bowdoin College.—Assistant Professor Leighton van Nort has been appointed acting chairman of the Department of Sociology during the absence of Professor Burton W. Taylor, who is on sabbatical leave.

University of Chicago.—The Graduate Library School wishes to announce that "Iron Curtains and Scholarship: The Exchange of Knowledge in a Divided World" is the subject of the Twenty-third Annual Conference of the Graduate Library School of the University of Chicago, to be held July 7-9, 1958. Ten speakers, including librarians, university faculty members, representatives of scholarly foundations, and a diplomat, will present formal papers. The speakers will examine the difficulties of communication between East and West, consider the life and work of intellectuals, publishing and bookselling, and libraries under communism, and evaluate the content of Russian and eastern European publications in the sciences, social sciences, and humanities, the American exploitation of these materials, and American library collections of them. A final paper will discuss the prospects for a freer exchange of knowledge in future.

Among those who will present papers are Kenneth Holland, president, Institute of International Education, Inc.; Alexander Dallin, Russian Institute, Columbia University; Everett C. Olson, associate dean, Division of the Physical Sciences, University of Chicago; Edward J. Brown, Department of Modern Languages, Brown University; Leopold H. Haimson, History Department, University of Chicago; Yuri Gvosdev, Embassy of the Union of Soviet Socialist Republics; Douglas W. Bryant, associate director, Harvard University Library, and member, A.L.A. International Relations Committee; Melville J. Rugles, vice-president, Council on Library Resources, Inc.; Thomas J. Whitby, Cyrillic Union Catalog, Library of Congress; and Howard W. Winger, Graduate Library School, Director of the Conference.

For further information concerning the Conference write to Howard W. Winger,

Graduate Library School, University of Chicago, Chicago 37, Illinois.

The College Entrance Examination Board.—The Board announces that research workers in psychology, education, and the social sciences may now obtain the volume *The Research Activities of the College Entrance Examination Board, 1952-1957*, which contains a non-technical discussion of the more than two hundred studies supported by the Board in this period. The research is grouped under more than sixty topics and pertains to the following major areas: test construction and validation, college admissions practices, the nature of the pupil-candidate-student universes, the nature of the school-college universes, and CEEB programs. The purpose of this volume is to inform researchers of the findings of Board-supported studies in these areas, as well as to elicit their suggestions concerning further research possibilities in which they may be interested. Those desiring a copy of this publication should address their request to Dr. Joshua A. Fishman, Director of Research, College Entrance Examination Board, 425 West 117th Street, New York 27, New York.

The Board once more announces its interest in considering preliminary statements of research plans in the general area of non-intellective factors related to college performance. Since its announcement to this effect in the spring of 1957, the Board has undertaken to support the following studies in this area: Dr. Edward F. O'Day, San Diego State College, "Measuring non-intellective factors via aptitude and achievement item types"; Dr. Paul F. Lazarsfeld, Columbia University, "A review and analysis of college performance and of its non-intellective components"; Dr. Natalie Rogoff, Bureau of Applied Social Research, Columbia University; "Colleges, careers and social contexts"; Dr. Peter H. Rossi and Dr. James S. Coleman, National Opinion Research Center, University of Chicago, "The processes of college choice and their consequences"; Dr. Cliff W. Wing, Jr., Tulane University, "Class

and cultural corrections for test scores and school records as predictors of college performance." In addition to these studies, the Board will continue to support research projects in this area that were announced just a year ago. This will mark the third year of the Board's expanded program of non-intellective factors research.

Investigators interested in submitting preliminary statements of research interest should first request a copy of the announcement delimiting the nature of the Board's interest in this research area. Preliminary statements received by July 1, 1958, will be considered for support beginning on July 1, 1959. All investigators submitting preliminary statements will be informed of the Board's decisions by December 1, 1958. Address all correspondence to Dr. Joshua A. Fishman, Director of Research, College Entrance Examination Board, 425 West 117th Street, New York 27, New York.

Emory University.—A new doctoral program in sociology, to begin September, 1958, has been announced. The Department of Sociology and Anthropology offers specialization and supervision of advanced research in sociological theory, social organization, and social interaction to candidates for the Ph.D. degree. All graduate students at this level are also expected to achieve an acceptable degree of competence in cultural anthropology. In support of this program the department offers three \$4,000 fellowships, and in 1959 it will offer four additional pre-doctoral fellowships at \$2,400. Inquiries should be addressed to Dr. James W. Wiggins, Chairman, Department of Sociology and Anthropology, Emory University.

The department sponsored an Interdisciplinary Symposium on Culture Contact in Underdeveloped Countries, September 11-17, 1957. Sixteen participants from the United States and England, representing the fields of anthropology, demography, economics, history, political science, public health, and sociology, prepared papers which will be published under the editorship of Helmut Schoeck and James W. Wiggins. The participants were: H. G. Barnett, Peter T. Bauer, Charles J. Erasmus, Elgin Groseclose, Gottfried Haberler, Murray P. Horwood, George P. Murdock, J. Fred Rippy, David N. Rowe, Wilson Emerson Schmidt, Alfred G. Smith, William S. Stokes, Warren S. Thompson, Justus M. van der

Kroef, James W. Wiggins, and Helmut Schoeck.

During 1957 three members of the department received grants for research: James W. Wiggins, an Emory University research grant for a study of role behavior; Helmut Schoeck, an Emory University research grant for a study of the sociology of medicine in Europe; and Alfred G. Smith, a research grant from the University Center in Georgia for an experimental test of the Sapir-Whorf hypothesis.

Anna Green Smith was visiting associate professor during the summer of 1957; Mildred Mell will be visiting professor during the spring of 1958; and Jack R. Conrad will be visiting assistant professor during the summer of 1958 while Alfred G. Smith holds that position at the University of Oregon.

Charles D. McGlamery has left the department for the University of Alabama. John Doby has been appointed associate professor, beginning September, 1958. Joseph E. Faulkner has been appointed instructor for the year 1957-58. Dudley E. DeGroot has been promoted to assistant professor.

Florida Southern College.—John E. Owen, chairman of the Department of Sociology, has been granted a leave of absence for 1958-59, in order to accept a Fulbright Lecturing Award at the University of Dacca, Pakistan. Mrs. Garnet Owen, assistant professor of sociology, will engage in research and writing in Pakistan.

International Sociological Association.—A conference on peaceful co-operation was held in Moscow, January 6-11. It was one of a series of meetings of social scientists supported by UNESCO. Papers were presented on inequality of income, educational selection, the film hero and conceptions of success in various cultures, and on the state and organization of sociological teaching and research. Present as members of the conference were: Friedmann (France), president of the International Sociological Association; Bottomore (England), secretary; Merran McCulloch (New Zealand), assistant secretary; Ostrovitianov, vice-chairman of the Soviet Academy of Science; Fedoseev and Berestnev (U.S.S.R.); Aron (France); Svoboda (Czechoslovakia); Lukics (Yugoslavia); Saksena (India); den Hollander (the Netherlands); Schelsky (German Federal Republic); Malinski (Roumania); and

Hughes (United States). Marshall and Forcart represented the staff of UNESCO. Polish representatives who had been expected did not come. The International Sociological Association will issue a report on the meeting.

Professor Everett C. Hughes has sent a full report of the meeting to the Social Science Research Council and to the American Sociological Society's representative on the board of the ISA.

The ISA's Subcommittee on Stratification Research held its Fourth Working Conference at Geneva, Switzerland, in December, 1957. The papers presented at the Conference will be published under the auspices of the ISA. The chairman of this subcommittee was David Glass (London School of Economics). Conference participants from the United States were Oscar Handlin (Harvard University), Peter M. Blau (University of Chicago), and C. Arnold Anderson (University of Kentucky).

University of Kansas City.—Howard Becker is now offering a seminar in qualitative methods of research in sociology.

Ernest Manheim has been appointed Henry Haskell Professor of Sociology.

Leon Warshay has joined the sociology department as an instructor.

McCahan Foundation.—A long-range study of the American family and its implications to life insurance is being undertaken by the David McCahan Foundation of the American College of Life Underwriters with the assistance of social scientists who have specialized in the field.

Implemented by a program of annual lectures growing out of creative research, the study will examine such topics as the relationship between generations, family stability, the significance of the size of the family, and building better families.

Dr. Davis W. Gregg, president of the American College, has announced that the second annual lecture of the Foundation will be delivered as a part of the annual meeting of the American Life Convention in Chicago next October by Dr. William Fielding Ogburn, emeritus professor of sociology at the University of Chicago.

University of Michigan.—The Survey Research Center announces the Eleventh Annual Summer Institute in Survey Research Tech-

niques. The Institute will run from July 21 to August 16, with an introductory session from June 23 to July 19. This Institute is designed to meet some of the educational and training needs of men and women engaged in business and governmental research or other statistical work and of graduate students and university instructors interested in quantitative research in the social sciences. The program of the regular session will include special presentation of the Center's ongoing research and the offering of three courses in survey research techniques which can be elected for graduate credit. For further information write to the Survey Research Center, University of Michigan, Ann Arbor, Michigan.

State of Minnesota.—The Department of Public Welfare announces that Joseph C. Lagey, formerly of Pennsylvania State University, has been appointed director of a ten-year follow-up study of fifteen hundred discharged mental-hospital patients. The study involves establishing intensive pre-discharge planning, as well as integrating and rationalizing community resources for the patient.

Robert Wolff, previously with the Institute of Communications Research, University of Illinois, will become leader of one of the community research teams.

Inez Hunting, previously with the Boston Pilot Study, will join the hospital research team.

The study is financed by a grant from the National Institute of Mental Health and is sponsored by the Division of Medical Services of the State of Minnesota.

Muhlenberg College.—Morris S. Greth, professor of sociology and chairman of the department, has returned to his teaching duties following successful recovery from a recent illness.

Robert Parke, Jr., is instructor in sociology.

The department is currently establishing census tracts for the non-central city portions of Lehigh County. Students in the urban sociology course have participated in this work, which is being conducted on behalf of the Lehigh County Community Council and the Bureau of the Census.

National Science Foundation.—The Foundation wishes to announce the following grants in sociology and social psychology recently

given as part of its Social Science Research Program: U. Bronfenbrenner, Cornell University, "Identification and Family Structure"; R. D. Luce, Harvard University, "Mathematics of Imperfect Discrimination"; C. De Soto, Johns Hopkins University, "Conceptual Learning of Relationships"; and D. O. Price, University of North Carolina, "Computer Research in Demography."

The next closing date for the receipt of proposals in the Social Science Research Program is October 1, 1958.

Ohio State University.—Professor A. R. Mangus is currently dividing his time between the Department of Sociology and Anthropology and the Department of Agricultural Economics and Rural Sociology. He spent the summer of 1957 as Visiting Professor of Family Sociology, University of Southern California.

Everett M. Rogers, assistant professor, joined the staff in a teaching and research capacity after receiving his Ph.D. degree from Iowa State College in March, 1957. He is conducting a research study of the communication of agricultural technology with Wade H. Andrews and Harold R. Capener.

Robert M. Dimit, associate professor, is directing a study of membership relations in a major farm organization in Ohio, in addition to his duties as extension specialist. The field work is completed and analysis is under way.

Wade H. Andrews was named associate professor of rural sociology effective July 1, 1957. In addition to his teaching duties, he is directing a study of membership relations in milk-producer co-operatives.

Dr. Andrews, Saad Nagi, instructor, and Raymond Klingel, research assistant, have been involved in a study of out-migration from a metropolitan center to the surrounding rural areas in Franklin County.

A new project has recently been developed with funds provided by the Ohio Heart Association. The study is directed by Wade Andrews and Saad Nagi and is concerned with the adjustment of family members when the farm operator is stricken by heart disease. Also co-operating on this project are the Department of Agricultural Engineering and the School of Home Economics.

Professor Merton D. Oyler has been appointed jointly to the Department of Sociology and Anthropology and the Department of

Agricultural Economics and Rural Sociology. In addition to his teaching responsibilities, he is engaged in a study of modernizing influences upon the Amish in Ohio. Dr. Oyler recently returned from a six months' leave in England, where he was engaged in a study of fringe communities.

Harold R. Capener, associate professor, will leave for India in March, 1958, for a two-year assignment on the Ohio State-India project, where he will be involved in community development work.

The Population Council.—W. Parker Mauldin was on leave from September through April on assignment with the United Nations Technical Assistance Administration. He served as a staff member of the UN Demographic Training and Research Centre for Asia in Bombay and has since visited Indian, Middle Eastern, and European universities and population research centers. His place was taken by Vincent H. Whitney, who is on leave from Brown University.

Dudley Kirk is serving as a member of the Technical Board of the second UN Training and Research Centre for Latin America in Santiago, Chile, which he visited in November.

Princeton University.—Dr. Gardner Patterson, director of the International Finance Section and professor in the Department of Economics and Sociology, has been named director of the Woodrow Wilson School of Public and International Affairs.

Professor Patterson will succeed Dr. Dana G. Munro, William Stewart Tod Professor of Public Affairs and director of the Wilson School since 1939.

The Wilson School is a co-operative enterprise of the departments of history, politics and economics, and sociology. On both undergraduate and advanced levels it seeks to carry forward President Wilson's interest in preparing Princeton men for government service and citizenship by emphasizing training in the interrelated disciplines of the social sciences.

San Jose State College.—In 1958-59 the Department of Sociology and Social Work will admit graduate students working toward a Master's degree in sociology. In the beginning, opportunity will be provided for specialization in probation and parole service, correctional institution counseling, industrial prob-

lems, and community planning. Plans have also been completed to introduce graduate courses in sociological theory and to expand the department's undergraduate courses.

Servico Nacional de Aprendizagem Comercial Departamento Comercial.—The Department of Commerce of Brazil wishes to announce the recent organization of its Social Research Division. Under the direction of the Brazilian labor economist, Robert Dannemann, the Division will study the labor market, occupational mobility, workers' productivity, and standard of living, as well as do job analysis.

The Society for the Scientific Study of Religion.—The Society will hold its fall meeting at Harvard University on November 1, 1958. Social scientists who would like to present papers on empirical studies in any phase of the social or social-psychological functions of religion are invited to send papers—or 300-word abstracts of proposed papers—to Theodore W. Sprague, 13 Follen Street, Cambridge 38, Massachusetts, before August 30.

The Society for the Scientific Study of Sex.—The first annual meeting of the Society will be held on November 8, 1958, at the Barbizon-Plaza Hotel in New York City. For details write Robert V. Sherwin, 1 East 42 Street, New York 17, New York.

United States Department of Health, Education, and Welfare.—A six-member panel of non-government experts will provide consultation to the National Institute of Mental Health on the mental health research program conducted in laboratories and other facilities at the National Institutes of Health, Bethesda, Maryland, and at field stations.

Membership on the new panel, known as the Board of Scientific Counsellors of the National Institute of Mental Health, is apportioned selectively between clinical and fundamental science categories to maintain balanced perspective. The panel includes Robert F. Bales, Neal E. Miller, Jordi Folch-Pi,

Horace W. Magoun, M.D., John Benjamin, and Stanley Cobb, M.D.

In addition to their review of the Institute's scientific activities, the new counseling body will provide the director of the Institute with objective viewpoints and guidelines on the long-range perspective of intramural research.

Membership on the Board is for a term of four years. However, for the purpose of establishing a rotation of tenure, the terms of the initial appointees, which commenced July 1, 1957, will expire at staggered intervals.

Western College for Women.—Dr. Edward W. Pohlman, professor of sociology, has received a Fulbright Award as a lecturer in sociology at the University of Karachi in Pakistan for the academic year 1958-59.

Western Reserve University.—Dr. Richard A. Schermerhorn, associate professor of sociology, has been appointed lecturer to the Salzburg Seminar in American Studies which convenes from August 24 to September 20, 1958. His section will deal with American minorities and will include twelve lectures and eight seminar sessions. Dr. Schermerhorn is one of four lecturers from the United States making up the faculty.

The World Health Organization.—Publication of WHO's *International Classification of Diseases* (7th rev.) has been announced. The new edition, effective January, 1958, will be used in the United States and by other member nations of the World Health Organization over the next ten years to classify causes of morbidity and mortality.

The seventh revised edition may be obtained through the Public Health Conference on Records and Statistics. The Conference is selling the seventh revised edition, which is in two volumes, at the price of \$3.50. Checks should be made payable to the Public Health Conference on Records and Statistics and orders directed to that organization, % Department of Health, Education, and Welfare—NOVS, Washington 25, D.C.

BOOK REVIEWS

Brücke und Tür: Essays des Philosophen zur Geschichte, Religion, Kunst, und Gesellschaft ("Bridge and Door: Essays of the Philosopher on History, Religion, Art, and Society"). By GEORG SIMMEL. Edited and with an Introduction by MICHAEL LANDMANN and MARGARETE SUSMAN. Stuttgart: K. F. Koehler Verlag, 1957. Pp. xxiv+281.

"If in the *bridge* [*Brücke*] separation and union meet in such manner that the former seems a thing of nature and the latter man-made, the *door* [*Tür*] draws both equally into human achievement and draws them together as human achievement."

Simmel loves a paradox. He loves contrast and dialectic; nature versus art, thought, and concept is one of his favorite themes. He loves to play with distance and nearness: art by being so far from any given real object can be perceived as close to any. God is so conceived that in his infinite distance he is equally accessible to all men. In the essays here drawn together one sees Simmel playing his favorite game with a number of problems. The editors are right in saying that he was primarily an essayist and that these essays give a fairly well-rounded view of his thought. Like other essayists, Simmel tended to make each essay a complete work, containing a great deal of his style of thought and many of his ideas—ideas so closely related that those who deny that a system of thought exists unless each part can be painfully taken apart and badly put together, with the joints showing, say that there was no system in it.

As a matter of fact, his thought is so much a system—or perhaps, to use his own phrase, so imbued with a consistent style—that one who wishes to understand those of his writings that are dubbed "sociological" should also be steeped in those nominally devoted to art, history, and culture or to philosophy of life. In these other essays one sees that peculiar relation of the concrete, or substantive, and the abstract which distinguishes his sociology from that of his contemporaries and, indeed, from most of those who call themselves sociologists. As a matter of sociology of knowledge, one could argue that the professionally sociological part of any man's work should be read in con-

junction with whatever else he may have written. Simmel, by the range of his interests and by the volume of his publication in books, periodicals, and even newspapers, makes this easy to do—at least one can sample his non-sociological work. This, incidentally, is also true of Weber, Pareto, Spencer, Durkheim, Sumner, Small, and—to some extent—Park. As sociology becomes more professional, we will probably have little or nothing of a sociologist's thought in print except that which meets the canons of professional journals.

Yielding to professionalism for the moment—and it is easy and pleasant to do so in this case—I find that the essays of special interest to sociologists are entitled "Sociological Aesthetics," "The Domain of Sociology," "Cities and the Life of the Mind," "Sociology of the Meal," "Individualism," and "Individual and Freedom." Of these, "The Sociology of the Meal" is the most whimsical. Eating is something we all must do; it is utterly banal. Yet some of the most refined of human rules have grown about the interaction of people while eating. The piece was written for a supplement to a Berlin daily paper.

The best known of the essays is "Cities and the Life of the Mind." The ideas are familiar to many sociologists, although they are not always attributed to Simmel. He emphasizes the impersonality of the city. Goods are produced for unknown customers, separated from the producer by many intervening agencies. Many conclusions are drawn, among them some concerning the struggle for individuality. While many of his statements still are cogent, others seem a bit dated. The contrast between the great city and the provincial city is, in part, made anachronistic by radio, television, and travel. Apparently, Simmel did not foresee how much the countryside would be merged with the city—nor how like the *Kleinstadt* the suburb could become. In speaking of the impersonality of the market, he did not foresee that an army of market researchers would come to call at everyone's ("everyone" in the sense of an adequate stratified sample) house to find out what we would all like made or done for us, leading to the phenomenon known as "personal-

izing." But who did foresee these developments? Even so, the difference between *Weltstadt* and provincial cities, no matter what their populations, is perhaps even more marked than when Simmel wrote. His formulation of the problem of individuality in a mass society is also still trenchant.

The most important sociological essay in the volume is "The Domain of Sociology," (now available *in toto* in English) abbreviated from his *Grundfragen der Soziologie* ("Basic Problems of Sociology"). This was the small book (translated in full in *The Sociology of Georg Simmel*, trans., ed., and with an intro. by Kurt H. Wolff [The Free Press, 1950] Part One) in which he stated his system for the cramming trade among students. (All German professors published such small books.) One will find in this essay a clear statement of the idea of interaction and the development of the notion—so fundamental to sociology—that forms of interaction are independent of the content and interests which bring people into interaction. Although this notion is implied in Tarde, Sighele, and others who are regarded as early sociologists, it is to Simmel that we owe the clear statement of the central place of interaction in sociological analysis. On the other hand, he was keenly interested in content—in news, events, interests, his times, the cities in which he lived—and he deals explicitly with the relations of the historical and the sociological. The purest sociology, of course, has to do with the forms of interaction, but there is a kind of sociologically minded history in which the historian of, say, religion, gives a new dimension to his material—that of interaction and the social forms arising out of it. Simmel sees the value—indeed, the necessity—of several kinds of sociological enterprise from the relatively concrete (bound to substance, to special social institutions such as religion or the state) to the utterly abstract. This accounts for his own penchant for moving from something like the study of criminology (by watching respectable audiences react in the theater) to consideration of the abstract problem of the reality of social entities other than the individual.

His discussion of this last point is still the classic one in sociological literature. While it is only metaphor, it is still a good one to say that one can no more understand the individual without reference to systems of interaction or small groups and structures without attention

to the larger systems in which they exist, than one can understand an organ without the body.

One wonders why Simmel had so few, if any, German students who undertook to do empirical work on his treasure of ideas. His disciples were mainly American. Albion Small published several chapters of the *Soziologie* in the *American Journal of Sociology* before the book appeared in German. Park and Burgess made Simmel's concept of interaction the central one in their *Introduction to the Science of Sociology*. Through the medium of that book a great deal of Simmel's thought was diffused in American sociology, often in textbooks which made no reference directly to Simmel.

Park had something of Simmel's own penchant for shifting from a philosophical idea to the day's news, a play, the life-story of a prostitute or a saint. He certainly regarded himself very much as a disciple of Simmel, although not of Simmel alone. And he had, as Simmel did not, a large number of students who did empirical work. Those students were nourished, more than they realized, on Simmel's ideas.

One can only speculate why Simmel did not have more disciples in sociology. Unlike Weber, he did not cover in his work a variety of historical epochs, movements, and institutions. There is no "Protestant Ethic," no long article on the Pharisees, no set of monographs on current problems of industrial relations. There is, in his own sense of the term, less *substance* in Simmel's work than in that of Weber. Or perhaps the essayist has as disciples only aspiring essayists. One thinks of another essayist, Cooley, who, like Simmel, had few disciples yet influenced many people profoundly.

The answer may lie in Simmel's very emphasis upon *Abstand*, standing off at a distance to have a good look, but a look that is like that of the artist who turns a piece of nature into a landscape (one of the essays has to do with the landscape) or a face into a portrait.

Presently, it looks as though we were in for something of a Simmel revival. Part of it comes through the people who experiment with small groups and have need of content-free formulations of interaction. Part of it may come from a rebound, in Germany, from both existentialism and Marxism—the honest social-democratic kind and the perverted Nazi kind alike—toward the elegant, urbane, humane, and intellectual philosophy of a pre-World War I citizen. It may, in American sociology,

be something of a reaction against a reaction against positivism—a peculiar reaction which overrationalized social conduct by forcing all of it into the mold of purposeful action and games.

In any case this particular volume puts a large deal of Simmel's thought—both philosophical and sociological—into a readable volume. Together with the recent English translations done by Wolff and Bendix, it puts a great amount of Simmel back into easily available print.

The Introduction tells something of Simmel's life and career. There is a bibliography of his work and of work about him. The latter is taken from a fuller one which is being assembled by Dr. Kurt Gassen for publication in the near future.

EVERETT C. HUGHES

University of Chicago

Durkheim and His Critics on the Sociology of Religion. By IMOGEN SEGER. ("Monograph Series.") New York: Bureau of Applied Social Research, Columbia University, 1957. Pp. ii+82.

This essay reviews the criticism of Émile Durkheim's work, *The Elementary Forms of the Religious Life*, systematizes it under a few major categories, and evaluates the status of Durkheim's theory as it emerges from the controversy. Durkheim's fundamental theory, of course, held that beliefs about the sacred are collective representations whose object is society itself. Thus society is the source of moral obligation and sentiments of respect represented in sacred entities. God, therefore, is a symbol for society. Sacred rites are modes of affirmation of society in collective actions.

Miss Seger cites eight main arguments raised by anthropologists against the theory. The most serious are (1) that Durkheim generalizes from a single case and (2) that he makes the unreasonable assumption that any existing society can represent a type of primitive man. She acknowledges these limitations but views Durkheim's work, within his own perspective, as "a pioneer demonstration of a general principle" (p. 16). (Perhaps it would be better to call it a pioneer construction of hypotheses about religious phenomena, since the evidence cannot provide a demonstration.) Furthermore, she says, certain crucial conten-

tions of fact have been borne out in further exploration of the hypotheses. Talcott Parsons summarizes these as "(1) that there is a basic distinction of attitudes toward the classes of things which Durkheim designates as 'sacred' and as 'profane' and (2) that sacred things have a symbolic significance" (p. 21). Moreover, Radcliffe-Brown has confirmed Durkheim's basic thesis of the social function of totemic religion among the Australians. On the basis of these confirmations, Miss Seger views the anthropological criticisms as corrective to the theory rather than fundamentally challenging.

Psychological criticisms of the theory have taken two directions. One was the attack on Durkheim for treating religion as a phenomenon of crowd psychology. Durkheim exposed himself to this attack by occasional references to crowd phenomena, but his basic concept of society as a moral community could hardly be equated with a crowd. Another form of psychological criticism was concerned with the depreciation of individual aspects of religious phenomena—the charismatic leader, the mystic, etc. Durkheim unquestionably subordinated the individual aspects of religion to the social. He was unrelenting in his opposition to psychological explanations of social phenomena. Miss Seger suggests, however, that much of what Durkheim classed within the category of *société* would now be treated as culture and makes possible a resolution of the individuality versus collectivity controversy.

The ideological and religious objections have been directed against Durkheim's tendency to replace individual autonomy with social determinism. Primarily, these were attacks on the value assumptions implicit in the theory. Miss Seger does not take up Parsons' criticism of the social solipsism in Durkheim's theory by which his own categories of analysis are relativized. This criticism starts within Durkheim's own premises and is much more serious than the critique of Richard and Webb.

The historical and pragmatic arguments against the theory do not bear too significantly on the scientific issues. They do lead, however, into the fundamental criticism raised in the section on the functional approach. Parsons has pointed out that Durkheim fell into the self-same error with which he charged the animistic and naturalistic views when he located the referent of religious symbols in the empirical society. Society, to Durkheim, is an

empirical phenomenon that is subject to scientific study. If the referent of religious symbolism is such an empirical entity, then science can demonstrate that religion is an illusion. Therefore, religion should disappear under the impact of scientific scrutiny. Religion has not disappeared, however, despite Durkheim's work. Thus Durkheim must be wrong. Parsons attempts to resolve this problem by introducing the common value system as the social entity on which Durkheim had focused. The sphere of the sacred which pertains to the non-empirical aspects of existence interpenetrates in several ways with the common value system of a society, although these are distinct entities. Durkheim referred both these phenomena—the common value system and the beliefs about sacred entities—to a single symbolic referent, the empirical society. This reductionism was a product of his earlier positivism and was not intrinsic to his fundamental theory. In this way, Durkheim's fundamental hypothesis can be set within a more adequate theoretical context.

Miss Seger concludes her review of the criticisms with the view that "most of the criticisms of Durkheim which have been studied in this essay contain an element of truth, but Durkheim's opinion in the main prevails over them. . . . His theory stands essentially unchanged, but corrections are made, negative corollaries introduced, and necessary elaboration is added" (p. 69).

The author has done a service to the sociology of religion. Her estimate of Durkheim's stature in the field will find ready agreement. However, she has not included a serious treatment of Parsons' critique of Durkheim—both the problem of social solipsism and the static character of his theory. There is, after all, no real principle of social change in Durkheim's theory. The concept of "effervescence in great rituals" cannot account for anything more than a fortifying of the existing moral community or a renewal of the apprehension of its significance. Durkheim, in fact, rested his principle of change on an evolutionary *mystique* which never entered the theory. These are serious criticisms of the theory and leave the master somewhat bloodier and more bowed than Miss Seger seems ready to admit.

GIBSON WINTER

University of Chicago

The Mormons. By THOMAS F. O'DEA. Chicago: University of Chicago Press, 1957. Pp. xii+289. \$5.00.

This is the best general sociological analysis of Mormonism yet made by either a Mormon or a non-Mormon scholar. As an "outsider" O'Dea has done a superb job of description and analysis of the value system and its overt manifestations in Mormon society. He has shown remarkable sensitivity to the subtleties of meaning often more implicit than explicit in the attitudes and behavior of the Mormon people. Mormon students themselves have difficulty in conveying to a "Gentile" the Mormon theology, church organization, and church practices. O'Dea has done this and much more and has done it well.

The core of this treatise is chapter vi, "The Values of Mormonism." However, chapters i-v form an indispensable introduction to the discussion of values. These chapters offer an excellent treatment of the essentials of church history, including the tragic conflicts with neighbors and the various migrations of the embattled sect, culminating in the dramatic transplains movement to the Great Basin.

The value system is astutely described by the author, drawing mainly upon the *Book of Mormon* and the *Doctrine and Covenants*, which constitute, with the *Holy Bible* and *The Pearl of Great Price*, the "standard works" of the church.

O'Dea discusses also the paradoxes of Mormonism. How to reconcile the civil and religious authority structures? Is a prophet with authority from God subject to any control of his followers or by the secular state? Mormonism developed on the American frontier in a period commonly designated as the "rise of the common man," yet it is in theory authoritarian. How can it be democratic as well?

In chapter ix, "Sources of Strain and Conflict," the author presents other "dilemmas," including, among others, the "encounter with secular thought," "authority and obedience versus democracy and individualism," "consent versus coercion," and "progress versus agrarianism."

The reviewer (as one of Mormon birth) is constrained to express gratification that the Mormon movement has apparently reached a stage of maturity where both Mormon and non-Mormon students may examine it with objectivity, free from the prejudgments which have colored so many of the innumerable vol-

umes on the subject. O'Dea says that it is a "peculiarly American religion" and "a peculiarly American subculture." As such, one might hope it may be regarded as an integral part of the total American experience.

LOWRY NELSON

University of Minnesota

Models of Man. By HERBERT A. SIMON. New York: John Wiley & Sons, 1957. Pp. xiv+287. \$5.00.

Increasingly, formulations of social science problems appear in mathematical terms. Often these show an insufficient acquaintance either with social science or with mathematics. The combination of social scientist, who knows what problems he wants to solve, with mathematician, who knows how to formulate these problems, is rare indeed.

Models of Man presents the products of such a combination. It consists of sixteen reprinted papers, ranging from learning psychology through small-group processes, organizational behavior, and political science, to economics.

The chapters of most direct interest to the sociologist are those on small-group processes. Here Simon begins with well-formulated and relatively unambiguous sets of propositions about group processes as set down by social psychologists (George Homans in one case, Leon Festinger in the other). These propositions, which are of the order, "As x increases, y increases," Simon translates into a mathematical language consisting of simultaneous differential equations. This language has, as to words, both advantages and disadvantages: it cannot tolerate great complexity, for it is not facile, as are our words; but it cannot tolerate ambiguity and unclarity either. Herein lies perhaps its greatest values; it clarifies and removes ambiguity from theories stated verbally. Whether mathematical descriptions of social processes will be most profitably built through this approach—using qualitative generalizations as postulates and building upward from them—is an open question. Whatever its ultimate answer, Simon has opened the question, a task more appropriate to the present stage of social science than that of answering it.

Other chapters of particular sociological in-

terest include a discussion of the concept of power in political systems and its mathematical analogues (chap. iv); an investigation of the way in which causality and spurious correlation can be recognized when variables are interrelated in a dynamic system (chaps. i and ii); and a stochastic model of imitative behavior (chap. ix).

I think that the major value of these models (I do not say this lightly, for several of them have had, in their original printing, strong impact upon my own thinking in mathematical sociology) lies in Simon's ability to break down the boundaries of disciplines, to use the tools current in one discipline to attack problems in another (e.g., he uses the differential-equation tools developed in physical dynamics for problems of small-group dynamics), or to juxtapose two ways of looking at the same events (e.g., the psychologist's view of choice behavior and the economist's). As a result almost every chapter casts a fresh perspective upon an existing problem, either by formulating that problem in the clear language of mathematics or by showing its connections to similar problems in other disciplines.

Two things this book does not do. It does not provide a definitive answer to the vital question: Will mathematics be of major use as a framework for sociological theory? These papers provide many glimpses of what may be done in restricted areas, but they do not provide a clear directive for future development. Second, as in most, if not all, mathematical theorizing in sociology at present, the papers do not develop (or even begin to develop) a theoretical edifice in any area. They stand as examples, as discrete attacks on problems of the moment. It is almost as though Simon is casting about for appropriate ways to merge social science problems with mathematical methods but without finding any which trigger off a whole new area of work.

In his Introduction to Part IV, however, there is some evidence that Simon has found his solution in a serious and long-term investigation of choice behavior. For the reader searching for the core of Simon's interest, this Introduction, together with chapters xiv and xv, comes closest, I think. These chapters begin to develop a theory of rational choice which conceives of choice behavior as consisting of a set of differentiated and complementary behaviors (e.g., search activities and fixation). As the beginning of a formal theory of

decision-making, this stands in direct contrast to the model of statistical decision theory, which views rational decision-making as a single "maximizing" or "minimaxing" activity.

This is not a book for the mathematical novice, but anyone with a modicum of college training in mathematics can follow it. And the serious student of mathematical sociology can find within its pages some of the best contemporary examples of setting sociology to mathematics.

JAMES S. COLEMAN

University of Chicago

Problems of Power in American Democracy.

Edited by ARTHUR KORNHAUSER. Detroit: Wayne State University Press, 1957. Pp. ix+239.

The lectures printed in this volume comprise the fifth annual series (1954-55) of the Leo M. Franklin Memorial Lectures delivered at Wayne University. The lecturers were Robert S. Lynd, Harold D. Lasswell, A. H. Maslow, C. Wright Mills, and Arthur Kornhauser. Each lecture is accompanied by abstracts of remarks by two discussants and the replies of the lecturer.

This book is not (and the nature of its formulation quite reasonably prevents it from being) a study of power in any technical sense. Perhaps it could be better described as the bench mark of scholarly thinking about power for 1955 or, better still, as a collection of inspirational discourses designed to motivate students toward the development of a more adequate methodological and conceptual basis for the study of power.

The lectures have a variety of perspectives. Lynd's and Lasswell's have more in common than any other pair. They are alike in that they have a somewhat prophetic overtone, a definite problem-solving approach, and imply clearly that action (social or political) is in order for the solution of problems related to the distribution of power in the contemporary United States. Both are agreed that power should rest in the hands of the general population and that power and freedom are not antithetical, both focus on the formulation of democratic goals, and both affirm the necessity of conceptualizing the electorate as an integral part of the institution of government if not its central core.

Lasswell sees the formulation of democratic goals as being most readily accomplished through "superior methods of communication." He denies that the present severe crisis must be followed by the emergence of a "garrison state," presenting more efficient and widespread communication as the means for preserving democracy.

Maslow is concerned with the impact of various forms of leadership on the personality of the individual; different forms and degrees of power relationships are examined with a view toward their effects on the mental health of those whose manipulation (or acculturation) is intended. The lecture is, as is Mills's, analytical and expository rather than exhortatory.

Mills offers a description of contemporary American social organization with respect to the structure of power and the emergent power elite; some attention is given to the dynamics of power, but the focus is on the structure. He urges no special philosophy about existing conditions and advocates no special action. Perhaps even more than Maslow does, he avoids the "problems-of-power" approach suggested by the title of the series and deals almost entirely with the description of the distribution of power from the detached perspective of a social anthropologist. That the host society is a democracy remains virtually incidental to his discussion.

Kornhauser's lecture is centered about problems of the professional social scientist which result from the existing power distribution in the United States. He reviews the functions of the social scientist as they should be prescribed by the philosophical and ethical codes of science and points to the pressures brought to bear on American social scientists in their endeavors to execute those functions. His expressed anxieties over the impediments to the development of a mature and useful social science—particularly those impediments deriving from control of the direction of research by power-wielding persons and institutions—serve to call attention to the need for careful discrimination between the work of social scientists and the work of persons using the methods and knowledge of social science.

The lectures are engaging to read and uniformly provocative. Still, the serious student must wander away more conscious than ever that a definitive concept of power is yet to be provided as a tool for social science. This is, of course, hardly the fault of these lecturers.

Perhaps, rather than a fault at all, the increased awareness of the gap will be their principal contribution.

THOMAS ELY LASSWELL

Grimmell College

The Institutions of Society. By JAMES K. FEIBLEMAN. London: George Allen & Unwin, Ltd., 1957. Pp. 400. \$8.50.

"A Philosopher Looks at Social Science" would make a good subtitle for this interesting book. Sociologists will find the treatment of the subject unusual, and graduate students should find that this intelligently written book will give them pause as they romp over the hurdles in their path toward a doctorate.

To Professor Feibleman institutions are the smallest social organizations that have a philosophy of their own. They are, however, more than a set of arguments, for they include social groups and human artifacts. The book moves from speculative accounts of the origins of such artifacts as speech, language, and tools by way of a central core of chapters which analyze the essentials of institutions as ideal-type organizations. Then follows a classification of the major varieties of institutions and two later parts which relate institutions, so classified, to the structure of society as a whole and to the beliefs (or, as he says, to the ontology) which constitute the culture of the society.

Although its theories are both descriptive and "backward-looking," and evidence in support of them either anecdotal or inadequate, the volume has a grand old-fashioned ring to it. Here is the whole social world described, one section of it, the institutions, in great detail, and all with a fine contempt for empirical evidence. It should be read by graduate students as an exemplar of a path that sociologists have not followed for thirty years.

JOHN MOGEY

University of Oxford

Community Involvement: The Webs of Formal and Informal Ties That Make for Action. By CHRISTOPHER SOWER, JOHN HOLLAND, KENNETH TIEDKE, and WALTER FREEMAN. Glencoe, Ill.: Free Press, 1957. Pp. 323. \$5.00.

This is an exhaustive description of the process of community self-survey. The survey described is concerned with unmet health needs, but its content is of minor importance; the authors' focus is the process itself, and, for this purpose, the over-all organization of the material is very effective. The book begins with a general description of the value structure of the Midwest and continues, in the manner of Chinese boxes, from larger to smaller unit until it finally pinpoints the county. Tables are included illustrating economic and demographic characteristics of the area under study.

The story of the mobilization of the seven hundred people who actually carried the necessary health survey questionnaire is told in temporal sequence, first, for the whole county and, second, in great detail for one local unit. In effect we get a macroscopic and microscopic view. The narrative is broken from time to time while the authors develop categories and typologies for describing what has happened. Thus the process of getting the survey off the ground is divided into analytic sections—"initiation" and "legitimation"—and the relationship between these phases and the actors who appear in the drama throughout them is carefully described and analyzed. At the same time the interlocking personal and professional relationships among the actors are spelled out, and so are their many and various motivations and goals. The story is told in such minute detail that finally we can almost feel the hardness of the seats in the courthouse basement where at meeting after meeting the slow, patient process of "involving" the necessary people leads onward to the ultimate action.

If this book has a fault, it is that it falls between two stools; it is neither a scholarly treatise nor a handbook but a compromise between them. Although the authors do, when it appears useful, articulate their analyses with the work of others, they do not make a systematic effort to tell us where they feel their work to be theoretically cumulative. Although they refer to their theoretical framework as a "model," we are unsure of its qualities. Will it predict? Was it developed from previous research experience or from theory? Is it a system of *post hoc* generalizations derived from the many data they have collected?

Perhaps it is better to think of this book as a manual for those who wish to undertake the

heroic task of harnessing the multilithic forces of society into concerted democratic action. But there is a problem here, too. The book is written in the style of a dissertation, and the writing is ponderous throughout; at times, indeed, meaning goes down before pedantry. For example, speaking of two sections of the county which got away to a late start in the survey and therefore, presumably, ran a somewhat modified course, they say: "The latter two [communities] were contacted for over a year following the April 26 meeting, and thus must be *analyzed in terms of a different dimension of time.*" (My italics.)

In spite of this objection, this is a valuable book. It gives us a magnificently detailed account of an important social process and some useful empirical generalizations. Perhaps it is unfair to wish that the authors would give us a general statement of what they make of a society which runs its affairs in this manner.

ELAINE CUMMING

University of Chicago

The Order of Presentation in Persuasion. Edited by CARL I. HOVLAND. ("Yale Studies in Attitude and Communication," Vol. I.) New Haven, Conn.: Yale University Press, 1957. Pp. x+192. \$4.00.

Carl Hovland's name has been associated for almost a decade with experimental studies of communication. This latest volume brings together a series of experiments concerning the effects produced on opinion and attitudes by the sequence in which information is presented.

Like its predecessors, this new work has many admirable features. The experiments are performed with care and reported with caution. Constant attention is given to variables requiring control in future research. Each study builds on the others, with the result that one sees knowledge growing into an interrelated structure of propositions. Special efforts are made to measure the extent to which the independent variables were actually created in the laboratory, thus giving added significance to negative findings.

The selection of the problems under study also deserves commendation. Hovland and his colleagues begin with questions of interest to practitioner and scientist alike. In this volume, for example, they inquire about the conditions under which an audience is affected most by

the first argument it hears as against the argument presented to it most recently. Or, again, are people less affected by new information if they have already evaluated earlier knowledge on the same subject? In questions such as these the living strands of practice and theory are woven together into unusually rich and provocative patterns.

The following propositions summarize the principal findings:

1. When two sides of an issue are presented successively by different communicators, the side presented first does not necessarily have the advantage.
2. If, after hearing only one side of a controversial issue, a response is made which publicly indicates one's position on the issue, the effectiveness of a subsequent presentation of the second side of the issue is reduced, thus entailing a primary effect.
3. The mere act of stating one's opinion anonymously on a questionnaire after hearing only one side of an issue does not significantly reduce the effectiveness of the second side.
4. When contradictory information is presented in a single communication, by a single communicator, there is a pronounced tendency for those items presented first to dominate the impression received.
5. The primacy effect found in presenting contradictory information in the same communication was reduced by interpolating other activities between the two blocks of information and by warning the subjects against the fallibility of first impressions.
6. Presentation of information relevant to the satisfaction of needs after these needs have been aroused brings about greater acceptance than an order which presents the information first and the need-arousal second.
7. Order of presentation is a more significant factor in influencing opinion for subjects with relatively weak needs for understanding than for those with high needs for understanding previously unordered material.
8. Placing communications highly desirable to the recipient first, followed by those less desirable, produces more opinion change than the reverse order.
9. When an authoritative communicator plans to mention *pro* arguments and also non-salient *con* arguments, the *pro*-first order is superior to the *con*-first order.

Stated in common-sense language, these far from common-sense conclusions do not communicate the major contribution of the studies: that existing theories of behavior enable us to formulate and explain many important problems of interpersonal influence.

Hovland's program of research is not producing a theory of communication as such. It is developing important new contributions to our general understanding of behavior, showing that these account for some of the specifics of communication. Readers will want to make various qualifications about the methodology of certain experiments, the likely generality of the findings, or the practical application of these results. Such qualifications are important. They are less significant, however, if one keeps foremost the theoretical contribution of this program of research.

GUY E. SWANSON

University of Michigan

The Freedom To Read: Perspective and Program. By RICHARD McKEON, ROBERT K. MERTON, and WALTER GELLHORN. New York: R. R. Bowker Co. (for the National Book Committee), 1957. Pp. viii+110. \$2.50.

This study grew out of a conviction that censorship, having its roots in private morality and religion, on the one hand, and in social and political goals, on the other, needs to be re-examined in the light of present-day problems. It makes clear that, despite the comparatively successful struggle against the efforts of the Puritans to censor writers and against the efforts of ruling groups both here and in England to control the reading of the middle and lower classes, the problem is by no means solved. The premises underlying these early attempts at censorship are still with us: reading, or certain kinds of reading, may be harmful to the development—or salvation—of the individual, and it may constitute a threat to the status quo.

The book, small as it is, outlines a thoughtful and courageous strategy geared to advance the freedom to read. Three approaches are offered, appropriately submitted by a philosopher, a sociologist, and a jurist, respectively.

McKeon reviews the philosophic, political, moral, and social arguments used in discussions of censorship today and points out that, contrary to popular conception, censorship and freedom do not stand in unambiguous opposition to each other. By some, freedom is conceived as scope for doing what one ought to do; by others, as lack of restraint on doing

as one pleases. In the first view, freedom can be achieved only by the wise or good man (Plato, the Stoics, Augustine, Spinoza, Hegel). In the second view, a man is free only if he acts in the light of his own preferences (Dewey, Bentham, Mill, Maritain). McKeon reviews the arguments both for and against censorship based on both these conceptions of freedom, stating that people may share the idea of freedom and yet may differ concerning the advisability of censorship.

Contemporary problems of censorship become most acute during times of international tension, confusion, or general insecurity. While not part of the normal operation of political power in a democracy, censorship makes its appearance either in government action, as a questionable extension of political power, or in pressure for change of law. "The means of censorship . . . have tended . . . to advance uniformity at the expense of discrimination and conformity at the expense of freedom." McKeon is "convinced that morality is not advanced by restricting communications, but . . . is crippled by restrictions. . . . And as for our nation's security, we are satisfied that . . . free institutions are more likely to be preserved by freedom of expression than by political control."

Merton presents a three-pronged program of research: (1) studies on the effects of books upon personality and behavior; (2) research on the social psychology and economics of reading; and (3) studies on attempted and actual restraints upon freedom to read, with particular reference to the social consequences of censorship. Such a program would, among other facets, provide codification of existing knowledge about the psychological effects of reading, of organized and individual responses to censored books, of patterns of reading and taste, and of support and opposition to censorship on the community level.

One of the researches Merton advocates, namely, an exploration of the extent to which librarians avoid controversial materials in book selection "at the price of conformity to the anticipated demands of censorial groups," has in the meantime been undertaken at the University of California's School of Librarianship (Berkeley) under the directorship of Marjorie Fiske. This study is supported by a grant from the Fund for the Republic, which also sponsored the production of the book under review.

Gellhorn suggests some steps for immediate legal action. He proposes a review of the validity of all state and local censorship laws and the recasting of existing statutes, and he is particularly emphatic about the need for "an appraisal of government's role as consumer or channel of books" and for "the evaluation of Federal censorship through Postal and Customs administration." In addition, he makes a moving plea to voluntary organizations on all levels to work for the preservation of intellectual freedom.

The National Book Committee, as well as the authors, should be complimented for their ability to co-ordinate specialized professional skill in the interest of public policy.

LEO LOWENTHAL

*University of California
Berkeley*

Sociological Theory: A Book of Readings.

Edited by LEWIS A. COSER and BERNARD ROSENBERG. New York: Macmillan Co., 1957. Pp. xiii+578. \$6.50.

The problem of introducing sociology students to sociological theory and sociological theorists has been solved in several different ways. In the past the effort was made to acquaint students with the history of sociology or, more frequently, the history of social thought. The sheer mass of material and its occasionally dubious relevance to contemporary concerns tended to result in somewhat superficial treatment. More recently, attempts have been made to build introductory theory courses around theories of contemporary relevance. But here again the thoroughgoing treatment of schools of thought and individual theorists which such an orientation requires places too heavy a burden on the course and on the instructor. More recently still, introduction to theory has been attempted by instituting courses stressing systematic theory in which texts written by a single author are employed. While such an approach has the virtue of consistency and integration, the student is not introduced to the traditions of his discipline and often fails to make the acquaintance of the great figures of the past.

Professors Coser and Rosenberg have attempted another solution. Their book of readings—accompanied by perceptive introduc-

tions to each section and by other writing of their own—is built around fundamental concepts and their interrelations. Most of the great figures of the past and present are represented, but not as founders of schools or builders of systems. Rather, they appear as contributors of concepts which have found their way into the fundamental vocabulary and frame of reference of sociology. The concepts are in turn integrated by their ordering in the text according to the orientation of the functional analysis of social systems. Thus Part I is devoted to the general concepts of "culture," "interaction," "social control," "power and authority," and "cohesion and conflict." Part II follows with the readings centered on "definition of the situation" and "role-taking and reference group." Finally, Part III is devoted to the structural concepts of "primary group," "status," "class," "bureaucracy," and "anomie," with a final chapter devoted to "structure and function." This mode of organization is surprisingly successful, showing clearly that there is a fundamental perspective present in modern sociology and that the thinking of certain men who, in some cases, would have been horrified to be included in such a perspective can be integrated within it.

The problems that such a perspective creates are less obvious but can nevertheless be found by the instructor who searches for them. The inclusion of Mead's "I" concept in the section on social control raises all the questions concerning the degree of the conditioning of personality by culture and social structure and the possibility of the transcendence of the self over the structures of society. The problem of the role of ideas and of religious orientations is raised in the section on social control and in a postscript chapter on the sociology of knowledge. So also is the question of the origin of value systems and of institutional structure. The presentation of Davis and Moore's analysis of the functions of social stratification side by side with Tumin's critique of their analysis raises as sharply as possible the issue of the relationship between an egalitarian ideology and a stratified social structure.

Taken in its totality, and remembering its purpose—to introduce the student to the realm of sociological theory—this we find an impressive work. It is so impressive, in fact, that one might wish that a book like this might be the basis of a one-semester introductory soci-

ology course for Sophomores, to be followed by a semester devoted to methods of research. After all, an intelligent introduction to sociological theory is the proper introduction to sociology. Must such an introduction now and forevermore be beyond the grasp of our Sophomores?

WILLIAM L. KOLB

Tulane University

Jail Administration. By MYRL E. ALEXANDER. Springfield, Ill.: Charles C Thomas, 1957. Pp. xvi+326. \$6.75.

The jails of the United States are the most neglected element in the penal system. They have been aptly described by a former inmate as "the unmentionable cloacal region of the community." More than a million persons pass through our jails annually, and those who are not caught up in the treadmill of recidivism (as more than 50 per cent are) come out the worse for the experience. Caught in the mesh of local, short-term, political patronage systems, and out of the sight of an apathetic public, jails will continue to blight the community through their influence on the lives of those unfortunate enough to pass through them.

Mr. Alexander is fully aware of these conditions, but he is by no means a muckraker. His many years of experience as the assistant director of the Federal Bureau of Prisons and with the Jail Inspection Service of the Bureau has led him to the more realistic view of "accentuating the positive." This monograph is eminently practical: as a handbook for jail employees, as a training manual, and as a source of insight to the civically interested layman. It lays stress, by precept and example, on precisely those elementary aspects of handling human beings in custody that every uninformed citizen assumes to be "normal" practice. By pointing to the rational procedures of the few jails that have been emancipated from the Neanderthal tradition, it is hoped that some jail administrators may be encouraged to emulate such radical practices as separating the convicted from the unconvicted, providing work for idle inmates or even (heaven forbid!) go so far as to feed jail inmates three meals a day. Such precepts and examples are sorely needed despite the difficulty of translating them into Parsonian

conceptual semantics for the benefit of sociology students.

One major criticism: underpaid jail employees and students will not be able to afford the book. Members of local political bodies who can afford the book, and who control the fortunes of jails, will probably not read this book out of a suspect deference for maintaining the status quo.

HANS W. MATTICK

Chicago, Illinois

Role Relations in the Mental Health Professions. By ALVIN ZANDER, ARTHUR R. COHEN, and EZRA STOTLAND. Ann Arbor, Mich.: Research Center for Group Dynamics, Institute for Social Research, 1957. Pp. vii+211. \$4.50.

A fascinating case for the study of interprofessional relationships has been subjected to study by these staff members of the Research Center for Group Dynamics. Their data are derived from personally administered interview schedules obtained from over a hundred and fifty each of psychiatrists, clinical psychologists, and psychiatric social workers. The general purpose of the study was to test a number of hypotheses regarding the nature of the relationships between persons who perceive their "power" to be high and those who perceive their "power" to be low in each of the three professions with those in the other two groups and with their own colleagues.

Psychiatrists and psychiatric social workers "are clear concerning their position in relation to each other, have accepted this placement as proper, and hold attitudes which maximize the possibility that the two groups will supplement each other well" (p. 42). On the other hand, the professions of psychiatry and clinical psychology "are not in accord concerning their interdependence. . . . There are attitudes present which are indicative of potential problems in the relations between the two groups" (p. 67). As for relations between the two "ancillary" professions, "Mild cordiality is the social worker's general feeling toward psychologists . . . the over-all tenor is [close] to neutrality . . ." (p. 101). "The large majority of psychologists are highly pleased concerning relations with social workers" (p. 109). "In general, the roles of these two professions are not well integrated . . . [they] appear to view each other across a

gulf created by the lack of any strong need for professional collaboration between them" (p. 113). "The results show that both social workers and psychologists tend to look past each other to psychiatrists as the more authoritative persons" (p. 123).

The most severe limitations of this study are technical in nature. The most consistently employed technique is the simple correlation, which, in terms of the size of their sample, becomes statistically significant at .18 ($P = 0.05$). Although they concede (1) that much of the variance is left unexplained by such a correlation coefficient (about 97 per cent of the variance is left unexplained by a coefficient of .18 whether or not it is "significant") and (2) that correlation does not indicate causation, they proceed to employ such coefficients as their major evidence and to assume a cause-and-effect relationship between any two variables in which a significant coefficient is obtained. It is not clear why the analysts chose so crude a technique when they obviously had sufficient data in sufficient quantities to use some method which would have indicated differential sources of variance or isolated individual variables from the effects of other and perhaps contingent ones. Any one of such techniques as partial correlation, factor analysis, or the analysis of variance would have provided a much cleaner and certainly more convincing picture. It is difficult to defend conclusions based on the relationship between one independent variable and one dependent one while all others are ignored (not controlled as would have been the case had partial correlations been employed). The reader may also wonder why the authors occasionally shift from correlation to chi square in comparing two variables when the necessary data for correlations are present. This reviewer would have preferred, assuming that the analysts had neither the time nor the resources to employ more sophisticated statistical techniques, that they stick with the simple chi square consistently and dispense with their masses of simple correlations. At least, once the criteria for classification are made clear, the chi square is easily interpreted, and its limitations are clear for all to see. In brief, the endless reporting of interminable correlation coefficients, many of them unimportant (although all are statistically significant), in conjunction with a bare minimum of interpretation and little or no creative or imaginative analytical comment (the repeated summarizing of

statistical findings is hardly a substitute) results in increasing monotony for the reader as the pages go by.

Further technical difficulties are suggested by the high rate of refusals from psychiatrists (23 per cent) because in nearly half the cases they were not interested in the study. Under different conditions with a different kind of problem this might be unimportant, but, insofar as this was not the primary reason for refusals from psychologists and social workers and insofar as refusal for lack of interest is related to the central problem of the study, it may well have introduced a serious bias in this case. In other words, it can be suspected that, in studying the relationship between psychiatry and other professions, those psychiatrists who "are not interested" represent an important block of opinion which has been omitted in this study. It may also be suggested that the variables of age (the psychiatrists averaged ten years older than the other two groups) and sex (6 per cent of the social workers were males as contrasted to 96 per cent of the psychiatrists) are extremely important in determining the quality of human relationships and that they were almost completely ignored in this study, the one exception being the comparisons made between male and female psychologists, which, incidentally, indicated that sex is an important variable.

Being neither psychiatrist, nor clinical psychologist, nor psychiatric social worker, this reviewer is not in a position to judge the value of this volume to persons engaged in those professions. The question to be properly entertained in a review in a sociological journal is: "Of what value, if any, is the volume from a sociological perspective?" Within that context it may be of interest to sociologists at three different levels of abstraction. At the lowest level the book contains information which would be useful for the medical sociologist who is unfamiliar with the interprofessional jealousies and competitions which exist on the mental health team and who finds it necessary to work in a setting in which such a team is operating.

At a middle level of abstraction the book implicitly suggests numerous hypotheses which are worthy of the attention of those engaged in the sociology of occupations and professions. It illustrates the need for research on interprofessional relationships, a stage not yet reached by most students of occupations who have largely focused on single occupations in isolation from other occupational groups. For ex-

ample, this study indicates that the aspirations of psychiatric social workers are toward the role of being better assistants to the psychiatrist. They see themselves as having no unique professional skills to offer; the more psychiatry they understand, the better they are able to assist the psychiatrist, and the better psychiatric social workers they are. When this is tied in with the reviewer's impression that, of all the specialty fields in social work, psychiatric social work has the highest prestige value among students, we are confronted with the curious dilemma of an occupation generally striving for professionalization, yet with its most prestigious group seeking to be a dependent assisting occupation rather than an autonomous professional group able to exercise independent judgment on the basis of an exclusive body of knowledge and to make independent decisions regarding the client.

At the highest level of abstraction the book can stimulate the growth of general sociological theory. The authors have pointed to ways in which the concept of social role, in the sense that it has been employed by Cooley and Mead, can be used as an operational tool in empirical research. They do not use "role" either in a play-acting sense or simply as a bundle of tasks. They have devised questions which can serve as cues to the whole complex of obligations, aspirations, self-conceptions, and relationships with others, all of which are involved in a social role. Their specific attempt does not, in the opinion of this reviewer, come off well, but it does facilitate the efforts of others who are interested in proceeding in this direction. Finally, it is impossible for a sociologist to read this volume without wishing that the data had been analyzed within the framework of Simmel's theories of the triad. These three professional groups, one in a clearly dominant power position, and the other two striving for acceptance (in very different ways), illustrate clearly some of the forms which Simmel suggests the triad may take. Perhaps most interesting is the role of the social worker as the *tertius gaudens* and the implications such a role has for the psychologists. Those sociologists with a Simmel-like bent will certainly find this volume an informative and suggestive piece of data.

IRWIN DEUTSCHER

Community Studies, Inc.
Kansas City, Missouri

Closed Ranks: An Experiment in Mental Health Education. By ELAINE and JOHN CUMMING. Cambridge, Mass.: Harvard University Press (for the Commonwealth Fund), 1957. Pp. xx+192. \$3.50.

During 1951 Elaine and John Cumming carried out a project designed to understand and change the attitudes toward mental illness of a small Canadian community. By and large their program did not succeed; questionnaire data obtained before and six months after the inception of the project revealed essentially similar patterns of attitudes about mental health and illness. As a matter of fact, there was strong evidence that the program had aroused considerable tension and antagonism.

The description of "Blackfoot's" closing its ranks against this variety of mental health education reads quite easily. The last part is an attempt to provide a comprehensive answer as to why the project failed to achieve its anticipated goals. In this latter section the authors critically review their assumptions and methods, and their subsequent papers demonstrate that they have learned a great deal from the "Blackfoot" experience. Interestingly, the later studies were carried out in a mental hospital where previous attempts to alter stereotypic prejudicial attitudes of nursing personnel against patients had failed. In their later program the Cummings effectively used "norm-bearers" as models for a more humanistic, accepting approach to patients by placing the personnel in roles which facilitated their motivation for change.

The authors should be congratulated for publishing a project which failed to achieve one of its major aims; the literature has been saturated with "successes" which do not stand the test of time. The most provocative questions raised by the book deal with the authors' explanation for this failure. The Cummings indicate that they were naïvely optimistic in expecting to produce altered attitudes within a test period of six months. Initial subtle resistances in the population were glossed over. No concomitant service function was present as an integral part of the program, and the project remained relatively external to the flow of events within the community until it aroused antagonism. Initial assumptions about popular concepts of mental illness, which did not benefit from Shirley Star's later data, proved to be erroneous. The authors

explore the basis for the tenacity against change of attitudes by utilizing Merton's model for tracing the intended and the unintended consequences of social action. Ultimately, they hypothesize that the "solidarity of the sane" is a key motive for isolating mental patients geographically, socially, and psychologically, since by this isolation the solidarity of the social system is upheld with the non-violation of predictable behavior norms. Others have reached similar conclusions in the past, but most often this has been based on armchair reasoning, without supporting field data.

Nevertheless, it is questionable whether such a hypothesis is entirely a logical derivative of the reported findings. It assumes a degree of homogeneity of attitudes beyond those actually observed. For example, when the respondents were classified in subgroups, it was found that those who view the causes of mental illness in social or economic models consider themselves more responsible for the care of the mentally ill than do those who tend to view causality in biological terms. How widespread is the former viewpoint? Does it represent a significant latent tendency toward change in attitude not fully explored by methods used in this program? Might some communities be more amenable to changes in attitude toward mental health, just as variability of community response has been apparent with changes in other attitudinal areas?

The authors, of course, are aware of these possibilities, but it appears to this reviewer that their theoretical position leads them to an overestimation of the current functional need for isolation of the mentally ill prior to large-scale investigations in many communities by different groups of investigators. The latter point is also discussed by the Cummings, but perhaps the need for independent, objective evaluation of the specific transactions between investigators and community is underemphasized. Were subtle personal difficulties partially responsible for the failure? Is the issue one of core values so deeply ingrained that they cannot be approached directly but rather must be handled through peripheral attitudes, no matter who conducts the study? These questions can be answered only by multiple transactions between different sets of research workers and communities. In this manner the hypotheses formulated by the authors will be put to an adequate test. Even before this time, however,

mental health educators both in the mental hospital and in the community at large will find much in the way of practical suggestions and theoretical stimulation from this excellent pioneering study.

MELVIN SABSHIN, M.D.

Michael Reese Hospital
Chicago, Illinois

The Church's Understanding of Itself. By R. H. T. THOMPSON. London: SCM Press, 1957. Pp. 110. 8s. 6d.

This is an ambitious title for a short report on 352 interviews with members of four church parishes in Birmingham, England. The interview schedule contains nineteen questions concerning religious beliefs and practices, as well as attitudes toward the clergy and fellow parishioners. Each of the four parishes, carrying the pseudonyms of the Evangelists, is discussed separately in about a dozen pages.

A central problem of research in this work, and a focus of future research, is the varied "churchmanship" of these four parishes. All belong to the diocese of Birmingham, but in religious ideology they range from St. Mark's, the "most Catholic," to St. Matthew's, the "most Protestant." In between are St. Luke's and St. John's, the "medium high." The problem is to fix the norms of expected religious behavior against which the findings of the study can be measured.

The conclusions are offered quite tentatively, but most of them will sound familiar to social scientists who have conducted research in American urban churches. The parish is not a meaningful social unit; religion largely fails as an integrating factor; the values of members are drawn more from secular than from religious groups; the pastoral role of the clergy is ambiguous; and social compatibility has greater appeal than doctrinal conviction. It must be noted that this is an opinion survey and not a detailed group study, but it is likely that the latter approach would simply have confirmed the findings of the survey.

JOSEPH H. FICHTER

Loyola University

Religion, Society and the Individual: An Introduction to the Sociology of Religion. By J. MILTON YINGER. New York: Macmillan Co., 1957. Pp. xvi+655. \$6.75.

This book is a competent attempt—the first in thirteen years—to supply sociologists with a comprehensive formal introduction to the sociology of religion suitable for use as a course textbook. The fact that it aspires also to give a lead to professional theorizing and research in the field interferes somewhat with its utility as a text because of the difficulty involved in addressing two levels of students simultaneously; but the style is forthright, lively, and readable, and the analogies in the opening and closing paragraphs of Part I are exceedingly vivid and apt. The book is divided into two parts—eleven chapters written by Professor Yinger and a selection of thirty-nine readings briefly annotated and supplemented by an extensive bibliography.

There is much evidence of fresh, original thinking here, but some of the basic assumptions on which the book rests need to be examined rather closely. The first of these emerge on pages 5-9, where the author argues (with appropriate tentativeness) that religion can be defined satisfactorily solely with reference to the functions it performs (i.e., “as a system of beliefs and practices by means of which a group of people struggles with . . . ultimate problems of human life”). By implication, the author sees little value in the classic conception of Durkheim and others who see religion as always relating to “that which is sacred” in, or to, the society, and thus having at least some substantive, though not necessarily culture-bound, content. If we define religion solely in functional terms, what happens if a variety of institutions (organized philosophical inquiry, for example) turn out to be performing the same functions? One wonders, further, whether function, at least where social phenomena are concerned, is well enough understood as yet to risk resting any of our working definitions squarely or exclusively upon it.

Yinger is at some pains to make clear (pp. 73 ff.) that he does not regard religion as necessarily “good” for personality or for society and that to do so is a persistent error resulting from implicit value-judgments which “objective study” in functional terms would eliminate. He does not, however, here provide us with the systematic, logically tight, theoretic specification of functions which may be

the only basis on which firm and steady “functional” inquiry into the phenomenon, with all values or biases “controlled,” can be pursued.

Although Yinger criticizes Gordon Allport for “too strong” assertion of a “need determination” of religious concepts (p. 74 n.), he himself asserts that “religion is in man; it is to be understood by the analysis of his needs, tendencies and potentialities” (p. 309). In juxtaposition to this statement is the explicit assertion that sociology inevitably will—in fact, must—take a “naturalistic view” of religion. I interpret this to mean that religious beliefs and practices are assumed to be creations, emanations, or projections out of the minds and acts of men, more or less unconsciously created by men to meet needs laid down for them by nature, and that they can in no case be regarded as having any validity in any other sense. Granted that science must stay close to the empirically verifiable and refrain from making statements about that which cannot be empirically verified (or readily and commonly inferred by logical steps from sensory experience by “competent others”), is it *necessary* to deny flatly that religious beliefs cannot possibly be thought of by social scientists as relating to anything that *might* have some “reality,” independent of human needs? Under some circumstances, that is, for the sake of some theoretic purposes, one might see (and agree upon) the necessity for taking this position, but Yinger does not seem to have made the case for it here.

Space does not permit comment on the many provocative hypotheses advanced by the author (e.g., on pp. 242, 243, 262, 263) or on his rather heavy reliance upon what seems increasingly to be a fruitless and conceptually unreliable “church-sect” typology. Fuller use of empirical studies might have been made advantageously in the body of the text; but the book, as it stands, is clearly a challenging piece of work.

ALLAN W. EISTER

Wellesley College

Sociology. By JOSEPH H. FICHTER. Chicago: University of Chicago Press, 1957. Pp. xiii+450. \$5.00.

Introductory textbooks vary according to their authors' conceptions of sociology as a field of study. Some approach the field as

systematizers. They outline sociology through a set of interlocking concepts, elaborated, as far as feasible, in a continuous fashion.

Fichter's textbook is of this type. Its aim is to equip the student with a well-differentiated conceptual apparatus which he may readily apply to his own experience. I can think of no introductory work in which logic has been applied more successfully and with less tendency to formalism. The basic concepts of the field are introduced and defined in a progression from the simpler to the more complex types. Discussion of each concept is followed by a descriptive chapter in which some area of the American scene serves to illustrate—and illustrate well—the category introduced. The author deliberately avoids loading the text with material which may divert the reader's attention from the continuity of the presentation. He dispenses with methodological references; there are no charts, tables, or illustrations. Yet these are omissions by design and not by default. The author's overriding purpose—to make the student sociologically articulate and conversant with the key concepts of the field—is most successfully accomplished. An easy-flowing English, free from redundancies and unexplained technical terms, helps to make this book a desirable guide to the beginning student. Over and above that, the author has made an original contribution to the semantics of sociology.

ERNEST MANHEIM

University of Kansas City

The Social Order: An Introduction to Sociology. By ROBERT BIERSTEDT. New York: McGraw-Hill Book Co., 1957. Pp. viii+577. \$6.00.

Professor Bierstedt has written an eminently readable, clear, and lively textbook. He has striven obstinately to adhere to a consistent set of central ideas. The book sets forth vividly the boundaries between sociology and two of its neighbors, anthropology and social psychology. Indeed, one might hazard the opinion that students from these two fields will find this precisely the kind of book that will help them distinguish among the subject matter of three often confused disciplines.

The author owes a heavy debt to R. M. MacIver, but the book is not merely a dis-

ciple's exegetic exercise. Where MacIver would be philosophical or inclined to concentrate his attention on problems of government, Bierstedt is mindful of the empirical nature of sociology and respectful of the pioneers whose formulations so often are ignored by the writers of introductory texts.

In other respects, too, this book differs from the general run of recent texts. By emphasizing the phenomenon of order in society, the author has rigorously pruned from his book many of the topics which have saddled sociology with the label "the science of the leftovers." Such terms as "social disorganization," "crowds," and "public" are omitted from discussion. There is no treatment of human ecology, and almost none of population. Statistical tables are almost completely lacking. A notable exclusion concerns the materials dealing with the process of socialization. In delineating the field of sociology, the author has allocated all such materials to social psychology; he has omitted from discussion such matters as communication and the development of the self in group life. By assigning the consideration of role to the field of social psychology, the author is able to devote a much fuller treatment than is usual to the topic of status, which is pivotal to his notions of social order.

Although this is predominantly a discussion of the social order, with only passing references to the cultural order or to the person, the author has included a chapter on the problems of social change. In this section the hand of MacIver can be readily detected. The treatment emphasizes the area of philosophy of history, with particular reference to Toynbee and Spengler. The chapter ignores the rich literature dealing with revolution and reform, with fashion and fad, sects and social movements, and the other varied and lively materials usually encompassed under the title "collective behavior."

In a more critical vein one might have anticipated in a discussion of the social order something more in the way of analysis of the work world. The book omits almost entirely any consideration of the ideas that have flowed from Max Weber concerning such things as the occupation and vocation of the individual and the corporate bodies that carry on some form of continuous activity in modern society. Wherever the latter are discussed, the author uses the distinctions made by MacIver be-

tween institutions and associations. The former are defined in cultural terms as patterns of behavior; associations connote organized groups. This choice of concepts has two unfortunate consequences. In the first place, the author appears to view associations as intentionally organized entities (p. 280), thus ignoring the distinctions between the forms that have been enacted and those of creative and/or of non-rational origin. The choice of terms has the further effect of lumping together the kinds of groups that carry on a continuous sort of activity and the sorts that meet or function in highly sporadic fashion. Short-lived bodies such as special committees, and still more evanescent entities such as conferences, are placed in the same category as long-persisting organizations, Yale University, for instance (p. 468). The author rightly calls attention to the confusion surrounding the terms "institution" and "association"; but his discussion of this area is not so clear as his analysis of social status.

Despite these reservations the book has very solid virtues. It stays close to a central focus of what belongs in an introductory course; it conveys a substantial body of factual material; the style is pleasantly scholarly; and, especially in those sections where the author has departed from his admired mentors, the presentation is conspicuously fresh and vigorous.

OSWALD HALL

University of Toronto

The Administrative State: An Introduction to Bureaucracy. By FRITZ MORSTEIN MARX. Chicago: University of Chicago Press, 1957. Pp. x+202. \$4.00.

Fritz Morstein Marx's aim, which I take to be the detailing of the role and dilemmas of administrative action in the political process, is commendable. Notwithstanding the scope of the work, indeed partly because of it, Marx's execution of his aim leaves much to be desired.

In his Foreword to the volume Roy C. Macridis says that the author "combines the best traits of sociology and political science while avoiding their characteristic faults." My reading suggests just the opposite.

Macridis takes the characteristic fault of sociological analyses of bureaucracies to be

an emphasis on "'structure' and 'role' at a very high (and sometimes vague) level of abstraction." If this means that sociologists sometimes or often fail in their pursuit of patterns of action and affect to specify what men *do* and *feel*, then, although I agree with the criticism, I suggest that I know of no sociological work on bureaucracy as subject to this criticism as the one under review. In 187 pages Marx manages to tell us almost nothing about the activities of government administrators. He reserves 3 pages out of the 187 for discussion of "The Day-by-Day Grind." Although he does refer among others, to the British, French, Prussian, Chinese, and United States administrative systems in his comparative analysis, I recommend the account of the development of the Chinese bureaucracy, entitled "The Living Example," as an instance of description so condensed and abstract as to be lifeless.

Macridis lists the characteristic faults of political scientists: a penchant for "exhaustive descriptions of institutional forms" and concern "with broad value judgments, notably the relation of bureaucracy to democratic principles." Now Marx's treatment of institutional forms—and this is his subject—is not exhaustive, just tedious. Even Max Weber, who could be most exhausting at times, never commented on the structure of organization in a drier fashion. Just one example of the way in which an order is passed down and executed or subverted would have brought welcome relief. And, as for concern with "the relation of bureaucracy to democratic principles"—Macridis does not renounce concern with this, it should be noted—*every* chapter seems to me worked about this concern, although only the last chapter—"Perspectives" deals explicitly with it. I do not mention this as a fault—I do not think it is—but rather as a suggestion that others may find more to recommend this book than I have been able to discover. Other readings are apparently possible.

SHELDON L. MESSINGER

Berkeley, California

Parkinson's Law and Other Studies in Administration. By C. NORTHCOTE PARKINSON. Boston: Houghton Mifflin Co., 1957. Pp. xi+113. \$3.00.

In recent years the sociology of bureaucracy has attracted the most powerful intelligences of the profession. Had Professor Parkinson of the University of Malaya confined himself to empirical research, this book would have won him a respected place in the field. Had he even, as a theorist, remained content with elaborating a logically exhaustive categorization, all might yet have been well. But, as the case stands, it may perhaps *not* be inappropriate to bring to the student's attention that his methodology lacks sophistication. Not only does he *have* variables and specifies that one is a function of another but he specifies *what* function it is.

This heuristic device leads him, first, to Parkinson's Law, which in the short, or vulgar, form states that "work expands so as to fill the time available for its completion," but which is itself expandable into the mathematical model

$$x = \frac{2k^m + l}{n}, \quad (1)$$

where x is the number of new staff required each year.

This law, like Stouffer's Law, the only other known law in sociology, receives impressive empirical support, especially in the finding that the number of administrative staff in the

British Admiralty is an inverse function of the number of ships.

The same is true of Parkinson's other formulations such as: the amount of time spent on an agenda item is a monotonically decreasing function of its importance; or (for Parkinson has created the sociology of geometry) the more important is a guest at a cocktail party, the nearer he will be found to the center of square E/7 at time $H + 90$. (The coordinate system receives explication in the text.)

Yet it is *precisely* in the strength of support that he receives from the data that lies the weakness of Parkinson as a theorist. His findings, like Newton's, must be assigned the status of a congeries of mere empirical generalizations, for he fails to substantiate their unique derivability from a higher-order paradigm in the frame-of-reference frame of reference. In brief, he has a scheme but not a schema.

With all its shortcomings as theory, sociologists will nevertheless find this book readily adaptable, and adoptable, as an eminently teachable introductory text in its field. For students at the University of Chicago, ecological diagrams are supplied.

GEORGE C. HOMANS

Harvard University

BOOK NOTES

Leader Attitudes and Group Effectiveness. By FRED E. FIEDLER. Urbana, Ill.: University of Illinois Press, 1958. Pp. 69. \$1.75.

In less than fifty pages of text, Fiedler describes and summarizes the findings of the University of Illinois research on "Social Perception and Group Effectiveness," a project involving some twenty-one researchers in the attempt to capture and domesticate a variable called "ASo." ASo (assumed similarity between opposites) is operationally defined as the similarity between a subject's description of his "most preferred" and "least preferred" associates. On the basis of some ingenious geometrizing and the pattern of correlations, ASo is claimed as a measure of generalized social distance between the subject and his associates. After a report on the technical details of measurement, Fiedler describes ASo's efficacy in predicting the productivity of high-school basketball teams, bomber crews, ROTC cadets, open-hearth steel crews, and farmer supply co-operatives. The end result, which follows from a complex set of findings on the relations among ASo, sociometric structure, and productivity is this: "In order to predict a group's productivity, we find that the leader must have two attributes. . . . [He] must be acceptable to his followers . . . [and he] must maintain a certain amount of psychological distance from his men." Sociological readers will find the report both substantively important and an excellent case study in sequential processes of research. Those who are interested in further detail must be warned that this monograph is a brief summary of thirteen technical reports and eleven journal articles and, although lucidly written, occasionally verges on the laconic.

Psychological Development through the Life Span. By SIDNEY L. PRESSEY and RAYMOND G. KUHNEN. New York: Harper & Bros., 1957. Pp. 654.

This sprawling revision of the authors' *Life: A Psychological Survey* (1939) combines a textbook on educational psychology, comments on educational and social policy, and an encyclopedic review of the literature on age correlates from birth to senility. Although the authors fail in the impossible task of integrating their voluminous materials (there are 1,059

studies officially cited and dozens more mentioned in the text), sociological readers will find the volume a treasure trove of annotated research on age differences in variables ranging from "happiness" to rate of block sorting. There is a special emphasis on social variables and a sixty-page chapter on age differences in patterns of social interaction.

The Child within the Group: An Experiment in Self-government. By MARION E. TURNER. Stanford, Calif.: Stanford University Press, 1957. Pp. 93. \$3.00.

In 1920 the author of this little volume headed a small private school on the West Coast. During its last three years she gradually introduced a program of self-government for the group, whose ages ranged from four to six and a half. Eventually, the youngsters developed a formal code of rules (e.g., "Every child in the school has an equal right to the traveling rings and may use them as he wishes") and a formalized trial-by-jury procedure for settling infractions thereof. The published account is devoted to literal transcripts (RALPH: "We were just banging swords"; EVERETT: "But our rule said 'under no circumstances'") of the "trials," a few summary tables of frequency of "offenses" (hitting was the most common) and offenders (Lewis was responsible for 45 per cent of the offenses), and vignettes of nine children. While lack of data on the selection of the children and on "controls" prevents the drawing of any firm conclusions, the protocol evidence and the author's keen insight into juvenile group dynamics make the volume of more than usual interest.

Psychology in the Soviet Union. Edited by BRIAN SIMON. Stanford, Calif.: Stanford University Press, 1957. Pp. 305.

In April, 1955, the editor was a member of a group of teachers and educationists who visited the Soviet Union to study experimental schools. He arranged with Soviet psychologists to make available to English readers papers on current psychology in Russia. Of thirty papers received, twenty are translated and reprinted in this volume and cover such topics as "Higher Nervous Activity and the Problem of Perception," "The Theory of Memory," "Psycho-

pathological Research in the U.S.S.R.," and "Child Psychology." The sociological reader will probably be impressed, not by the presence of Pavlovian approaches, but by the total absence of anything even resembling Western social psychology and concepts of motivation. The potential for these areas may perhaps be judged by the following report on the Fourteenth International Congress of Psychology: "It must be said, however, that some reports were presented to the Congress, which had no scientific significance whatever. Among these were, first, many papers dealing with so-called social psychology. In this field, instead of genuine investigation, superficial inquiries, principally by means of questionnaires, are widely used."

The Fabric of Society: An Introduction to the Social Sciences. By RALPH ROSS and ERNEST VAN DEN HAAG. New York: Harcourt, Brace & Co., 1957. Pp. xv+777.

This volume has the twin distinctions, probably not unrelated, of being one of the few elementary texts in social science boasting an extract by T. S. Eliot and a favorable review in the *New Yorker*. Except for a frankly Freudian conception of personality and a special emphasis on art and popular culture, the conceptual framework is standard and eclectic (the four major sections are "Persons, Groups, and Culture," "Science and Symbols," "Economic Aspects of Society," and "The Organization of Power"). The style, however, is far from textbookish, the flavor perhaps being given by the following consecutive pairs in the Index: Raymond Bauer and Simone de Beauvoir; Bernard Berelson and Bernard Berenson; Gilbert Murray and Henry Murray; George Bernard Shaw and Paul Sheatsley; Edmund Wilson and Logan Wilson.

Social Class, Maternal Health and Child Care.

By WALTER E. BOEK, EDWIN D. LAWSON, ALFRED YANKAUER, and MARVIN B. SUSSMAN. Albany, N.Y.: New York State Department of Health, 1957. Pp. xii+138.

This monograph reports the findings of a 1955 survey of 1,803 upstate New York mothers, sampled from birth registrations in such a fashion as to guarantee that each had a three-month-old baby at the time of the interview. Since the interviewers were students in fifteen upstate New York colleges which volunteered

to co-operate with the project, the universe was limited to purposively selected geographical areas near the participating colleges.

The published report has no underlying analytical theme but presents several types of findings of interest to sociologists. Alternative measures of social status are compared (Warner's index, Hollingshead's index, Centers' items, income, interviewers' ratings, etc.) with results consistent with other recent studies (Warner and Hollingshead seem to do about the best). In addition, a number of cross-tabulations of social class and aspects of child-rearing and health practices are reported. The child-rearing data are somewhat more congruent with the Sears Boston area findings than with the Davis and Havighurst Chicago data. The health practice materials show the usual class disparities (83 per cent of the "Class I and II" children had diphtheria, whooping cough, and tetanus injections as contrasted with 51 per cent of the Class V children), although differences among the top four classes in the five class distributions are often minimal.

Economic Models: An Exposition. By E. F. BEACH. New York: John Wiley & Sons, 1957. Pp. 277.

This volume is designed as an introduction into formalized models in economics for the non-mathematically trained reader. The author considers both the mathematization, primarily through calculus, of economic conceptions such as supply and demand and the newer "econometric" models based on probability and regression. While the probability of successfully steering the naïve reader through calculus and elementary statistics in 203 pages is somewhat low, the exposition is extremely lucid and should enable the reader with minimal background to make considerable progress in understanding the general aims of the currently flourishing, model-making fraternity in economics. The author also includes some brief but important comments on the empirical accomplishments in this field.

Motivation and Market Behavior. Edited by ROBERT FERBER and HUGH G. WALES. Homewood, Ill.: Richard D. Irwin, Inc., 1958. Pp. 437.

The editors of this volume have gathered together, under the sponsorship of the Amer-

ican Marketing Association, a symposium on the currently controversial area of "motivation research." The volume includes brief statements of their positions by leading proponents and opponents in the field, including Ernest Dichter, James M. Vicary, Pierre Martineau, and Alfred Politz; summary essays on particular methods; and examples of specific researches of varying levels of "depth." The editors take no sides but the general tenor of the field can be inferred from the fact that their book classifies research techniques as "Projective and Psychological," "Nonprojective Survey," and "Other."

The American Teenager. H. H. REMMERS and D. H. RADLER. Indianapolis and New York: Bobbs-Merrill Co., 1957. Pp. 267. \$3.75.

A popularization ("Even so, most of young Remmers' colleagues scoffed. Why try to measure attitudes? They're here today and gone tomorrow!") by a free-lance writer (Radler) of the findings on high-school students of the Purdue Opinion Panel. The materials consist essentially of marginals (e.g., 41 per cent of the sample agreed to the statement, "Sending letters and telegrams to congressmen has little influence upon legislators.") for questions on personal problems, relations with parents, attitudes toward school, and political beliefs. Those who believe in the validity of marginals of attitude items may share the authors' concern at such findings as "15 per cent would refuse some criminals the right to have a lawyer," or "only 45 per cent believe that newspapers should be allowed to print anything they want except military secrets," or "33 per cent say that people who refuse to testify against themselves should be made to talk or should be severely punished." For most of the items tables are presented giv-

ing breakdowns by sex, grade, region, rural-urban residence, religion, and income.

Social Science in Public Relations. By REX F. HARLOW. New York: Harper & Bros., 1957. Pp. 203. \$3.50.

Harlow has attempted to cull from the writings of professional social scientists generalizations which will be of use to practitioners of public relations. His emphasis is largely on the fields of attitude and opinion change and to a lesser extent on studies of leadership and executive selection. If the suggestions are sometimes contradictory ("The public relations man should work indirectly rather than attack . . . broadside" versus "the appeal must be direct and emotional") and vague ("It is always well to take into account the setting of the members of the audience; environmental influences are powerful"), the blame can hardly be put on the author or on the field of public relations, since his conclusions seem to be quite reasonable inferences from the standard research literature in the fields he covers.

The Frontiers of Social Science. Edited by BALJIT SINGH. London: Macmillan & Co., Ltd., 1957. Pp. xi+519. 45s.

Thirty-eight short pieces are collected here in honor of Radhakamal Mukerjee. In addition to two essays by Mukerjee himself and six articles on Indian economics and sociology, there are sections on the integration of social science, values, social physics, regional sociology, ecology, population, institutional economics, mysticism, and art. Among the contributors are Talcott Parsons, Gardner Murphy, Stuart C. Dodd, Emory S. Bogardus, F. Stuart Chapin, Carle C. Zimmerman, J. O. Hertzler, James A. Quinn, Leopold von Wiese, Ernest W. Burgess, Rupert Vance, Pitrim A. Sorokin, Herbert Read, and William Ernest Hocking.

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- BERNSTEIN, MARVER H. *The Politics of Israel: The First Decade of Statehood*. Princeton, N.J.: Princeton University Press, 1957. Pp. xiv+360. \$6.00. History and description of the development of Israeli political and governmental institutions.
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- BLANSHARD, PAUL. *American Freedom and Catholic Power, 1958*. Boston: Beacon Press, 1958. Pp. xii+402. \$3.95. A new edition.
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